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THE NUTRITIONAL SIGNIFICANCE OF MILK WITH SPECIAL REFERENCE TO MILK SUGAR

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Thou shalt have milk for thy food, for the food of thy household, and for the food of thy maidens — King Solomon

THE prohibition against producing patent and related refined flours (100 lbs. wheat yielding from 58 to 72 lbs. flour) in favor of 80 percent flour (100 lbs. wheat yielding 80 lbs. flour) brings home the fact that famine is really stalking the earth and that food is scarce even in this rich country. Under conditions of food scarcity the more efficiently produced foods tend to replace the less efficiently produced foods. There thus develops, often with the aid of religious or ideological sanctions, a change in dietary patterns and habits. What is the status of milk from this viewpoint of efficient production, utilization, and displacement of other foods? Will the trend be towards relatively greater or lesser consumption of milk and its products?

One factor of overwhelming importance in the over-all economy of milk production is the possession by dairy cattle of a huge (30- to 40-gallon capacity) fermentation chamber, the rumen (Fig. 1). All swallowed food and water passes into this chamber, where it is warmed, agitated, and fermented under most ideal conditions. The rumen microorganisms, like some plants in the field, synthesize proteins from such simple nitrogenous compounds as urea, asparagin, succinamide, nitrates, nitrites, and ammonium bicarbonate. They also synthesize B-complex vitamins. These organisms also produce various organic acids, such as acidic, lactic, pyruvic, and such short-chain fatty acids as are found in butterfat. Cattle, moreover, consume and digest, with the aid of their rumen storage and fermentation facilities, fabulous quantities of roughage which such species as

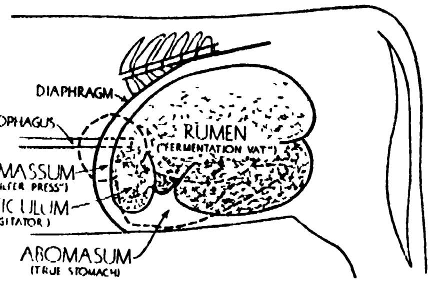

FIG. 1. DIGESTION IN CATTLE

THE MICROBIOLOGICAL SYNTHESIS OF AMINO ACIDS AND B VITAMINS IN THE RUMEN OF CATTLE MAKES THIS SPECIES WHOLLY INDEPENDENT OF DIETARY B-COMPLEX VITAMINS AND PARTLY INDEPENDENT OF DIETARY AMINO ACIDS.

pigs, chickens, and man cannot consume or digest. The milk nutrients are thus, in part, obtained in the nature of a windfall, from materials virtually useless for food of man and such simple-stomached species of farm animals as pigs and chickens.

A second factor in the economy of milk production is that it is an inherently efficient process. The energetic efficiency of milk production in superior dairy cows is

about 35 percent; 35 percent of the digested nutrient energy is converted into milk. The energetic efficiency of milk production is about twice as high as for egg production in comparably producing hens, and several times as high as for beef production. Pork production is more efficient than beef production, but pigs compete with man for grain and vitamins, which cows do only in part. The good dairy cow is the most efficient converter and creator (by way of the rumen microorganisms) of food of animal origin for man, and, unlike some cash plant crops, the grazing cow is a preserver of soil fertility.

The dairyman thus appears to be engaged in the most efficient food-producing enterprise, and milk as food is not likely to be replaced by other foods of animal origin. On the contrary, the relative consumption of milk in various forms is likely to increase with the passage of time because of the nutritional economy of milk production and because of the effect on soil fertility and national stability of the dairy enterprise.

THE broad significance of milk as a food may be considered from several viewpoints, such as its evolutionary significance, its physical condition, chemical composition, and nutritional value. Then, too, milk is unique in containing casein, short-chain fatty acids, and milk sugar not found elsewhere.

Milk is, in fact, the only substance developed in the course of evolution to serve as the exclusive food in the most critical period of postnatal mammalian life. True, milk is not suitable as an *only* food for adults, mostly because it is low in iron, copper, and manganese, substances normally supplied by the liver of young mammals. Many generations of animals have, however, been reared on exclusive milk diets supplemented with these minerals.[1] It is amazing that some 8,000 different species (about half extinct), ranging from reptile-like monotremes up through moles, mice, monkeys, and finally to man, should have developed this special food for their young, and that all milks should contain, although in different proportions, identical or closely similar milk constituents.

Since milk was evolved for infants, toothless and delicate of lining, it is not surprising that it has a fluid, bland form. Milk is a fluid, despite the fact that it has more solid nutrients per unit weight than many solid foods. For instance, whereas the solid content of fresh milk is 13 percent, that of turnips is 9 percent, tomatoes 6 percent, lettuce 5 percent. Milk, of course, coagulates into a solid—a light, fluffy curd – on reaching the stomach. These characteristics of milk are apparently evolutionary adaptations to the needs of the young. But these characteristics, together with the high buffer properties of milk, combine to make it also the most indispensable single food for the invalid—especially sufferers from delicate or injured stomach linings, such as ulcer. These properties also make it an excellent supplement to all diets.

As to the known chemical composition of milk, it is customary to present it in tabular form with, perhaps, a comparison of the content in a quart of milk (the optimal intake per day for an average person) with some dietary standard, such as the one recommended by the National Research Council. (Table 1.) This table of the chemical composition of milk does not, however, do justice to its nutritional significance; the nutritional value of a food cannot always be inferred from its chemical composition.

For instance, the recommended nicotinic acid for an average man is 18 mg. a day. As a quart of milk contains but 1 mg., it would be logical to conclude that one would have to consume 18 quarts of milk per day to supply the needed 18 mg. of nicotinic acid. This is not the case because milk is not only an excellent food for man but also for the microorganisms that synthesize

nicotinic acid in his intestine, and these organisms probably supply adequate nicotinic acid. The result is that a milk diet, although very poor in nicotinic acid, supplies abundant nicotinic acid, probably by stimulating the intestinal organisms to synthesize it (see reference *i*, Table 1). This holds to variable degrees for other water-soluble vitamins. Milk thus has a higher *nutritionally-effective* B-vitamin value than its chemical composition indicates.

It may be similarly shown that milk has

TABLE 1

NUTRIENT[a] LEVELS IN MILK WITH COMPARISON OF THE NATIONAL RESEARCH COUNCIL'S RECOMMENDATION

Nutrient	Amount in one quart[b] cow's milk	N R C daily standard[c]	Portion of standard need supplied by one qt cow's milk	Amount in one quart woman's milk
Protein, gm.	35	70	$\frac{1}{2}$	13
Fat, gm	39			35
Sugar, gm	49			75
Calories	700	3000	$\frac{1}{4}$	680
Ash, gm.	7 0			2.0
Calcium, gm.	1 2	0 8	$1\frac{1}{2}$	0 34
Phosphorus	1 0	1 2	$\frac{5}{6}$	0 15
Iron, mg	1 0	12	$\frac{1}{12}$	2.0
Copper, mg	0 1	1.0	$\frac{1}{10}$	0 6
Vitamin A, I U[d]	1800 ±50%	5000	$\frac{2}{5}$	2400
Vitamin D, I U[e]	20 (5 to 40)	400 (?)	(?)	4 to 100
Vitamin C, mg.[f]	20	75	$\frac{1}{4}$	50
Thiamine, mg[g]	0.4	1.8	$\frac{1}{4}$	0 14
Riboflavin, mg[h]	2 0	2 7	$\frac{3}{4}$	0 35-0 42
Nicotinic acid, mg[i]	0 8	18	$\frac{1}{18}$	1.8
Pantothenic acid, mg	3 5			2.0
Pyridoxine, mg.	0 67			0 04
Choline, mg .	147			
Biotin, mcg[j] .	30			8
Inositol, mg	180			330
Folic acid, mg	50			450
Total solids, gm	130			120
Water, gm	870			880

[a] For detailed vitamin values of cow's milk see Lawrence, J. M , Herrington, B. L , Maynard, L. A. *Am. J. Dis Child* , **70,** 193, 1945; *J. Nut.*, **32,** 73, 1946; Kon, S. K. *Proc. Nut. Soc* Cambridge, **2,** 149–157, 1944. For vitamin values of human milk, see Macy, I. G , *et al. Am. J. Dis. Child* , **43,** 1062, 1932, and **70,** 135, 1945.

[b] One quart milk = 0 975 kg. = 2.15 lb. = 946 5 cc. 1 kg milk = 1 025 quart.

[c] For a 70-kilogram (155-pound), moderately active man.

[d] Includes both vitamin A and carotene Great seasonal variations occur, depending on the content in food. Cow's milk contains about 330 mcg. vitamin A and 300 mcg. carotene; human milk, 650 mcg. vitamin A and 250 mcg. carotene.

[e] Vitamin D varies with the amount of exposure of cows to light and, therefore, with season. Commercial "vitamin D milk" contains up to 400 I.U. per quart.

[f] Vitamin C in milk as drawn from udder. Much is destroyed by pasteurization and light. Raw milk upon reaching the city contains about 17 mg. reduced ascorbic acid per liter; pasteurized milk delivered to consumer, about 5 mg. per liter and, if exposed to light, virtually none.

[g] Thiamine values are uncertain as from 5 to 25 per cent is destroyed on pasteurization

[h] Riboflavin in milk as drawn from udder. While stable to heat, it is very unstable to light—about half is destroyed on exposing milk bottle to light for several hours.

[i] The need for dietary nicotinic acid apparently varies with the nature of the diet, especially with the tryptophane content. See Krehl, W. A., *et al. J. Nut.* **31.** 85, 1946.

[j] One mcg. = 0.001 mg.

a higher *nutritionally-effective* protein value than its chemical composition indicates. This is because the proteins in milk, by supplying limiting amino acids, raise the biological value of inferior proteins consumed with the milk. For instance, the biological value of potato protein is 70 percent when eaten alone but 86 percent when eaten with milk; the biological value of white flour protein (in bread) is 50 percent when eaten alone and 75 percent when eaten with milk; the biological value of corn protein is almost doubled by consuming the corn with milk.

It will presently be shown that milk sugar acts as an antirachitic agent, an effect which could not be inferred from its chemical composition.

Similar statements might be made with regard to the supplementary and protective values of many other milk constituents.

MILK contains the four basic nutrient categories: vitamins, proteins, minerals, and fuel (fats and carbohydrates). These nutrients are found in all good natural foods, although in different proportions. It is intriguing, however, that milk sugar, or lactose, is found in milk alone. Milks from all species of mammals contain lactose, and no other natural food contains it. Lactation is apparently the only natural process in which lactose is formed. No other sugar is present in milk above trace levels.[2] The sugar in blood is glucose, and the mammary gland produces lactose mostly from glucose.

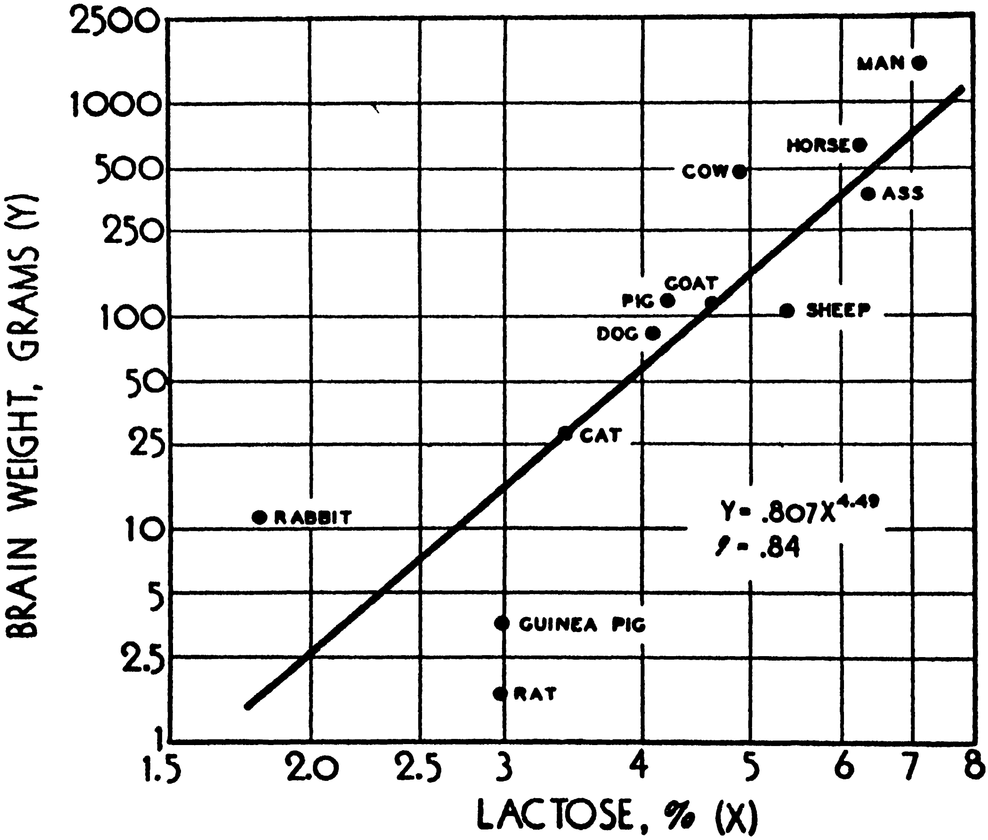

FIG. 2. LACTOSE CONTENT OF MILK VS. BRAIN WEIGHT

MAN HAS THE LARGEST BRAIN AND HUMAN MILK HAS THE HIGHEST LACTOSE PERCENTAGE. THE LACTOSE PERCENTAGE IN MILK OF DIFFERENT SPECIES TENDS TO VARY WITH THE BRAIN SIZE.

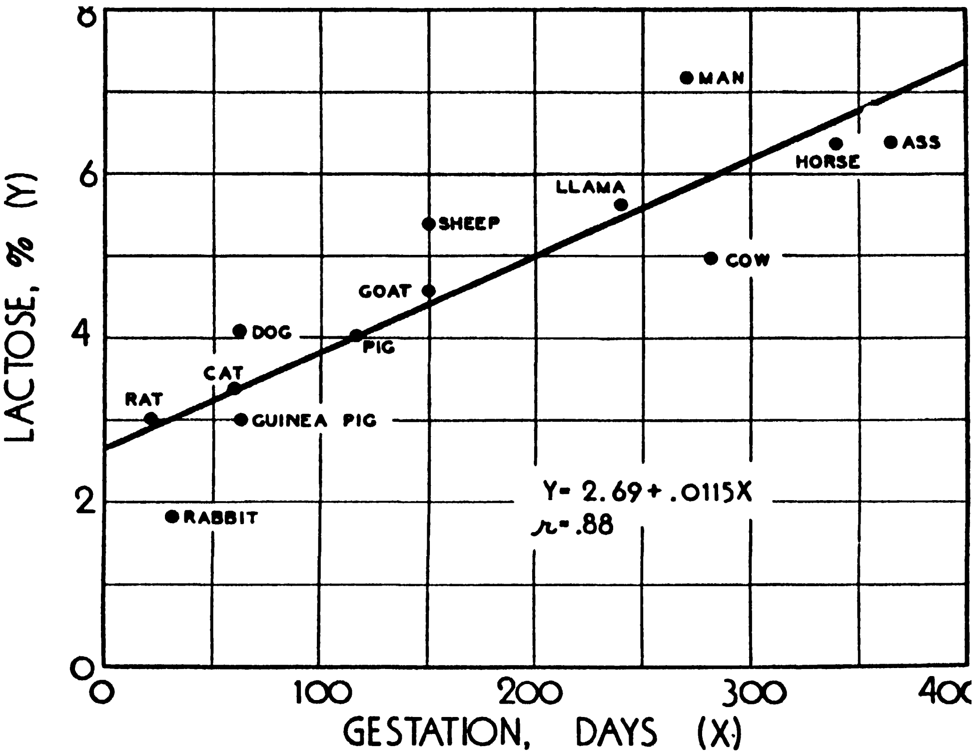

FIG. 3. LACTOSE CONTENT OF MILK VS. GESTATION PERIOD

THE LACTOSE PERCENTAGE IN MILK OF DIFFERENT SPECIES TENDS TO VARY WITH THE GESTATION PERIOD AND PHYSIOLOGIC AGE AT BIRTH.

It is also intriguing that of all species, human milk is richest in lactose. Lactose makes up over half (56 percent) of the dry matter in woman's milk, a little over a third (36 percent) in cow's milk, and only 6 percent in rabbit's milk. The sugar level in milk seems to vary *directly* with the weight of the adult brain (Fig. 2) and with the length of the gestation period (Fig. 3), and *inversely* with the fat and protein content of the milk (Figs. 4, 5).

Since the fuel value of lactose is the same as of glucose, from which it is derived, many believe that the nutritional value of milk sugar is no greater than that of cane sugar.[8] It does not, however, make sense to assume that (speaking figuratively) nature would go to the trouble of developing a special mechanism for producing lactose from glucose without serving some special function. Moreover, since mammals probably evolved from egg-laying forms, it would be simplest, from the evolutionary viewpoint, to produce milk of a composition similar to eggs, with the fuel in the form of fat; and the deviation from the principle of simplicity by evolving a special fuel, lactose, would seem to be significant. But what is that significance?

While lactose and sucrose have the same empirical formula, $C_{12}H_{22}O_{11}$, they differ in many respects. One molecule of sucrose yields on hydrolysis two molecules of glucose; but one molecule of lactose yields on hydrolysis one molecule of glucose and one of galactose. While glucose and galactose have the same empirical formula, $C_6H_{12}O_6$, they differ in spatial configuration.

The unique spatial configuration of lactose and of its constituent, galactose, gives it unique nutritional properties. For in-

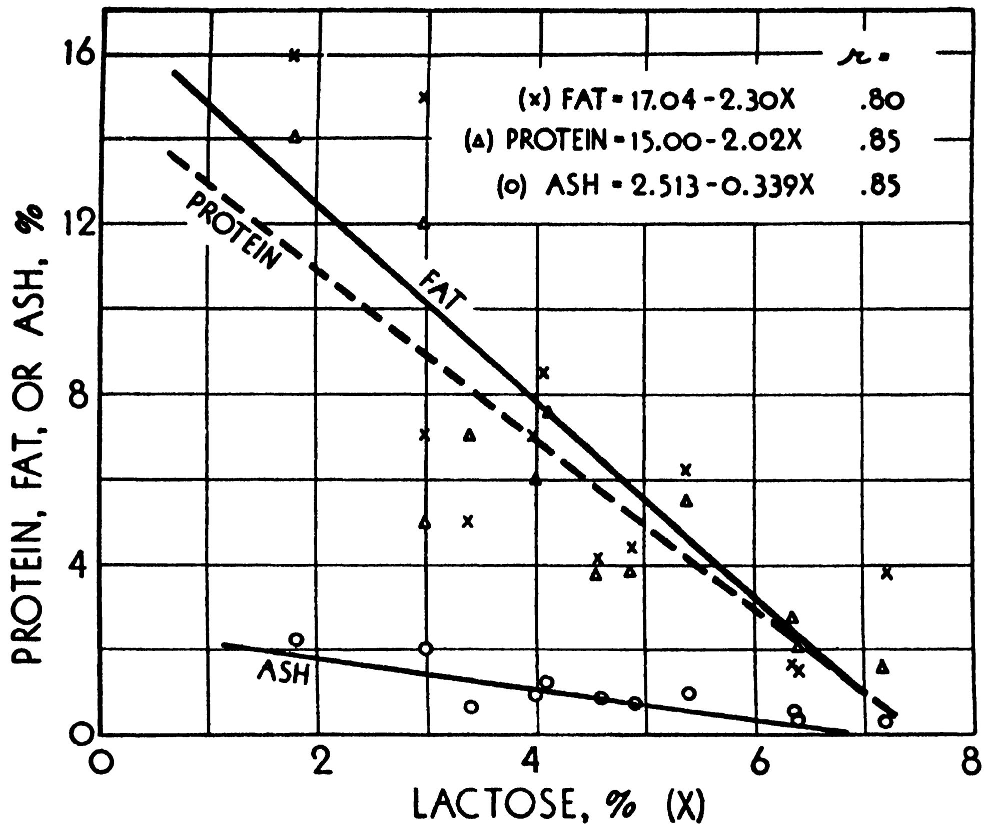

FIG. 4. LACTOSE VS. PROTEIN, FAT, AND ASH IN MILK

IT IS INTRIGUING THAT THE PERCENTAGES OF PROTEIN, FAT, AND ASH IN MILK OF DIFFERENT SPECIES SHOULD TEND TO VARY INVERSELY WITH THE PERCENTAGES OF LACTOSE.

stance, lactose is less soluble, less sweet, and more stable than sucrose. The lower sweetness of lactose (about one-sixth as sweet as sucrose) may be important in giving milk its bland taste and its acceptability as an exclusive diet. The lower solubility and greater stability of lactose enables it (unlike other sugars) to pass unchanged into the intestine. There the lactose nourishes acid-producing bacteria such as *B. acidophilus* and *B. bulgaricus*. The lactose is oxidized by these and related organisms to lactic acid.

And just as lactic acid keeps fermented milk (such as buttermilk) free from putrefaction and infection, so it also keeps the large intestine of man wholesome, free from scatological putrefaction. This is the basis of Metchnikoff's suggestion[4] for promoting "intestinal hygiene" and "prolongation of life" by greater consumption of milk.

The acid medium produced by lactic acid fermentation, moreover, facilitates the absorption or utilization of dietary calcium and phosphate; lactose thus acts as an antirachitic agent,[5] as does, for example, vitamin D, but, of course, by a different mechanism. Woman's milk is more antirachitic than cow's milk because it is much richer in lactose.

The relation of lactose to brain size (Fig. 2) may be explained by the fact that on hydrolysis lactose yields one molecule of galactose as well as one of glucose. The brain is rich in galactosides, that is, substances which contain galactose (as well as

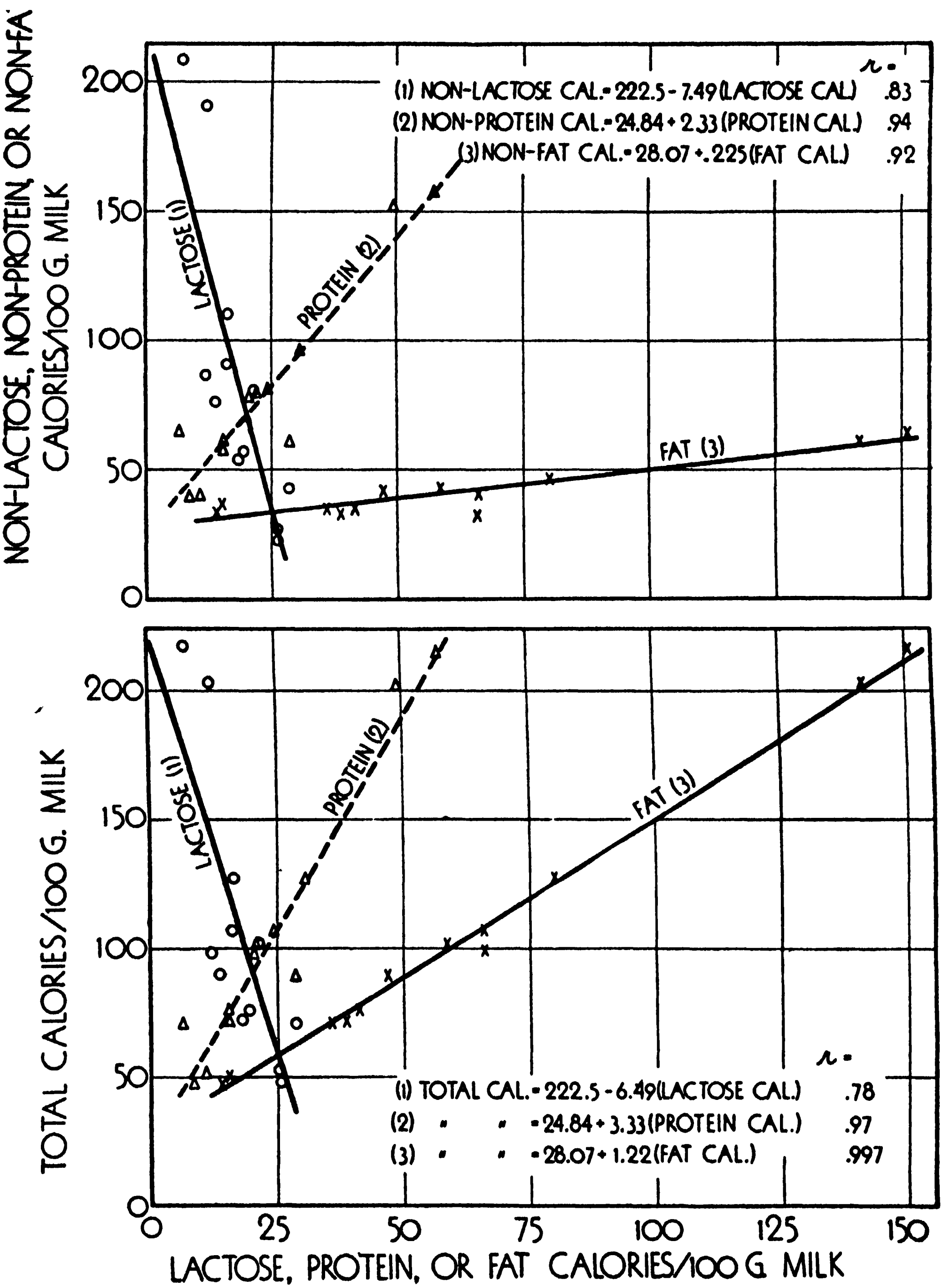

FIG. 5. CALORIC RELATIONS AMONG COMPONENTS OF MILK

THE TOTAL MILK CALORIES (LOWER HALF) AS WELL AS THE NONLACTOSE, NONPROTEIN, AND NONFAT CALORIES IN MILK OF DIFFERENT SPECIES TEND TO VARY DIRECTLY WITH THE FAT AND PROTEIN CALORIES BUT INVERSELY WITH THE LACTOSE CALORIES IN THE MILKS.

fatty acids and nitrogenous substances). It is reasonable to assume that the larger the brain and the more rapid its early growth, the more galactose it needs for its galactosides and, therefore, the more lactose it needs in the diet; and that different species of mammals developed milks with lactose levels in proportion to their needs for galactose.

Glucose could, theoretically, be used in place of galactose for the production of brain and nerve tissue; practically, however, glucose may not be stable enough: it is too readily oxidized; indeed, glucose is apparently the only fuel normally oxidized by the brain. Galactose, a more stable sugar, was therefore presumably evolved. The consumption of galactose in the form of lactose thus increases the stability of the brain and nervous system above what it would be if glucose were the only available sugar. The adult may be able to synthesize the galactose to supply the normal metabolic needs of the nervous system, but the infant may not be able to synthesize the lactose at a sufficiently rapid rate during early rapid growth and development. But even the adult's nervous system may, perhaps, benefit, perhaps maintain higher stability, by consuming lactose (in the form of milk).

There is no experimental evidence for the suggestion that lactose may serve as a "stabilizer" for the nervous system, but it is not unreasonable. It is known that the convulsions following parathyroidectomy may be prevented or stopped by a milk diet.[6] This effect is attributed to the calcium in the milk and to the antirachitic effect of the lactose, as previously explained. But it is also possible that the lactose itself may have a stabilizing effect on the nervous system by supplying it with the "building stone" galactose.

In addition to such galactosides as cerebrosides, the brain contains sphingomyelins similar to cerebrosides except that in place of the galactose group there is a choline-phosphate group. Choline is very important, often becoming a limiting factor in many metabolic processes, in addition to forming acetylcholine, and there is some evidence, based on the clinical syndromes of Gaucher's and of Newman-Pick's disease (absence of cerebrosides), that the galactose of milk sugar, which enters into the structure of cerebrosides, may "spare" the choline in the companion lipoid sphingomyelin. This might be a very important function of lactose—to act as a choline-sparer.

A great deal more could be said about the nutritional consequences of the greater stability of lactose, such as forming the relatively more stable "galactose glycogen,"[7] maintaining a more constant blood-sugar level,[8] serving as a prophylactic to ketosis.[9] It was said (not proved) that lactose-fed rats probably live longer than sucrose-fed rats,[10] and have more "living tissue" than glucose-fed rats,[11] and that the specific dynamic effect (a waste energy) of lactose is probably less than of glucose.[12]

The lactose differences in the milk of different species, aside from differences in brain size, may be associated with difference in choline need. For instance, on stimulating the nerves of the sweat glands, some species, such as man, produce acetylcholine while others, such as the horse, produce adrenaline. The choline, and therefore lactose, requirements would be different in the two species.

Finally, it appears that there is a supplementary relation between lactose and butterfat. "It seems that nature has put lactose and milk fat together as an optimum combination for the young animal;" "lactose has an as yet unknown effect on intestinal conditions which is counteracted by butterfat but not by corn oil."[13] This problem is still under investigation, so that there is no agreement on these conclusions.[13, 14]

The present knowledge of the function of lactose seems clear on some points and

confused on others. It is clear that it has antirachitic effects; that it suppresses putrefaction in the intestine and promotes "intestinal hygiene;" that it may tend to prolong life; that it is involved in, or at least correlated with, the growth and development of the mammalian brain and somehow with the length of the gestation period and physiologic age at birth; that it is apparently involved in stabilizing some metabolic processes in the body; that milk sugar and milk fat may supplement each other in nutrition, as might be expected from their simultaneous evolution and from the organism theory. More knowledge is, however, needed to enable one to speak with greater assurance on this fascinating problem of the evolution of lactose and other constituents in the "most perfect food," milk.

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THE PARALLEL ROLES OF PHYSICAL AND SOCIAL SCIENCE

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IN THESE times when the values of science are being actively considered and issues are raised as between the service of social and physical science, it is appropriate to seek some perspective on the work which is being done. If we are to contribute to, rather than delay, the "trend toward system and unity in the scientific enterprise as a whole" we cannot afford to magnify distinguishing differences. We must look for some general purpose in the joint endeavor of scientists. Strange symbols and esoteric details often keep one area of science from becoming familiar to workers in another area. Can we not find common ground by overlooking technicalities and turning to larger aspects?

The effort to see social science in perspective is made difficult by the fact that we are all so close to it. And, again, the study of man and his relations, pursued in the manner of science rather than in the manner of individual judgment and ready prescription, has not been under way sufficiently long for its approach to prevail widely. There is a strong tendency to view the entire field in personal, common-sense terms. One is likely to ask, "What is individual behavior more than volition, and what are social problems more than differences of interest? Is not the heart of the matter that of getting people to be less selfish in their living and more cosmopolitan in their outlook?" Perhaps; but does such a general conception direct attention to the possibilities of scientific insight? Does it provide suitable guides for scientific effort? Familiarity with outward aspects does not guarantee understanding of inward relations.

In parallel fashion one might ask, "What is physics more than the study of the relationships and movements of units, large and small? Is not the heart of the problem that of getting things to move as we desire them to?" Possibly; but from a statement of this nature one might conclude that physics was given mainly to such commonplace matters as how to drive nails or knock croquet balls about. Simple conceptions do not encompass the horizons of scientific endeavor, nor do they establish its basic character. The sophistication of mature thought is not comprehended in sketchy verbalisms.

It is important to ask whether a particular conception of a field is conducive to the development of science on the level of basic principles or on the level of engineering applications. Is there some danger that we, as scientists, shall insist that physical science be thought of in terms of systematic abstractions and at the same time conceive of social science in terms of purely practical attempts? To do so is to contribute to the continued poverty of social understanding and to deny to social science the discharge of its main duty. For one does not approach an area solely in terms of immediate, practical considerations and come out with science. In all fields of understanding there is the necessity for a framework of theory as a basis for more concrete forms of application, and it is to the gradual development of this frame of thought that fundamental effort in any science must primarily be dedicated.

The development and testing of theory carry us beyond the level of everyday observation and interpretation. So long as one looks at society in terms of personal problems he will see only personal problems in society; when one views society with scientific interest he will observe in man's

behavior significant generalities. Basically, it is the social scientist's purpose to find in social phenomena those underlying uniformities which can be integrated into a thought structure that is capable of abstract manipulation and is reasonably accurate in predicting consequences. By virtue of these properties this science should be effective in affording man some control over the world of social events. It is the social scientist's conviction that by studying man and his behavior scientifically he will eventually be able to offer society not only a coherent system of ideas about man, but a consistent set of principles which can be utilized by man for his own betterment.

What problems will social science solve? Will it undertake to remake human nature? Will it stop wars? Purge society of crim ? Eliminate poverty? For perspective we can turn to the field of physical science. Does man try to change the course of the planets? Does he counsel the seasons? Do his deliberations level the tides? The answer is that man does what he reasonably can. He adapts nature to his advantage where feasible and adapts his habits to nature where necessary. Thus he stores the rain in reservoirs, but builds houses and wears clothes to temper the effects of weather. He seeks to manage in part where he cannot command in full. Without serious thought of checking the continuous rise and fall of coastlines he dredges harbors and raises dikes. In the field of human relations we can expect man to do the same sorts of things he does in the field of physical relations. The social scientist cannot remake basic human traits, but he can modify and redirect them. He can aid man in making more satisfying and efficient adaptations and, as his scientific knowledge increases, he can develop new forms of control over social conditions and forces.

How, in practical terms, are such things to be done? For example, how will social science settle conflicts of interest from which so many social difficulties spring? Will science forsake its long tradition of impartiality and take sides? Will it make the strong stronger and the weak weaker? Will it prepare for one side lessons in the art of docile living while tendering to the other techniques of domination? The answer must be that immediate action is the province of the administrator and the practical worker, not of the researcher or the designer. The social scientist, in studying the forces of society, will take note of those which oppose each other, but he will not expect to build science by deciding disputes. He may participate in practical affairs as a form of personal service and as a matter of maintaining an orientation, but vocationally his are not the functions of the legislator, the administrator, the jurist, or the moralist. The world's work has many divisions with differing responsibilities. The scientist's service is real even if it be several steps removed.

In the physical field, man has learned to utilize much of science for his advantage. We may therefore turn to technology for further ideas on how to put knowledge to work. Physical science has not eliminated opposed forces either in equilibriums or in "opposite and equal reactions;" and we cannot expect to eradicate them, in either static or dynamic form, from the social scene. But through technical applications of science man does hope to deal with forces, whether physical or social, before they reach critical proportions, and his goal is to arrange conditions so that forces will expend themselves in contributing to the common good instead of wasting in useless heat or dissipating in wanton destruction. The purpose of science, practically, is to provide principles by which such ends can be achieved.

Applications of physical science have given us factories, railroads, airplanes, radios, mechanical refrigerators, and so on. These things are possible because means have been found for transforming

and channeling energy so that it behaves within limits which man expects and regards as desirable. Man builds machines which utilize force and carefully direct it. Thus the combustion in the cylinder of a gasoline motor does not wreck engine and operator because the energy is confined by adequate forces and masses opposed to it, and the force generated is largely directed toward the work to be done. Technology, rooted in principles of science, consists essentially in prearranging responding parts and inertial masses so that force will serve man's will.

The social scientist thinks of social machinery in the same fashion. For every phase of scientific knowledge and application in the physical field there is a counterpart in the realm of society. Social forces, no less than physical forces, have their methods of generation, their mediums of transmission, and their appropriate machinery for translation into effective results. Anyone who doubts the reality of social force must of necessity doubt the reality of war, and anyone inclined to question the actuality of social machinery must question the existence of government. The commonly observed conflicts of interest in the field of society are no different in their general outline from the contrary forces in the physical world or the universal competition in the biological world. The problem in any case is one of studying the kinds of machinery which are effective and efficient in transforming, conveying, and utilizing force.

The skeptical may insist, "Show me these social machines. Let me examine their parts. I must have concreteness." Does one ask to see shafts and pulleys in the universe? Does he question whether objects fall because he can see no lever to push them down? Are we to doubt that light and gravity traverse empty space because we know of no medium which conducts them? Social machines are common patterns wrought in the minds of men. They exist largely as conventions, sometimes written, more often not. They may be laws, they may be traditions, they may be newly accepted practices. In essence, they are attitudes, habits, and expectancies —mind sets which generate, guide, and limit action. But we need not be concerned here with substance. That is the province of the social scientist; the point of general interest is that conditions can be arranged in society so that force is channeled and regularized.

To avoid a world of chaos all of us want adequate social machinery, and we want competent administrators prepared to operate it. We want the aggregate energy of mankind to express itself within generally acceptable limits and be directed toward the accomplishment of ends which are both socially and individually beneficial. It is theoretically possible to design social machinery having various degrees of immediate efficiency—to waste different amounts of the social energy in "heat," "friction," and "lost motion." It is possible to set the design so that the tolerance limits for satisfactory results are very narrow, as in totalitarian states, or very broad, as in a democracy. It is possible through design to establish high or low factors of dependability, stability, or safety. In short, with increasing scientific knowledge and within the limits of compatible characteristics, any kind of social machinery that is desired can be arranged.

One may be tempted to ask, "Then why don't the social scientists design an ideal machine and stop all the inefficiency and turmoil in the social world?" One might as well ask, "Why don't the engineers in the automotive field give us a motor which obtains all the value of the fuel, loses no energy in wasted heat, has no internal load, does not get out of order, and will not be superseded by something better?" Our wishes must not mislead our judgment. We live in a practical world, a world of conflicting values and necessary

compromises, both social and physical. There is not now and never will be any perfect form of gasoline motor, or of social life. For that which is ideal in some respects is achieved by sacrifices elsewhere; and that which has been attained is soon thought ordinary, denying satisfaction for long. In the dreams of tomorrow the goals of today are forgotten. Knowledge advances, interests shift, desires expand. Under the impetus of changing conditions and perspectives designs also must change. Science knows no rest. Continued effort fashions constantly improved offerings, but never the ultimate.

With respect to getting his knowledge and insights incorporated into practice, the social scientist is in much the same position as the designing or consulting engineer. Neither one can set up what he regards as the most desirable model and then say, "Here is what the world should have; take it." Save for their professional writing both work at the bidding of others and within limits set by others. The scientist characteristically and almost necessarily looks at his problems in fairly narrow terms; society has found it practicable to set up other groups (administrators and legislators) to look at problems in broader terms and to interpret more widely the desires of people. So the designer of some phase of a new model of an automobile is told to work out his design within the limits of certain over-all specifications decided on by a planning board, most of whose members are not engineers at all. Likewise the social scientist who works out a new form of government, or perhaps a model city plan, will probably not find it of any use to do so unless he has been requested by an administrator or a policy-forming group to assist them, and unless they have laid down the general limits within which his study and recommendations are to fall. When engineers are members of policy-forming groups they have a better opportunity to advance their particular views; but the social engineer is not free to "run" society any more than a designing engineer "runs" an industry. Scientists are frequently disappointed; but beyond the beliefs and recommendations of specialists lie the judgments of the great body of people who, theoretically and practically, are the ones to be satisfied.

THERE are other parallels between physical and social science which should be noted. Perhaps the one of largest interest concerns the presumed normative character of social study. One not infrequently finds among his friends a tendency to think of the events of the world as falling into two classes, those which are in the domain of science and those which are covered by morals. There is a tendency further to identify physical things with science and social affairs with morals. Such an attitude grows out of man's long background of thinking about human action primarily from the moral point of view; he may not realize that behavior can also be looked at from many other frames of reference. Hence, even among some physical scientists, there is a strong belief that the chief character of social science is normative, that is, that it exists to determine what people *ought* to do. Much of what has just been said bears on the point: Society has given to groups other than physical and social scientists the function of interpreting its current wants and deciding what should be done. But the question as usually conceived goes deeper: Is there not something beyond immediate wants? Are there no fundamentals in man's existence? Does the social scientist not have anything more to offer society than machines? Can he give no advice as to the ends for which machines are to be used?

The first answer to these questions is, No. The fact that, as individuals, we recognize a strong need for definitive answers in the area of large purposes and ideals is not warrant for assuming that science is the

appropriate medium for providing those answers. We must always give heed to the fine line between science and philosophy. The first obligation of science is to view the world of things as they are. This view is normative only in the sense that it is descriptive. Social science is concerned with the same kind of aspects in the social world that physical science is in the physical world. One is not immediately any more moral than the other. The problems of weighing the findings of scientific work and weaving them, along with other considerations, into a suitable picture of the ends for which man should hope and the purposes for which he should live are the responsibility chiefly of philosophy and of religion.

But the second answer to the questions is a qualified Yes. Man's basic interest in the world does not stop with a description of statics; man seeks as well a description of natural processes, of changes which regularly follow certain patterns. The second obligation of science, therefore, is to deal with the world as a system of interactions—a world of cause and consequence. The mature scientist sees the static aspects of nature as but phases of a dynamic universe. Accordingly, the social scientist, in studying manifold social sequences, has the duty of studying those which involve various goals and various intermediate values. When man lives according to certain specified ideologies, what happens? Does he progress toward the goals he hoped for? What do different patterns of living lead to?

The social scientist does not seek to prescribe the ultimate; science finds no answers for basic teleology. That is an area for individual interpretation and group choice. But science can hope to identify those proximate ends which tend toward certain ultimate purposes, and it can in turn describe the general effect on society of holding various philosophies. Science has no traffic with notions of inherent rightness, but it has a basic interest in discovering what results follow the acceptance of particular orientations. To this end the social scientist will gather data over long periods of human experience and from the varied practices of widely dispersed societies. He will describe, analyze, and relate. Like the physical scientist, he will seek to sift the essential from the nonessential among the conditions which produce certain results. Without presumption to moral finality, his findings will give breadth and perspective to the thinking of philosophers and to the plans of statesmen.

As for those who may fear that the social scientist, after prolonged study, will decide how man should live, and dictate the ends he should accept for his existence, they may refer to the relation between science and philosophy or religion. Certainly physical science narrows the range within which man's reasoning is unrestricted; but does it not still leave large opportunity for man's yearning to go beyond scientific facts and build pictures which quicken his daily effort, strengthen his high resolve, and encourage his trust that in ethics there is something enduring? Social science will restrict and refine man's notions about what is good, but it will not take from man his right to think, to interpret, and to decide. Social science affords a mirror for man's behavior and a telescope for his judgment, but man's ultimate choice is his own. Society determines its course, with such counsel as it may take, through the instrumentality of the machinery it elects.

The reality of values cannot be doubted. There are ends for which each individual hopes and there are goals toward which he strives. Values relate his activities to these large ends. They are as real as the tropisms of biology or the "pulls" in any field of science. We may not understand why a tree grows vertically but we do not therefore ignore or deny the tendency. One might as well demand a science of physics without gravity, or a science of chemistry

without valence, as a science of man without values. They are part of the scheme of attractions which move mankind as planets and particles are moved. The full nature of the causes which make all things move—the nature of energy, of life, of psychological response—is perhaps as much a basic mystery in one field of science as another. Science studies what it finds means of studying. We must concede actualities whether or not we can resolve them.

But values are not absolute; they are not ultimate, they are instrumental. They are connecting links between statuses and purposes. They inhere in the relationships between properties and large designs, relating the means to the end. We may draw an analogy from the physical world by asking, What is the value of air? The respondent may point out that air is necessary to support life, though it does not guarantee life. It affords temperature moderation, else we should have the extreme heat and cold of the moon and all life would disappear. It carries water vapor, supporting the hydrologic cycle and permitting terrestrial life. It transmits sound. It supports aircraft but is an impediment to rockets. It turns windmills and sails ships; it produces storms and destroys. It is generally impure; it often contains smoke, sulfur dioxide, and noxious gases. It carries dusts, soot, bacteria, viruses, pollens, and fungi, giving rise to many ills. What is the value of air? Specific properties become utilities when related to purposes. Values, scientifically, are these utilities.

Given certain ends, the industrial engineer can say that certain materials and processes are necessary in order to achieve them. We hope that the social scientist or social engineer may be able to draw on his pure principles to render the same sort of service. He will be acquainted with the basic psychological needs of mankind—the essentials without which a society cannot be stable and maintain a long-time existence. He will know what values are called for in order that certain more distant ends espoused by a civilization may be approached. He will recognize that certain goals are incompatible with others. He should be able to outline what consequences may be expected when certain values are increased, reduced, or altered.

And, while pursuing these practical activities of helping society solve its problems, he will probably find opportunity to develop his basic science still further. Has not pure chemistry profited from research connected with the very practical problems of producing better electric lamps, or polyamides for manufacturing nylon?

Science arises wherever man turns his attention with intent to observe, describe, relate, and synthesize. In both physical and social science the fundamental object is the study of force—its generation, its behavior under the influence of various factors, its effective control through the arrangement of other adequate forces and masses, and ultimately its appropriate channeling so that it will serve ends chosen in advance by man. These ends then become a part of the cause-effect sequence which must be understood in designing and operating physical or social machinery. The social scientist may wish with the physical scientist that the affairs of man were conducted on a higher plane; but he takes human nature and human behavior as the physical scientist takes physical nature and physical behavior. It is their duty to study and describe and to offer their findings for the service of mankind. It is not ground for criticism, however, if either one works with the thought that his developing insights may eventually bear fruit in making a better world.

The advance of science lies along a broad avenue, theoretical on one side, practical on the other. We must honor workers who find their chief interest principally along one side or the other of this avenue There must be theory; there must be applications.

What the ideal proportion is cannot be stated. Pure science must synthesize and provide an interpretation for the numerous practical studies; it must delve into areas which only the theorist immediately cares about; it must be the fountain from which many new applications become possible. But basic science is not the most common nor the most conspicuous form. The ratio of expenditures for applied research to expenditures for pure research is about six to one in this country, not counting war research. If social science seems at times to be characterized by nearness to practical problems, does it not find a stimulating precedent in physical science?

It is not fitting to close without pausing for recognition of the underlying motives of the true scientist. His work is so little understood that it deserves frequent appreciation. Behind and beyond all thoughtful modes of adaptation lies the scientific spirit. This spirit is not entirely separate from man's wants, for it dwells in man; but neither is it to be identified wholly with practical affairs, for it transcends the bounds of necessity and exceeds the stress of immediacy. It may be called man's curiosity at work; in truth it is more than that. It is the steady conquest by intellectual force of the mysteries of nature. It is the reconstruction in comprehensible terms of a universe which, in its original form, is largely incomprehensible. Science is man's most daring response to the challenge of the vast unknown. It is an expression of his willingness fearlessly to face truth. If in our several labors we have effected something of a division of effort, can it be said that research workers are less able to satisfy their deep desire to understand and appreciate nature when engaged in the study of man than when engaged in the study of masses?

A "GRASS-COUNTER" IN THE PACIFIC*

By L. T. BURCHAM
Captain, U S Marine Corps Reserve

THE following is a compilation from notes and letters written while on duty with the First Marine Division, Fleet Marine Force, in the Southwest and Western Pacific, with such additional remarks regarding place names, locations, and movements as the removal of wartime censorship regulations now permits The writer was on military furlough from a position as Range Examiner with the U. S Department of Interior

Somewhere in New Zealand
November 15, 1942

Dear Miriam:

While en route to our overseas destination our conversations frequently turned to speculation on the nature of the terrain and vegetation in the country we would be visiting. Knowing that the climate of the region was of the Mediterranean type, I'd predicted that we'd find many of the familiar annual grasses and weeds which have been introduced into southern California and are so common and widespread in other regions This suggestion was met with comments ranging from faint doubt to absolute disbelief

The day after we reached port I had an opportunity to get ashore for a few hours. Sure enough, before we'd been on land ten minutes we passed a waste spot beside a warehouse where we saw bur clover (*Medicago hispida*), a rye grass (*Lolium* sp.), a brome grass (*Bromus* sp.), and the same species of chicory that was growing in the garden in front of our house when I left home. During the afternoon's walk, all within city limits, we observed 12 additional species of plants—aside from those cultivated in gardens which would be familiar to almost anyone: These included 5 grasses, 2 shrubs, and 5 broadleaf herbs. They bear out my point that to one interested in plants and their distribution, familiar friends will be waiting whenever he steps ashore These will help him to feel at home in strange places, and one will also be able to make new and delightful acquaintances

So it is here one sees a few familiar plants and many strange new ones The general appearance of the vegetation is interesting, too, largely from the effect of the wind and topography upon it We are in an area where steep mountains are fronted by a narrow, comparatively level coastal plain. The original vegetation, now largely cleared away, was forest, with small areas of grassy meadow in some of the canyon bottoms and on the windswept heights. There are, in this region, prevailing winds from the northwest quadrant which sweep in off the Tasman Sea and Cook Straits almost continuously and with great force, they carry quantities of fine sand particles, enough to affect the eyes. The apparent effect on tree vegetation is to "hedge" it-- when traveling a trail that affords a view downward on the tops of trees and shrubs they appear similar to a close-cropped hedge or as though heavily browsed by livestock Their leaves are nearly all thickly cutinized and leathery in texture.

Within, these forests have a tropical aspect. The canopy is quite dense, there is almost a complete absence of undergrowth and practically no litter under the trees. Epiphytes and mosses, lichens and lianas are plentiful, the lianas frequently entwining the trunk of some small tree so tightly as to deform it into a gnarled, spiraled, grotesque form. Ferns are so

*"Grass-counter" is a common appellation of range examiners throughout the Western states

predominantly a part of this flora that the fern has been taken as the emblem of the Dominion; they are everywhere in evidence —sword fern, bracken, maidenhair, and the ancestral tree ferns.

To you—in the Northern Hemisphere where winter is just beginning—it will seem strange when I say it is early spring here. The flowering heads of the grasses are just starting to break out of their sheaths. In another week or so I should be able to start the collections that I've planned; so I shall have to get busy then and "make hay while the sun shines."

Guadalcanal
December 7, 1942

Dear Miriam.

This island is somewhat unique among the islands of the Southwest Pacific in that its northern half consists of a gently rolling coastal plain extending from the base of a high range of mountains to the sea, a feature that is not common in these parts where rugged topography is the rule. Natural grasslands occupy considerable portions of this coastal plain, with long fingers extending up into the foothills on the north and west slopes of the ridges. The species of both grasses and other plants are for the most part unfamiliar to me, but their characteristics are similar to many observed in other areas; it will be fascinating to delve more deeply into the relationships of the plants encountered here.

Goodenough Island
Territory of Papua
October 25, 1943

Dear Miriam:

After a ten months' sojourn in Australia we are again in the tropics. Their warmth is particularly ingratiating after the chill of a winter in southern latitudes. And again the abrupt transition from winter to summer has emphasized the verdure of the tropics.

Our camp is located in the margin of the rain forest, not far from the base of Mount Nimadao, a peak that towers more than 7,000 feet above us and has its summit wreathed in white clouds during much of the day.

The mature rain forest, such as that in which we are camped, is composed of a wide variety of trees, both hardwoods and softwoods, ranging up to 150 feet or more in height. I have noted 3 or 4 species of wild figs, several legumes, one tree whose fruits indicate it to be an ebony, and one that I believe belongs to the genus *Eucalyptus;* these are only a few of the almost innumerable kinds. Ordinarily there is little underbrush in the mature forest — small native clearings tend to cause a good deal of it, extending into the surrounding forest at every point where the sun is admitted There is a considerable ground cover of ferns, mosses, broad-leaved herbs, and quite a few trailing plants of the legume and morning-glory families; the treetops are often intertwined with many lianas. I have noted no grasses growing in the forest proper but have seen a few small sedges A number of grasses which apparently require part shade have been seen along the margins of the forest. The litter on the forest floor is very sparse, in places not entirely covering the ground; seldom more than an inch in depth, this is in vivid contrast to the relatively deep litter occurring under forests at home.

From this description you can correctly assume that my impression of the jungle is quite divergent from that portrayed by current stories from the Pacific islands which tell of the dank, steaming masses of tangled tropical vegetation growing in a perpetual ooze. Indeed, I find that the jungle is *not* a fearsome place.

Between our camp and the ocean stretches some 2 miles of savanna—tall tropical grasses varying from 3 to 6 feet tall, interspersed with occasional trees which are a light-barked, sparse-foliaged species of legume, invariably found associated with the grasses of these savannas. Elsewhere

Official U.S. Marine Corps Photo

MOUNT NIMADAO, GOODENOUGH ISLAND

ITS SUMMIT, TOWERING MORE THAN SEVEN THOUSAND FEET, IS USUALLY WREATHED IN WHITE CLOUDS.

appear considerable tracts of open grassland—the grass species being similar to those in the savannas—but the trees are absent. On this island some areas of grassland extend high up on the mountain slopes, to elevations of 3 or 4 thousand feet above sea level.

Since Goodenough Island has been scarcely touched by white men and was but thinly populated by native peoples prior to their advent, the natural vegetation has been very little disturbed. Collections should prove interesting as well as provide useful data on the distribution of plants occurring here.

Official U S Marine Corps Photo

ISLAND NATIVES

A DARK-SKINNED, FUZZY-HAIRED, SHORT-STATURED MELANESIAN PEOPLE ON GOODENOUGH ISLAND.

Goodenough Island
November 26, 1943

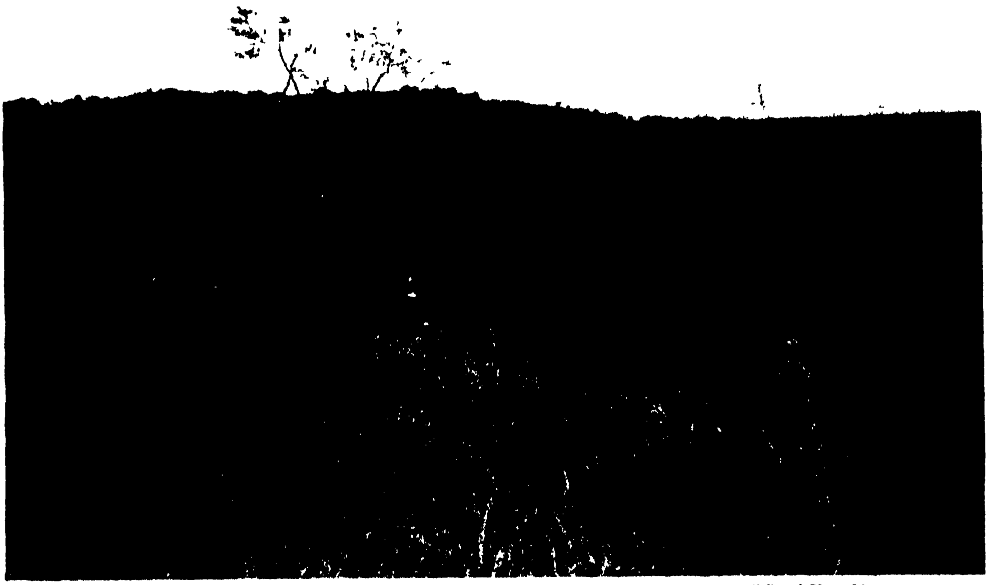
Dear Miriam:

My first collecting was done in the vicinity of a small native village located a short distance from our camp. For the most part the local inhabitants—a dark-skinned, short-statured, fuzzy-haired Melanesian people—were removed to a neighboring island when Allied military forces began to establish a base here. I prowled about their deserted houses. They are small, grass-thatched shacks, raised 4 to 5 feet above the ground to keep out dampness from the frequent rains. The floors are made from pieces of split bamboo or the outer layer of the trunk of a palm tree, with cracks between to provide ample ventilation. Considering that they are built entirely of local materials, without a single nail or other iron fastening, they are remarkable structures indeed; in addition, they seem very well adapted to the climate in which these people dwell.

A satisfactory collection was obtained from the environs of the village and from the surrounding grassland—nine grasses, representing the dominant and associated species which are typical of these areas.

Recently an opportunity was afforded to spend a day in the grasslands on the upper slopes of Mount Nimadao. There are appreciable differences between these higher elevation grasslands and the ones of the coastal areas. In the lowlands the dominants are kangaroo grass (*Themeda australis*), blade grass (*Imperata cylindrica*), and a species of *Manisuris*. As one ascends the mountain slopes, first the blade grass and *Manisuris* disappear at elevations of about 1,000 feet; between 1,500 and 2,000 feet the kangaroo grass gradually diminishes in quantity until, at elevations higher than these, there is left only a tufted bunch grass, which was not in flower at this time and hence could not be identified.

Such occasions as this, when one can climb mountain trails, scramble over rocky ledges, and visit new spots, are quite a pleasant holiday from routine work. Needless to say, other things than the plants catch my eye on these trips. The birds impress me, more by their vociferousness than by numbers. This applies particularly to the white cockatoos, parrots, friar birds, and pigeons. We saw hornbills—



Official U. S. Marine Corps Photo

SAVANNA, GOODENOUGH ISLAND

TROPICAL GRASSES RANGING FROM THREE TO SIX FEET TALL ARE INTERSPERSED WITH OCCASIONAL TREES. THE DOMINANT GRASSES HERE ARE *Manisuris rottboellioides* AND *Themeda australis*

truly birds from the circus—with immense, grotesquely-shaped beaks, and loud, swishing flight like the hiss of escaping steam. I think the most interesting was a lordly eagle—the Red-backed Sea Eagle, with rich chestnut plumage, except for white head and neck, and white-tipped tail; several of them were very busily engaged in catching, of all things, grasshoppers! No accounting for the doings of the little wild folk.

At one point we came out onto the crest of a hill that afforded a view of the entire east coast of the island. Spread before us were trees, grassland; the camps on the coastal plain, with here and there thin plumes of blue smoke curling upward; and beyond that, the white breakers on the reefs and the Solomon Sea in the distance.

Goodenough Island
November 29, 1943

Dear Miriam:

Tonight I must tell you of one of the most intriguing sights I've seen in many a long day—truly something you'd never expect to see outside of fairyland.

There is here a species of small white mushroom, commonly found on wet pieces of partially rotted wood. It is a diminutive, delicate thing, never more than about half an inch across the cap, and with a slender stem not that long. Its chalky whiteness heightens the impression of frailty.

Beautiful as it is by day, it is amazingly enchanting at night. For then it becomes luminous with a pale-green phosphorescence that is so strong a watch can be read by the

glow. As youngsters we've all heard a great deal about fairies—and that a toadstool springs up whenever they want to sit down. When grown up, of course we think that all a lot of nonsense. Having seen this mushroom, I wonder if there mightn't be some truth underlying the fairy tale—and I wish so much that every child could have the privilege of at least once walking in darkness along a path bordered with these tiny, glowing, fairy jewels.

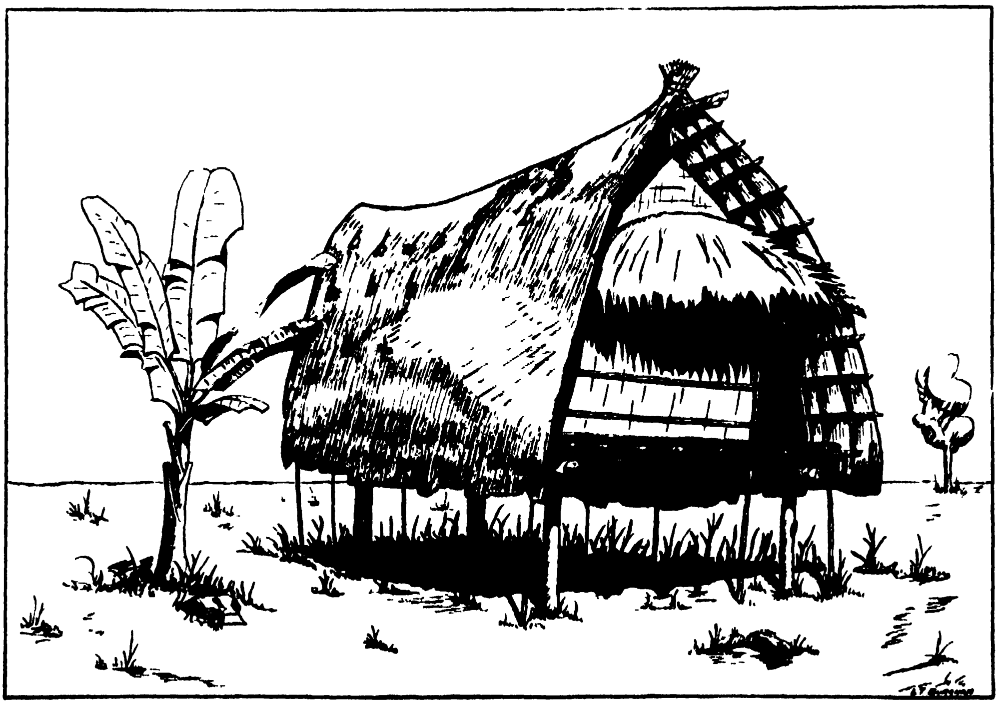
Cape Gloucester
New Britain
February 19, 1944

Dear Miriam:

When we returned to camp yesterday I had quite a stack of mail waiting 35 letters, 2 packages, and some magazines. You have no doubt been concerned over my not writing for nearly a month, but it was unavoidable.

You see, we started out on what was to have been a routine 5-day patrol—and did not return until yesterday, 28 days later During that time we walked a straight-line distance of more than 75 miles through most interesting country; but since I went over some portions of the trail 4 to 6 times, as well as taking a few "side trips," I traveled over 125 miles. Well, I always wanted to go on an extended hiking trip; every minute of this one was fascinating.

Our purpose was to contact the natives of western New Britain, who had fled into the hills from the enemy, to tell them that it was now safe to return to their homes and to escort them back to their villages. Our patrol started from Sag Sag, at the extreme western tip of the island, and crossed the divide between Mount Talawe on the north and Mount Tangi on the south, into the interior of the island; from here we turned south and followed the Itni River to the



NATIVE HOUSE, HAIWALI VILLAGE, GOODENOUGH ISLAND
THESE SMALL, GRASS-THATCHED SHACKS, RAISED FOUR TO FIVE FEET TO KEEP OUT DAMPNESS, ARE BUILT ENTIRELY FROM LOCAL MATERIALS, WITHOUT A SINGLE NAIL OR OTHER IRON FASTENING.

Official U.S Marine Corps Photo

WESTERN NEW BRITAIN NATIVES

HIDING FROM THE ENEMY IN THE HILLS, SOME HAD LIVED IN CAVES FOR NEARLY TWO YEARS. THIS PARTY OF WOMEN IS RETURNING TO THE VILLAGE OF AIPATI, CARRYING THEIR GOODS ON THEIR HEADS.

south coast of New Britain. Then we retraced this route to the native village of Agulupella, in the interior, and from there continued toward the northeast to strike the north coast of New Britain at Borgen Bay, and then west along the coast to our camp at Cape Gloucester.

Much of our route was through country which had probably not been visited by white men prior to this time. While I could not make any collections on this patrol, I did keep an extensive record of my observations in my notebook.

Where we crossed the divide on the south slopes of Mount Talawe was in the lower fringe of the "moss forest," which occurs above the lower limits of cloud formation on the mountain slopes. It is marked by a great increase in epiphytic mosses and ferns and other signs of constant dampness and lower temperatures; there is also a conspicuous tree with smooth, thin, white bark, resembling birch in general appearance, but not papery nor peeling as that does.

Just east of the divide was an excellent example of the virgin forests at elevations of approximately 2,500 feet. The mature trees, about 20 per acre, are from 150 to 200 feet in total height, with boles clear and straight to well over 100 feet in many instances. Most of them would average around 40 inches in diameter at breast height (4.5 feet above average ground level), with some as large as 60 to 70 inches. There is here the usual profusion of species and of mixed softwoods and hardwoods—the hardwoods including some known locally as "mahogany" and "rosewood,"

as well as teak, ebony, and eucalyptus, all of which are valuable timber trees. The crowns spread widely, forming a canopy that is almost unbroken. The soil is deep and fertile, a well-drained clay loam that is heavily humified to approximately one foot in depth and underlain with basaltic bedrock at 3 to 6 feet. There is practically no litter on the forest floor, as the dead vegetation decays almost as rapidly as it falls. While reproduction and undergrowth are plentiful, they are not dense enough to hamper movement to any appreciable degree.

We camped for the night in this forest. Rain had been falling intermittently all during the day; shortly after we stopped it began in earnest. With the assistance of the native boys guiding our party we built a leaf-thatched lean-to and found wood dry enough for a fire. We made beds of slender, springy poles placed across two small logs (thank heaven for my air mattress), cooked some rice to eke out our field rations, and dried our soaking clothes over the blaze. Sometime after midnight I awoke to see the sky clear and the ground alight with phosphorescent wood. The forest floor was black as pitch, except for the weird light of the decaying wood; phosphorescent lichens clothed the buttresses of some of the tall trees, overhead, a few small patches of starlight shone through the dense canopy of leaves. I had the enchanting sensation of floating in velvety blackness miles away in space, surrounded by glowing stars.

Borgen Bay
New Britain
April 30, 1944

Dear Miriam:

A few days ago I had a piece of bad luck —for me a near tragedy. We moved from Talasea, on the Willaumez Peninsula, traveling in open boats (LCM's) the distance of some 120 miles. During the night it rained—a tropical downpour—and in addition, the boats shipped quantities of water over the sides. And so my desk was soaked overnight in water. It contained all my papers, stationery, and drafting instruments, as well as personal books, notes, and the plant collection from the Willaumez Peninsula.

Since we landed here I've been busy as a little beaver, drying them out—but you know what happens to a book when it is soaked in salt water for some 15 hours. The bindings are ruined, the illustrations are soggy masses, and I'll have to tie them up with string to keep the pages in. The plant collection is a poor sight indeed; but, thanks to using waterproof ink, the natural-history notes are in good shape. Now I can more fully appreciate what I've read of the difficulties of travel encountered by some of the early botanists and scientists in our own country.

Tomorrow is the first day of May; I hope to "go a-Maying" and collect some of the representative species from the grasslands in this vicinity.

Pavuvu Island
Russell Islands
June 7, 1944

Dear Miriam:

For nearly a month we have been engaged in building a camp in the coconut groves which cover several thousand acres of the northwestern portion of this island. The plantation is quite a pleasant change, after months spent bivouacked in rain forests most of the time. We are located on a narrow peninsula, with small bays on either side which permit the frequent breezes to sweep across our camp, tempering somewhat the humid, tropical climate.

There are no areas of natural grassland on this island, but the coconut plantations have been underplanted with a cover crop of carpet grass (*Axonopus compressus*) and *Rhaphis aciculata*. This is a common practice throughout the Southwest Pacific where the plantations were cleared from rain forest; without such a cover crop objection-

able weeds and shrubs would invade the area before the coconut trees become established. Through the use of a cover crop, and controlling it by grazing cattle, the plantations are kept clean and free of encroaching vegetation.

The range management practices here are quite interesting and vastly different from ranching in the States. About 1,500 head of cattle and a few horses were on this island when it was retaken. The cattle are a mongrel breed, a strain of zebu, or Brahman, being the only apparent common heritage; in addition, some show traces of Hereford, or Aberdeen Angus, or Shorthorn ancestry. Although a scrawny lot, they appear thrifty and are no doubt well adapted to this climate. Since their primary function is to keep the cover crop under control, they have been left to lapse into a semiwild state. There is no evidence of care or handling beyond a few barbed-wire fences to keep them from straying into the rain forest bordering the plantation. With a heavy stand of grass, a year-long growing season, and abundant rainfall, livestock can be left to shift for themselves where the quality of the meat and purity of blood lines are matters of no concern.

Peleliu Island
Palau Islands
October 1, 1944

Dear Miriam:

There has been little opportunity for writing these past few days, as we have been rather busy -or is that an understatement! You will have guessed, and now I can tell you, that I'm in the Palau operation.

About 6 days ago I spent a day on the eastern side of our little island. There has been but little action there; consequently it retains much of its original appearance and beauty. The tiny islets, set in shallow blue-green lagoons, and white sand beaches along the coast look like a city park; it has the most exquisite small-scale tropical scenery I've seen. The white herons in the swamps, with black legs and long yellow bills; sandpipers, and cormorants along the reef; the black-and-white tropic birds, with long, trailing tail feathers—all these heightened the parklike effect.

Peleliu Island
Palau Islands
October 26, 1944

Dear Miriam:

During the past few days our outfit has been back for another inning with the little men. And now our job here is finished. During the 40 days here I've personally toured nearly 75 percent of this coral outcrop, most of it on foot. It is a violent contrast to the places we've previously visited.

Geologically, it is a most recent bit of land. As a matter of fact, the term "land" is practically an overstatement—it is a coral reef that has only recently been raised above water, with a sand bar washed up along the western shore. The soil is everywhere very scanty, slightly weathered, and mixed with bits of broken coral; in many places the trees grow in crevices in the coral, seemingly without soil. The hills retain the configuration of an underwater reef, with steep, ragged ridges, sharp cliffs, and many enclosed depressions.

Originally much of the eastern part of the island was a mangrove swamp, and the southern and western portions had a considerable cover of forest. Much of the vegetation in the south and west parts of the island, especially on the higher ridges, has been denuded by our operations. But the nature of the terrain is such that erosion will present no problem on these coral ridges; in a very few years they will again be clothed with trees.

The settled portions of the island differ markedly from the relatively uninhabited eastern part. Particularly obvious are the meager efforts at cultivation and the lack of anything resembling lawns, gardens, or parks. A very few of the more important

buildings are very poorly landscaped; for example, a circle of bare earth enclosed in a low concrete curbing in the entrance area. These attempts have been unattended for a period of years. There are no lawns--indeed, only one vestige of a suitable lawn grass was discovered no flowers, and no ornamental shrubs. Nor have I seen, or had reported, a single temple, shrine, or any other evidence of a place of worship on this island. This in spite of more than thirty years of occupancy and in contradiction of our ideas that the Japanese expend considerable pains on their gardens, parks, and temple grounds.

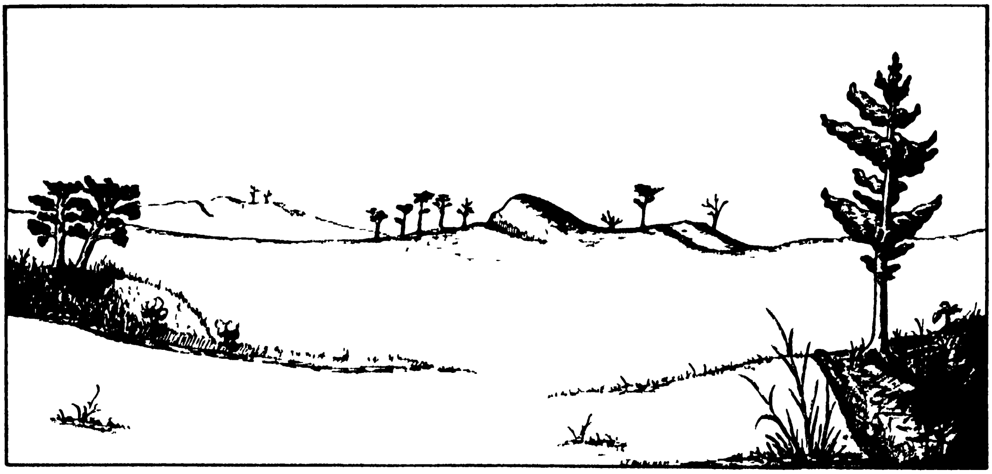
My time spent collecting grasses has been quite well rewarded. From scattered locations about buildings, in the forest, and along beaches I have gathered a total of 15 species, which I believe to constitute a complete representation of the grass flora here. And the improvised plant press I previously mentioned [the pages of a Japanese military manual printed on newsprint] has served very well indeed.

Inubi, Okinawa Shima
Ryukyu Islands
April 14, 1945

Dear Miriam:

Before answering your last letter let me tell you something of my plant collecting this afternoon. I have my plant driers along on this trip, so the specimens can be properly prepared. This being in the Northern Hemisphere, it is now spring; the spring flora is just becoming abundant, and collecting should yield some very interesting specimens. The little orchid mentioned in my last letter was from here.

It is welcome, too, to see a landscape dominated by pine trees instead of coconut palms, although the climate here is considered semitropical. The pines occurring as isolated specimens or in small groups, on coral outcrops and along the cart roads, are mostly gnarled and grotesque, the trees that are so picturesquely depicted in Japanese prints. On the ridges above the cultivated lands occur pure stands of pine woodland.



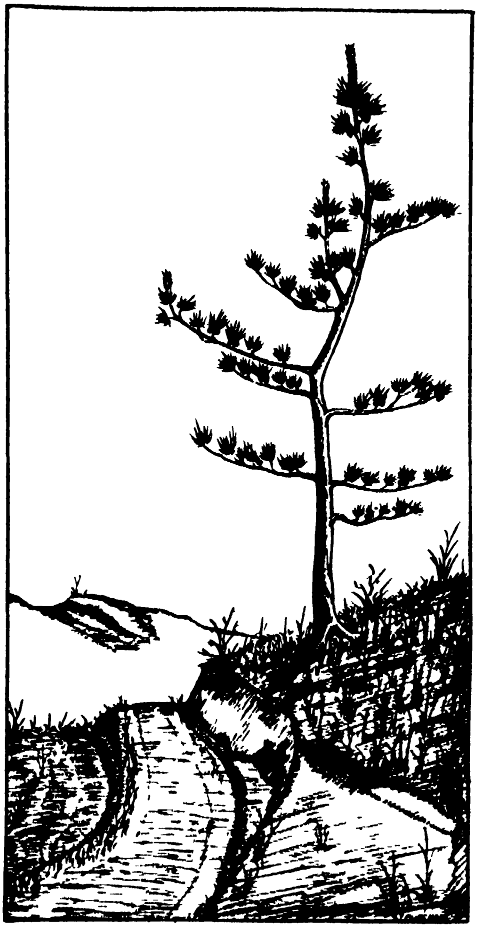
ASATO RIDGE, SOUTHERN OKINAWA

This afternoon I was collecting in one of these pine woodlands, on the ridge east of Inubi, where we are now staying. In many respects it is similar to pine forests at home. There is much evidence of heavy cutting and of considerable grazing; consequently, the stands are quite open and the trees

relatively small. On the lower edge of the woodland was a pit for making charcoal and a stack of poles evidently cut for this purpose; these varied from 4 to 6 inches in diameter and had from 10 to 12 growth rings. In other areas are stacks of firewood cut from the forests; considerable quantities of small timbers were removed from the forests for construction of defenses on the island before we arrived.

The trees in this woodland averaged about 40 feet in height and ranged between 8 and 20 inches in diameter. An understory of grasses, ferns, and low shrubs forms a ground cover that is practically continuous—among them such familiar plants as huckleberry (*Vaccinium wrightii*) and bracken fern. The close usage has destroyed much of the litter from the ground, and in many places the soil is bare and somewhat compacted.

This proved a fruitful area for collecting; I secured an even dozen grasses, many of them not previously encountered in my travels. Besides these I collected other plants typical of the understory of this pine woodland type, to give a representative cross section of the associated species.



PINUS MASSONIANA

JUVENILE SPECIMEN OF THIS PINE ON OKINAWA.

Hanja, Okinawa Shima
Ryukyu Islands
June 22, 1945

Dear Miriam:

For some time I've been intending to write telling something of this island and its people. Like all of Japan proper, and most of the Asiatic continent, the land is densely populated—here about 900 persons per square mile. The people are primarily agricultural; the farms average about one and one-half acres each and are worked on a family basis.

The land is very rough, so the fields are tiny—and extensively terraced to control erosion; the small fields also follow from the prevalence of hand cultivation to the exclusion of everything else. So small, indeed, are some of the fields that I have seen plots barely a square yard in area painstakingly terraced and planted to crops. The intensive hand cultivation, coupled with the need for maintaining maximum productiveness, results in very careful and excellent tillage methods. Crops seem quite good—to all appearances the productivity of the soil would rate better than the average of similar soils in the States. The principal crops are rice, sugar cane, sweet potatoes, soy beans, and wheat; many small plots of garden vegetables are grown for home use—cabbage, carrots, onions, tomatoes, and the like.

This impresses one as an old country, closely cropped, showing much evidence

of human subjugation and careful, painful tending. Typically, it appears a land of small farms, small houses, and small people —small people both in stature and thought. Men have made their mark on the land—unmistakably and unalterably. But one comes inevitably to the conclusion that in return the land has made just as unmistakable and just as unalterable an imprint on the lives of these people. They are subjugated by their land—their small houses, small fields, small farms. Their lives have been so completely dominated by the work of eking out a living from the soil, with the problems of home, family, and self, that they have had no time to think of anything else—nor much time to think at all: no interests beyond their homes; no thoughts beyond their horizon; and no knowledge of things outside the contacts of their immediate lives. Instead of expanding they seem to have grown into their country, their lives, and themselves. Their very civilization seems to have become static.

Motobu Peninsula
Okinawa Shima
July 16, 1945

Dear Miriam:

These lines are being scribbled as I sit with my back against a wind-gnarled pine near the edge of a 70-foot cliff. From here I have a wide view of the reefs, offshore islands, and the East China Sea. There is a fringe of pine and acacia trees along the summit of the cliff—perhaps typifying the queer admixture of vegetation from the Northern and Southern Hemispheres that one finds on this island. Here, too, we find a link with the past—a prominent member of the undergrowth is the palmlike cycad (*Cycas circinalis*), a representative of the most primitive group of present-day flowering plants.

Lately I have been prowling about the field borders, climbing the coral cliffs in front of our camp, and wandering along the rocky seashore in search of grasses to complete the collection from this area. It has yielded a number of interesting specimens. Again I have collected *Thuarea involuta*, the dainty little seashore grass that plants its own seeds, which I first encountered on Pavuvu Island. On coral outcrops along the coast I found *Ischaemum murinum*, a little-known grass that occurs mainly in the Southern Hemisphere.

Throughout the island are many vaults where the natives have interred the bones of their ancestors. Two days ago I found a tall grass, with coarse basal leaves, an erect culm, and gracefully nodding panicle, growing in a handful of decaying pine needles on the top of one of these concrete burial vaults. It was *Spodiopogon sibiricus*, of unusual occurrence on islands south of Japan; this single clump is the only specimen I have been able to find here. How it came to grow in that spot would doubtless be a fascinating story of plant distribution.

THE REDISCOVERY OF CUP BUTTE

By RONALD L. IVES
Upper Montclair, New Jersey

CUP BUTTE, perched atop an ancient gravel bar a mile above sea level, in the center of the Salt Lake Desert, is one of the least-visited geologic features in North America, yet, according to maps and descriptions published by G. K. Gilbert in 1890,* it is one of the most nearly perfect known examples of a looped bar.

The waters of Pleistocene Lake Bonneville, which formed the butte, have long since evaporated, leaving the ancient shore structure high and dry. Because of the aridity of the region, no heavy cover of vegetation has concealed or modified the surface features; and no chemical weathering has occurred to alter the original structure.

During an extensive review of the physiography of the Salt Lake Desert, it was found essential to study the shoreline features of extinct Lake Bonneville. From Gilbert's descriptions and illustration (Fig. 1), it appeared that the orientation of Cup Butte would give a very good indication of ancient current direction, and a visit to the area was scheduled.

Cup Butte is shown on only two published maps, both included in Gilbert's monograph. On the larger of these, the structure is shown at Lat. 39°54′ N.; Long. 112°47′ W., Alt. 5,200′ m.s.l. The smaller, a detail map of the "Old River Bed" (here called the Gilbert River Bed), by W. D. Johnson, shows Cup Butte about 5 miles

*Gilbert, G. K. "Lake Bonneville," U. S. Geol Survey Monograph 1, Washington, 138, 169, 372, 412, 419, Plate VI, 1890

FIG. 1. SKETCH OF CUP BUTTE
AS IT WOULD APPEAR FROM A POINT ABOUT 1,500 FEET IN THE AIR ABOVE THE ANCIENT GILBERT RIVER BED. THIS FIGURE, FROM GILBERT'S REPORT, HAS A VERTICAL SCALE ABOUT 2.5 TIMES THE HORIZONTAL.

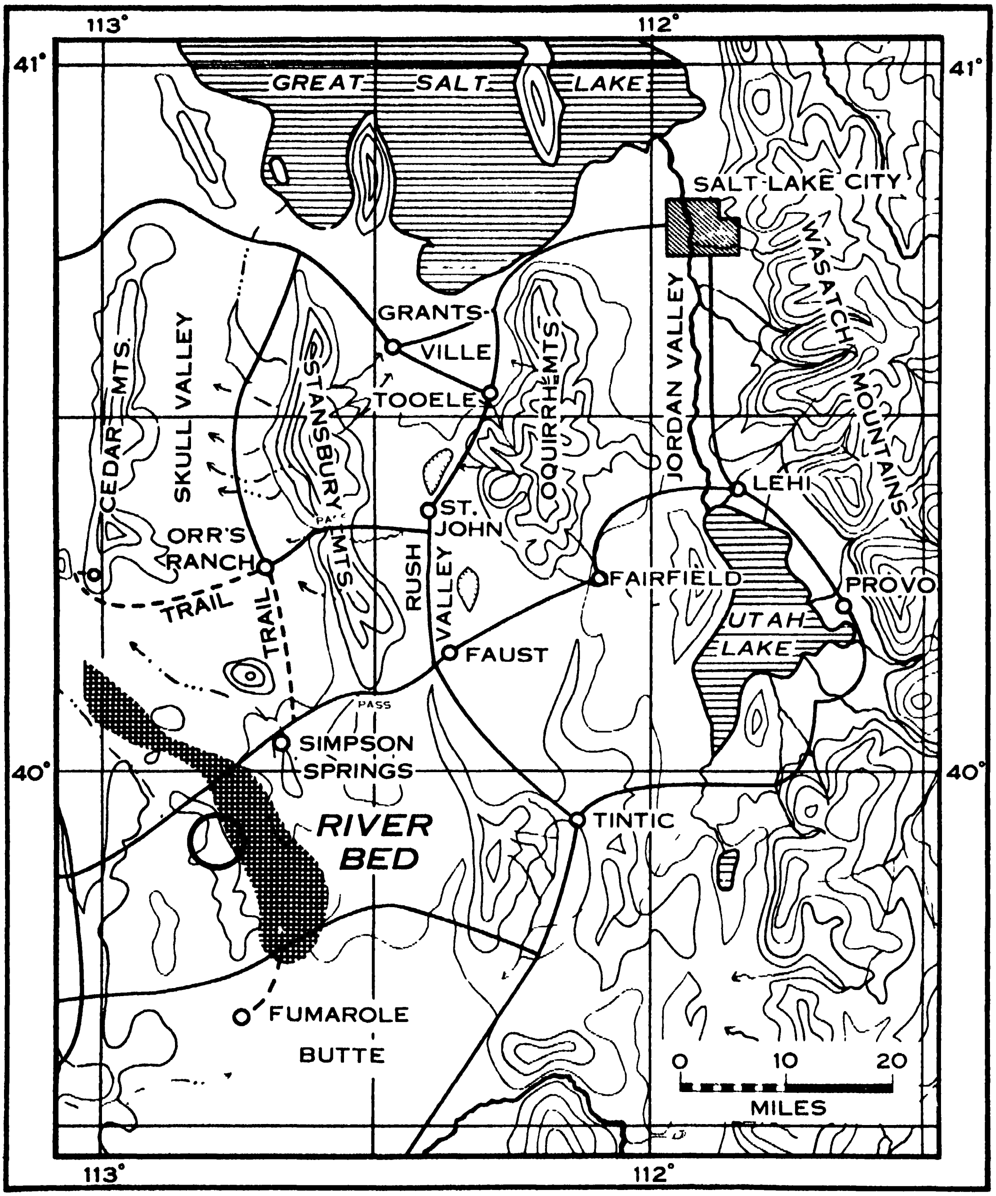


FIG 2. SKETCH MAP OF A PART OF THE UTAH DESERT AREA

SHOWING LOCATION OF THE GILBERT RIVER BED (CROSSHATCH), AND THE AREA WITHIN WHICH CUP BUTTE WOULD PROBABLY BE FOUND CONTOUR INTERVAL IS ABOUT 1,000 FEET, LOWEST CONTOUR 5,000 FEET.

south of the River Bed stage station, 7 miles west-northwest of the Bonneville shoreline above the "Snowplow," and 11 miles southwest of Simpson Springs. Gilbert (p. 55) locates and describes the structure as "an island of Lake Bonneville standing on the desert near what is known as the 'Old River Bed.'"

The "Old River Bed" was a definitely recoverable site, as was Simpson Springs, site of a former CCC camp; but the River Bed stage station has long since disappeared

FIG. 3 RESERVOIR BUTTE, ON THE WEST SIDE OF THE GILBERT RIVER BED

THIS IS ONE OF THE MOST FAMOUS SHORELINE STRUCTURE SITES IN THE WORLD, A BAR HAVING BEEN BUILT ON THE FLANK OF THE MOUNTAIN AT EACH STADIUM OF PLEISTOCENE LAKE BONNEVILLE. THIS PHOTOGRAPH, TAKEN FROM THE SOUTHEAST, ALMOST EXACTLY DUPLICATES GILBERT'S SKETCHED PLATE XXIV.

and has been replaced by a historical marker on the presumed site. From the geographic descriptions and Gilbert's sketch (Fig. 1), it appeared that Cup Butte was somewhere within a 5-mile circle, on the west side of the River Bed, south of Reservoir Butte (Fig. 3). The location of the search area, in relation to major features of the Utah Desert area, is shown in Figure 2.

Inquiries among local miners and ranchers disclosed that few had ever heard of Cup Butte and that several were very certain that there was no such feature near the River Bed "or anywhere else in the world." In contrast, Mr. Daniel T. Orr,† pioneer rancher of Skull Valley who had once worked for Gilbert, declared most emphatically that if Gilbert had described the feature, it was there.

†A most helpful and dependable informant. The trilobite *Orria elegans* was found by, and named for, Mr Orr.

Because of the dependability of Gilbert's work, we decided to disregard the local negative information and, with Gilbert's maps and description as a guide, to attempt to find Cup Butte.

Preliminary to detailed ground studies, an aerial reconnaissance of the River Bed and the adjacent mountain region was made, with the skilled pilotage of the late George Langham, Captain, A.C. This study indicated that Johnson's map of the Old River Bed (Gilbert, *op. cit.* Plate XXXI, opp. p. 182) was at least substantially correct, although somewhat distorted because of a local magnetic attraction (probably caused by buried volcanic materials). Cup Butte was not seen from the plane, owing to "bad seeing" and the rough air characteristic of desert mountain regions.

Following the aerial study, a jeep traverse of the Old River Bed was made in company with the late Master Sergeant De Vore, M.C.

During this journey, on which the jeep was taken where nothing on wheels had ever been before, Reservoir Butte (Fig. 3) was identified, Gilbert's "Snowplow" structure was found, and Fumarole Butte (Fig. 2), with its accompanying hot springs, was visited. Cup Butte, however, remained unfound.

To narrow down the search area, a visual reconnaissance was made from the summit of Reservoir Butte, a weird combination of a desert mountain and a group of shoreline structures (Fig. 3), which overlooks almost all the ancient shoals and shorelines of Lake Bonneville in this general vicinity.

Although these visual observations were made very difficult by bad seeing, resulting from the "boiling air" common to the desert, they showed conclusively that if Cup Butte existed, it was in a small area atop an ancient shoal, defiladed by a quartzite butte southeast of Reservoir Butte. This remaining search area, approximately 1.5 by 2.5 miles in extent, was centered about a point 3 miles southeast of Reservoir Butte. Cup Butte—if present, should be visible from Bar Hill, another high point, about 5 miles south-southeast of Reservoir Butte.

Accordingly, when circumstances permitted, a jeep ascent of Bar Hill was made from the south, the chosen trail winding back and forth over a series of ancient gravel bars built across the mouths of local valleys during various stages of the ancient lake.

From the northeast side of the summit of Bar Hill, the ancient shoal area which could not be seen from the top of Reservoir Butte was plainly visible. These former shoals are now a mesa, flanked by 35° angle-of-rest talus slopes. Through the desert air, boiling despite the zero temperature of early February, a small knoll of gray rock, shaped like the "horn" of Cup Butte (Fig. 1), was dimly visible on the extreme southeast end of the highest

FIG. 4. VIEW NORTHEAST FROM BAR HILL

SHOWING ANCIENT BARS AND SHOALS WEST OF THE GILBERT RIVER BED. HIGHEST LEVEL CORRESPONDS TO THE BONNEVILLE SHORELINE; CENTER LEVEL IS THE INTERMEDIATE; LOWEST LEVEL IS THE PROVO. THE SIMPSON MOUNTAINS ARE IN LEFT DISTANCE; THE GILBERT RIVER BED IS IN MIDDLE DISTANCE. THE SUSPECTED LOCATION OF CUP BUTTE (LATER FOUND CORRECT) IS AT THE RIGHT END OF THE HIGHEST TERRACE.

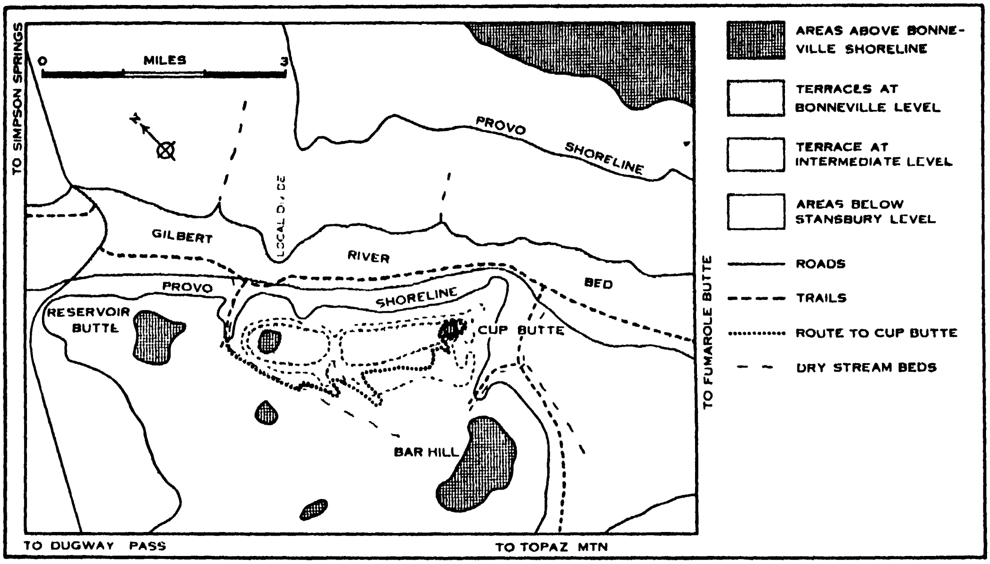


FIG 5. SKETCH MAP OF THE GILBERT RIVER BED AREA
SHOWING LOCATION OF RESERVOIR BUTTE, CUP BUTTE, BAR HILL, AND THE ROUTE TAKEN TO REACH CUP BUTTE.

terrace level. If Cup Butte existed, this was it! A general view of the area comprises Figure 4. All that now remained was to find a trail to this structure.

Topographic study from the summit of Bar Hill showed that the best route to the supposed location of Cup Butte was up a dry wash to the west of the shoal area and that this route was certain to be rather difficult.

Using a jeep which had been specially "tuned up" for climbing, we followed the River Bed trail from the Simpson Springs —Dugway Pass Road to the highest point in the Gilbert River Bed—and a branch trail up the dry wash to the west. From the end of this trail, the dry stream bed was followed for about a mile (Fig. 5), and then a tributary wash for about 1,500 feet. By making a number of switchbacks, the intermediate terrace level, about 200 feet below the Bonneville, was reached with no more difficulty than was experienced in climbing Bar Hill, although the course was somewhat devious because of a random arrangement of large beach boulders at several locations. From the intermediate level to the summit, however, it appeared that the 35° boundary slope of the Bonneville terrace, composed of loose, angular talus, would have to be climbed. This, theoretically, was beyond the ability of a jeep.

Almost directly below the supposed location of Cup Butte, at the edge of a steep "drop off," one area was found in which the talus seemed to be quite firm. After several attempts to climb this slope in "low-low" and "four-wheel," the pulling power of the jeep was increased slightly by disconnecting the fan and generator, and the traction by partly deflating the tires. On the next attempt, the jeep shuddered onto the flat surface of the Bonneville level of shore deposits. After reconnecting the fan and pumping up the tires, we reached Cup Butte by threading a maze of boulders.

Locations of principal topographic features, roads, trails, and the route followed to Cup Butte are shown in Figure 5, a sketch map based on W. D. Johnson's (Gilbert, Plate XXXI, opp. p. 182). A view of Cup Butte from the south, showing the "horn," and with the jeep on the rim to give scale, comprises Figure 6.

FIG 6 CUP BUTTE, AS SEEN FROM THE SOUTH RIM

THE INTERIOR OF THE BOWL IS ABOUT 250 FEET LONG, 175 FEET WIDE, AND 40 FEET DEEP. THE "HORN" IS ABOUT 30 FEET HIGH THE JEEP SHOWN ON THE RIM OF THE CUP INDICATES THE APPROXIMATE SCALE.

BECAUSE of the probable importance of Cup Butte in many investigations of ancient Lake Bonneville, two additional visits were made to the site in an effort to secure accurate location data.

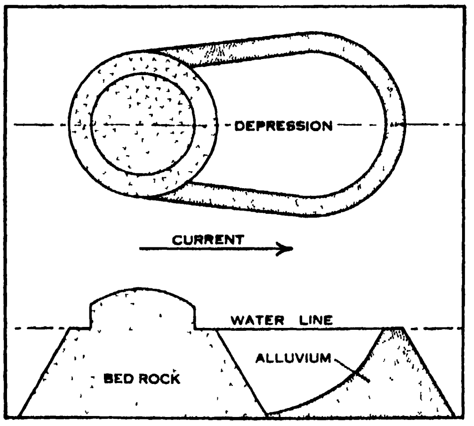


FIG 7 A LOOPED BAR

UPPER FIGURE IS A PLAN VIEW, WITH THE SECTION PLANE PASSING THROUGH THE WATER LINE. LOWER FIGURE IS A VERTICAL SECTION, WITH THE PLANE PASSING THROUGH THE AXIS OF THE STRUCTURE.

By triangulation, Cup Butte was found to be 9.33 miles S.37°W. of the Simpson Springs historical marker, the probable error in this measurement, after curvature and refraction errors, being 0.1 miles and one degree of arc.

By corrected aneroid barometer, the following altitudes were determined: Top of horn, 5,240′; rim of cup, 5,210′; bottom of bowl, 5,170′; all altitude differences being correct to less than 5 feet and all being above mean sea level, with a probable error not exceeding 25 feet.

Determination of geographic position by means of a sun sight was not possible because of boiling air. Position as given by the average of a number of star sights, taken after midnight in March 1946, indicates that the latitude of Cup Butte is 39° 56′ N. and the longitude 112° 48′ W.; both determinations having a probable error of one minute of arc, owing in large part to atmospheric unsteadiness and in lesser part to lack of refraction data for a

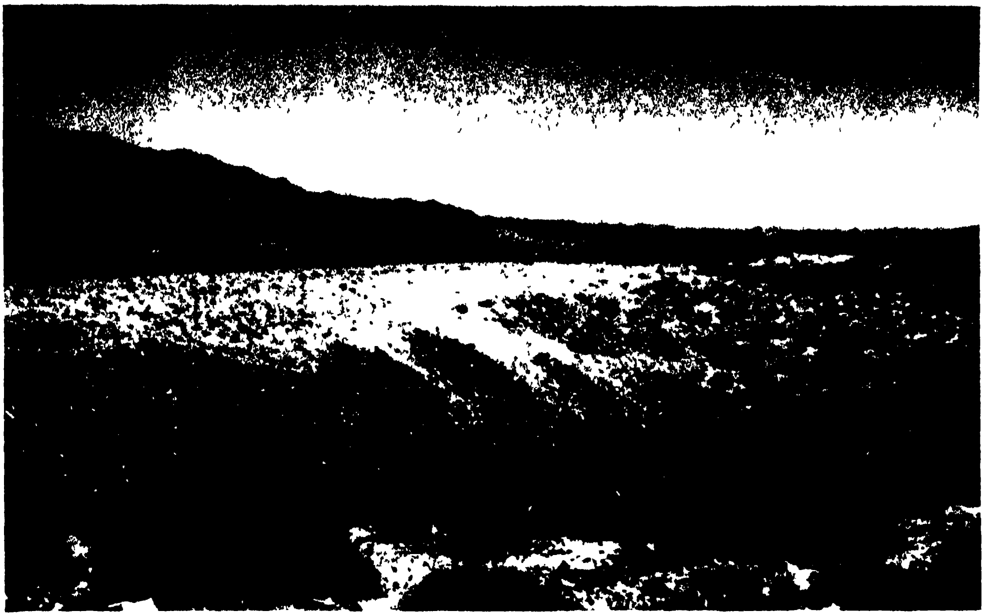


FIG 8 EAST WALL OF CUP BUTTE, AS SEEN FROM THE "HORN"

SHOWING THE TWO SEQUENCES OF DEPOSITION MATERIAL ON THE FLOOR OF THE CUP, AND FORMING THE RIGHT (SOUTH) WALL, IS COMPACTED AND WEATHERED, WHILE THAT IN LEFT CENTER IS LOOSE AND SUBSTANTIALLY UNWEATHERED THIS LOOSE MATERIAL FORMS A THIN VENEER OVER THE OLDER MATERIAL

high point a mile above sea level. Some discrepancies in the locations computed suggested atmospheric stratification.

Magnetic north, atop the butte, was found to be 22° east of true north as determined by a star sight. Several other determinations indicate quite plainly that there is a local magnetic attraction in the Gilbert River Bed area, probably most intense in the vicinity of buried volcanic structures ("Local Divide," Fig. 5).

Corrected bearing of the center line of Cup Butte is 322°. This indicates the direction from which the formative currents came and, with slightly less accuracy, the source of the resultant dynamic wind during Bonneville time. Modern observations, made in an area on the delta of the Gilbert River, about 15 miles northwest of Cup Butte, indicate that the resultant dynamic wind now comes from about 309°‡. The difference of 13° between ancient current direction and modern wind direction is not regarded as significant in view of the geographic separation of the points of observation. A statement that the source of the resultant dynamic wind has not changed since at least the later Pleistocene seems entirely justified.

According to generally accepted theories of bar formation, a looped bar, such as Cup Butte, will be formed when a debris-laden current impinges on an obstacle of relatively small dimensions so that, after parting, the current passes on both sides of the obstacle. Debris is deposited at the margin of the slack water behind the obstacle. The general structure of Cup Butte is exactly as called for by this theory.

A generalized section of a looped bar, slightly idealized for clarity, comprises Figure 7. Similarity of this figure to

‡This figure is an average of two determinations and is in general accord with wind-constructed topographic features in the same area Ives, R. L. *Amer. J. Sci*, **244**, 492-501, 1946.

Gilbert's sketch (Fig. 1) and the text illustrations (Figs. 6, 8) is marked.

Detailed study of the deposits comprising the walls of the cup indicates that construction was a "two-step" process, the major part of the work being relatively old and present as compacted and weathered bar material. Over this was deposited a thin layer of loose, angular rock fragments, which appear relatively much newer than the material forming the major part of the butte (Fig. 8).

From this evidence, which is also present elsewhere, it is concluded that the waters of Lake Bonneville rose twice to the level of Cup Butte, remaining at that level for a relatively long time during the first rise and for a very short period during the second. Comparison of deposits in this area suggests that the first rise was that usually classed as Bonneville and that the second was approximately contemporaneous with the Provo stage.

Field checking of Gilbert's description of Cup Butte shows that his work, done more than 60 years ago, was as accurate as the state of the art permitted. Although modern methods of observation and transportation accelerate field work somewhat, the only great improvement in surface geological field methods since 1885 seems to be the general use of photography.

In view of the chronic bad seeing in the Salt Lake Desert and the paucity of topographic control points in 1885, the accuracy of Gilbert's cartographic work is notable.

Although Cup Butte, because of its isolation, will probably be visited very infrequently, this excellent example of a "fossil" looped bar can be located with very little difficulty from Gilbert's original maps and descriptions. During the early part of the winter of 1945–46, Lieutenant Colonel P. A. Leighton, C.W.S., located and ascended Cup Butte, using only these data. With the additional trail and location data here presented, Cup Butte should be reachable, on first attempt, by any competent field worker.

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IMPRESSIONS OF JAPANESE MEDICINE AT THE END OF WORLD WAR II*

By RICHARD B. BERLIN†

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AMERICAN Occupation Forces who arrived in Japan shortly after the surrender were afforded an excellent opportunity to observe the defeated enemy about whom we had heard so much and to evaluate some of the concepts which were formed during the conflict. Curiosity was especially high among American medical officers concerning the state of Japanese medical practice and the effect which the war had upon it. The remarks which follow are based upon impressions received during 16 weeks on the Island of Kyushu between September 1945 and January 1946. Six weeks of this period were spent supervising the activation of a Japanese hospital in Nagasaki. This meant close daily contact with native physicians, nurses, and civilians who were making the first attempts at revitalizing a profession which had been terribly crippled by the catastrophic events of preceding years.

Until recently, Nagasaki possessed a major medical center and boasted one of the finest medical schools in Japan. Its buildings were handsome, modern, and built to withstand earthquakes. The concrete frames of these buildings now stand alone on a vast expanse of rubble. They were about two-thirds of a mile from the center of the atomic bomb blast. Eighty percent of the 800 persons in the buildings were killed outright, including all but 4 of the medical professors and most of the medical students, nurses, and patients.

* The opinions expressed are those of the author and not of the Navy Department

† The author wishes gratefully to acknowledge the aid of Lt Walter D Roberts (MC) USN in gathering material for this paper

BOMBED AREA SEEN FROM KYUSHU MEDICAL SCHOOL

JAPANESE TECHNICIAN
IN THE TEMPORARY HOSPITAL IN NAGASAKI.

Most of the deaths were caused by the heavy plaster ceilings, which were pulled down by the blast. The odor of decaying bodies was still heavy in some of the rooms 4 months after the bomb fell.

Judging from the equipment which remained in the ruins of the large medical center, its physical plant would have compared favorably with most modern Western institutions. There were separate buildings for dermatology, gynecology, ophthalmology, neuropsychiatry, etc. Each department had several laboratories and showed evidence of active research and teaching programs There were many wax and plaster models and large collections of medical photographs. Many gross and microscopic specimens were also found in the ruins.

With the loss of its principal medical center, Nagasaki was virtually without hospital facilities until the arrival of the American forces. Consequently, victims of the atomic bomb received inadequate care or no medical attention at all. The Japanese had converted one school building into a hospital for their treatment and study prior to our arrival, with a staff composed of survivors of the medical school faculty. This was probably the best hospital in the area.

In December 1945, four months after the atomic bomb, Japanese authorities told us that there were about 300 hospital beds in the Nagasaki area, divided among some 30 small hospitals. This would provide 1 bed for each 500 of the population. Although some of these hospitals were quite acceptable, most of them were in small houses of mud and bamboo that were hardly worthy of the term "hospital." About all they offered was the sympathy of a physician and a space on the floor to rest.

One of the larger institutions was the women's venereal disease hospital of about 50 beds, the "beds" consisting of straw mats on the floor. There were no other furnishings in the rooms, and 6 to 8 patients were crowded into an area which was adequate for about two. Each patient had her few belongings piled in one corner of the mat which covered her area. The rooms were dark, filthy, and fetid. Treatment rooms were littered with ancient instruments and old, dusty bottles. Aseptic technique was not observed, and instruments were sterilized by dropping them into a caldron of water boiling on a charcoal stove. One gynecologist was in attendance. Medications which were available were very inadequate.

At Omura, which is about 25 miles from Nagasaki, there was a large naval hospital built for temporary wartime use. This was excellently equipped for about 2,500 patients, but was not in use because of the lack of personnel and the state of general confusion which prevailed. In Sasebo, about 75 miles north of Nagasaki, were two naval hospitals of a combined capacity of six or seven hundred patients. These hospitals were also idle.

In order to relieve the chaos, and also as part of the demobilization program, Japanese Army and Navy medical stores and equipment on western Kyushu were pooled and brought to Nagasaki for distribution. The prefectural government was instructed to provide a suitable building and to hire the required labor and staff for the establishment of a hospital in the shortest possible

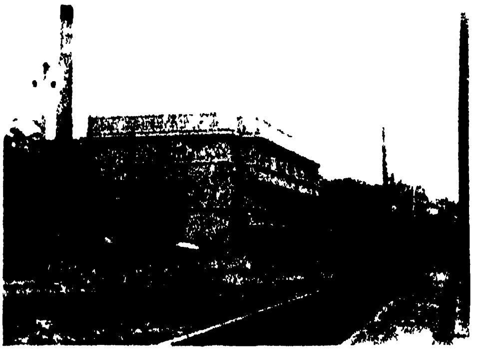
time. Though the projected hospital could not hope to care for more than a token number of the needy, it would be a step in the right direction.

A modern concrete building in a section of the city which had not been damaged by bombing was chosen for the site. The hospital was to contain about 150 beds and meet American standards. In the course of 3 months it was converted into an effective hospital. Japanese interpreters told us that, if left to their own methods, the same project would have taken a year to complete. American prodding was needed continually to initiate and carry out this project; the fact that it was intended solely for the benefit of the Japanese did not seem to be an inducement for them to work on it.

Most of the building-trades men were highly skilled, though their tools and methods were antiquated and everything was done by hand. Evidence of wartime shortages was ever-present and was emphasized by the skills which workers had developed. For example, the carpenters were able to work with few nails; they also substituted a string coated with lampblack to mark their work instead of using a pencil. Their tools were always very keen, and their work tidy.

Women and children, as well as men, are employed in the building trades in Japan. When it was necessary to cut a space for a door through a heavy reinforced-concrete wall, the task was assigned to an old woman of about seventy years and a boy of twelve. Both thumped with a mallet and chisel for 2 weeks while all other work in the vicinity was held up because of the dust they created. Two men could have done the job in 3 days.

A great deal of equipment was salvaged from the ruins of the medical center; operating tables, electric refrigerators, surgical lamps, furniture, and sinks were found in good condition. Though all these things had been in the ruins for 4 months and permission had been granted to remove what was needed, the Japanese made little effort to do so until specifically ordered. Occasionally one would see a solitary medical student wandering through the ruins of his school looking for something to take home.



EMERGENCY HOSPITAL

FOR THE JAPANESE UNDER AMERICAN SUPERVISION

The beds for the hospital were procured from the Omura Naval Hospital These were sturdy Western-style wooden beds with thick straw mattresses. The bed linens were made from bolts of airplane cloth from a nearby airfield.

The water supply came from the city water mains. Nagasaki had been one of the few cities in Japan with a modern water purification system that had not been seriously damaged by the war. Repeated tests showed the water to be satisfactory. However, experience made us hesitant about relying upon its potability. For example, it was usual to be able to culture all the common bacterial enteric pathogens from the city water of Sasebo.

Each floor of the hospital building had one hot-water heater in the hallway. This consisted of a tank which contained coils attached to the central steam-heating system. Since there was no coal for the furnaces during most of the winter, there was no hot water.

The hospital kitchen was light and spacious and was lined with white tile. The cooking was done in large pressure caldrons heated from the central heating plant. The diets were very meager since no special

foods were available for hospital patients. The diet consisted of rice, a few greens, some citrus fruit, and a little fish. On the rare days when fish was obtained, its preparation forced the American occupants to leave the building in search of fresh air.

THE Japanese Army and Navy medical supplies were of considerable interest, although language difficulties prevented identification of all the items which were seen. Military medical equipment was usually packed in sturdy wooden packing cases with cloth or leather handles on the ends and a hinged top. Some of them were adapted for transport on horseback. Within the outer cases, there were highly polished and varnished smaller boxes with many compartments. These appeared to be handmade and were fitted with brass corners and elaborate hardware. After opening several boxes within boxes, one would finally find a small object such as a glass funnel or test tube.

Each box was prominently marked with a Red Cross. This was striking in view of the fact that such markings on Allied equipment in the Pacific combat areas were not only disrespected by the Japanese but rated a high priority as targets. Some of the cases were made of heavy leather and lined with wicker. There were many test-tube racks, alcohol lamps, bacteriological stains, and small kits of surgical instruments in aluminum containers, mostly of Japanese manufacture.

Most of the surgical instruments were plated with a metal resembling chromium, which was often chipped and ravaged by rust. The tooling was inferior, and only a small percentage of the hemostats and similar objects were in workable condition. A few German instruments were seen which were decidedly better. Such instruments as microscopes, cystoscopes, and sigmoidoscopes were made in Japan.

There were portable X-ray machines which were very compact and reported to be very efficient. The principal unit came in a canvas case the size of a bowling ball and weighed about 50 pounds. We were told that these were dental units but that

KYUSHU MEDICAL CENTER
THESE BLASTED BUILDINGS ARE SITUATED ON THE EDGE OF THE AREA DESTROYED BY THE BOMB

BOMB DAMAGE TO THE KYUSHU MEDICAL CENTER

they were frequently used for other work as well.

A large X-ray machine was procured for the hospital, but, like so many other objects, it had one important part missing, so it was useless. No one could locate a tube for it. Finally several were obtained—they were the wrong size. An excellent darkroom was set up; it had one essential chemical solution missing, so no work could be done there either. Several packages of X-ray film, sealed and marked with Japanese manufacturers' labels, were procured. On opening them and examining the film it was seen to bear the mark "Property of the U. S. Government- Eastman Kodak Company."

We also came across some large, complex, cumbersome machines that resembled small steam engines. We learned they were formaldehyde vaporizers and were used to spray the air in a sickroom.

The most plentiful object in all the stores was the Westergren type of erythrocyte sedimentation apparatus. There were thousands of the tubes and several large truckloads of the wooden racks, most of them used. We learned that the sedimentation rate is a highly favored procedure and is widely used by the Japanese doctors. Readings are made after 1, 2, and 24 hours.

Most of the commonly used pharmaceutical preparations which were seen in the United States before 1940 were found in the warehouses. These were mostly Japanese brands Labels were in Japanese and Latin or English. Many of the drugs were packaged in gaily colored glass ampoules. These varied from a fraction of a cubic centimeter to a 500-cc capacity. It was not possible to learn what all of them contained.

Morphine was packaged as a white powder in cork-stoppered bottles of about 30-gm. capacity There were many vitamin preparations in the form of pills, capsules, ointments, or combinations with other drugs.

There was a granular white powder resembling calcium chloride which came in yellow plastic containers This was found in large quantities and was intended as a universal protective agent against war gases. There were also large kits with complicated

apparatus and pictorial instructions for using them in combatting chemical warfare agents. There were thousands of well-made gas masks of conventional design.

Sulfonamides were found in fair quantity, but the Japanese doctors had little faith in their own brands and would much prefer to try the American products if given the opportunity. They were also a bit uncertain about the indications and contraindications for these drugs.

Penicillin came in small black paper packages and was prepared in two forms. One was a small green pellet of compressed powder for oral administration, and the other was for parenteral use. The doctors told us that there was very little of this drug available and that it was distributed to the Army principally for morale purposes as they had heard a great deal about its use by Americans. When asked about the efficacy of the Japanese product, one of the doctors laughed and shrugged as he replied, "No gutt." The Japanese apparently had no means of refrigerating such perishable products.

There were few biological preparations other than some diagnostic antigens and cowpox vaccine. The cowpox was dispensed in 10- to 30-cc. quantities in bottles. No insulin or liver extract was found.

Some difficulty was encountered in establishing a laboratory for the hospital since no experienced technicians could be obtained. The Japanese physicians were accustomed to performing their own laboratory tests at the bedside, and, if not watched, all the equipment which was brought to the laboratory would soon find its way to the wards throughout the building. This caused much loss of time and duplication of work.

In order to remedy this situation, a young physician was hired to take charge of the laboratory. He was the son of a distinguished professor of medicine and had spent a year in the Imperial Navy after completing medical school. His training had been shortened by the accelerated program during the war, and he felt that it was not up to par. He learned the laboratory work very rapidly and soon trained a young girl as an assistant.

Marble table tops, sinks, centrifuges, incubators, and the like were brought from the atomic bomb ruins. An electric refrigerator, which was probably one of the very last ones in existence in the city, was placed in the laboratory. It was soon realized, however, that there was nothing to put in it.

The bulk of the work submitted to the laboratory in its first weeks of operation consisted of bacteriological smears, urine, and blood for sedimentation rates. It was very difficult to keep the laboratory clean because of the reluctance of the Japanese workers to dispose of old specimens. These would be found scattered about the floor in spite of repeated criticism. Cleansing of glassware was hindered by the lack of soap and hot water.

In Japan, the physician is a respected and important member of the community. In spite of this, his clothing is now almost as tattered as that of the poorest peasant and his diet has the same deadly monotony. He wears remnants of his Army or Navy uniform with a metal pin on the cap to identify his profession. He walks to work or, if he is fortunate, pedals a biycle. Though the prefectural government had several automobiles, the only motor vehicles in the hands of local medical people were two motorcycles which belonged to the two gynecologists. The hospital was unable to procure an ambulance.

Five physicians and 3 interns, all graduates of the Nagasaki Medical School, comprised the staff of the hospital. Most of them had been second lieutenants in the Army who had been on overseas duty. They were an industrious group and exhibited the attitude of sympathy which identifies their profession elsewhere in the world. They were pitifully ignorant of

recent development in the world of science and immensely curious about American methods and equipment. Even the older ones showed no hesitancy about coming to a young American doctor with questions, much as a student consults a professor. No medical literature dated later than 1935 was encountered. However, these physicians were familiar with most of the standard American textbooks of medicine and surgery published before that time.

SHORTLY after work on the building was started, prospective patients began to gather at the front door, and it was decided to open several out-patient clinics while the construction was still going on. It soon became necessary to place a few of the more serious cases on the wards for treatment. Thus, by the time the hospital was ready to open, it was well filled with patients.

In view of the exceedingly bad food situation, families were permitted to bring food to the patients. This usually consisted of some rice and a few tangerines. It never was difficult to keep the patient's fluid intake at a high level as there was always a pot of hot tea at the bedside.

When a young child or its mother was a patient, both remained in the same bed. If there were six patients in a room, one would always find at least that many friends and relatives in spite of vigorous attempts to restrict the hours of visiting. This gave the place the appearance of a family hotel. Since the heating system was not functioning most of the time, patients brought heavy quilts to the wards with them.

The contagious rooms were enclosed in glass. Outside of the door was a clothes tree with surgical gowns and a table with a pan of antiseptic solution. The effect of these was principally psychological. When an infant was ill and its mother in bed with it, the task of indoctrinating her into contagious technique was a formidable one.

In the first 15 days there were 800 patients in the clinic. They were charged fees based on a complex point system. Each therapeutic procedure was given an arbitrary point value which was multiplied by a conversion factor to determine the bill. The conversion factor was subject to daily fluctuations as economic conditions changed. The highest point value was assigned to the performance of a *corpus callosum* puncture. This was worth 500 points. Simple dressings would average 3 to 10 points. Several hundred major and minor procedures were thus listed.

The first 800 patients paid a total of 271 yen. At that time, there were 15 yen to the American dollar. A carton of cigarettes sold on the black market for 125 yen, and a respectable kimono was priced at 500 to 1,000 yen. The doctors in the hospital were paid a salary of 600 to 1,000 yen per month.

The surgical clinic provided a vast amount of pathological material. The doctor in charge labored long hours, as did the interns who assisted him. Nonambulatory patients were brought in on stretchers of bamboo or on small two-wheeled carts. There were no agents available for general anesthesia, so local anesthesia with procaine was used for the only procedures that could be carried out. During this period, one often wondered what became of those unfortunates who developed acute surgical emergencies such as perforated ulcer or appendicitis.

For a surgical scrub, the surgeon used a small enamel basin with a few ounces of medicated soap and a piece of gauze to rub his hands. After several minutes of this, chlor-cresol solution was rubbed in. An abbreviated surgical mask was worn continuously by those who worked in the clinic. The idea was apparently to protect the wearer of the mask rather than the patient. All surgical dressings were impregnated with acriflavine, a very popular drug.

We watched for evidence of the stoicism

which is reputedly a Japanese characteristic. Only one example was seen in the clinic: a young man who underwent a finger amputation under rather unfavorable circumstances. Japanese children were noisy and afraid of the doctor, much as children are elsewhere in the world. Those treated by Americans in a large charity hospital in the United States were judged by the author to be much more stoical than their young counterparts in Japan.

Failure of the steam fitters to produce steam in the large autoclave which had been procured for the hospital necessitated improvisation of other methods of sterilization. Small objects were sterilized by the Japanese by boiling, and larger objects such as trays were "sterilized" by swabbing them with a flaming alcohol sponge.

The instruments which the physicians used were often in need of repair and sharpening. This brought to mind a contrast between their equipment and that of the carpenters the carpenters took time to sharpen theirs daily. Silk sutures were used exclusively; no catgut was available, and cotton was very scarce.

MANY victims of the atomic bomb came to the clinics. Their treatment consisted principally of simple surgical dressings. The wounds during this period (4 to 5 months after the bomb) were granulating third-degree burns, remarkably dry and free of infection in spite of extremely poor hygiene on the part of most of the patients. Many patients bore dense, disfiguring scars, and contractures of the extremities were beginning to present themselves. None of the victims received plastic surgery, nor were attempts at skin grafting made. The injuries resembled those of common thermal burns, although some were pigmented as has been observed following excessive X-ray exposure. There was some alopecia, but at this time most of the victims had new hair growing in the injured regions.

The attitude of civilians toward the bomb was difficult to discern. It often appeared that their only response was one of admiration of such a magnificent weapon. There was no outward exhibition of bitterness or animosity, and those who had escaped injury spoke freely of the spectacle and even joked about it.

Treatment procedures which were carried out in the clinics were orthodox for the most part. Some methods were unusual, however. The following is a summary of some of the things which were observed.

A comatose male with a severe basilar skull fracture was treated with artificial respiration and application of camphor in oil to the scalp locally. An old compound fracture of the tibia with nonunion and a warm fluctuant abscess with tense red skin overlying it was given local applications of sulfanilamide ointment containing vitamins A and D.

Pyogenic infections of the skin, which were very common, were dressed with mercurochrome and acriflavine gauze. A massive cold abscess of the subcutaneous tissues of the back was incised and drained, and the patient made ambulatory. Weeping eczema of the scalp was treated with boric acid ointment, followed by sulfonamide ointment.

Psychiatric disorders were infrequent. Such patients were given barbiturate sedation and were sent home under the care of a relative or friend. Coronary thrombosis was recognized as a clinical entity and was treated with digitalis and bed rest. Diabetes was not observed in the clinics. The internist had seen only about 50 cases of the disease during the past 15 years.

There were many upper respiratory infections. The Japanese physicians showed no hesitancy in diagnosing diphtheria on physical examination only. On several occasions the American forces became concerned about the number of cases of diphtheria in the native population, but repeated bacteriological examinations did not confirm the diagnosis in most instances.

Burns were treated in much the same manner as other superficial injuries. There was an abortive attempt at debridement, and various ointments were applied. The patients were filthy and usually returned with their dressings in tatters. No parenteral therapy was seen even in the more serious cases. Despite all this, the burns healed well.

There were thousands of 500-cc. ampoules of Ringer's Solution in the medical warehouse. This was the principal agent used for the treatment of shock and was said to be an expediency brought on by the scarcity of prepared human plasma. Plasma was used by the Army for severe cases only and was reserved for use on the battlefield in the tropics. The Japanese doctors said that they used Ringer's Solution extensively and found it efficacious, although they admitted that it was not as satisfactory as plasma. They also used it to prevent dehydration but rarely gave over 500 cc. per day even in cases of severe dysenteries.

Blood was administered on the battlefield by direct transfusion of 50-cc. quantities, using a syringe. Another method of so-called transfusion, which was observed in Japanese hospitals, was the removal of about 10 cc. of blood from the arm and injecting it into the buttocks intramuscularly. This was one of the methods used to treat the anemia of victims of the atomic bomb.

The nurses who assisted in the clinic were very helpful and appeared to be well trained. They were cheerful and worked quickly and quietly. They lived on one of the wards of the hospital and cooked their meals over a charcoal stove in the back yard.

It soon became evident that a number of serological tests should be done by the hospital. Since there was an ample supply of antigen for the Murata test, it was tried in order to compare it with the Kahn reaction with the hope that it could be used routinely.

The Murata test is a ring test for syphilis which is widely used by the Japanese and is a modification of the Sachs-Georgi reaction. It is a rapid and simple technique but in our hands did not compare favorably with the sensitivity of the Kahn test It produced a positive test only with those sera which were very strongly positive with the Kahn Presumptive test. It is unlikely that this was due to deterioration of the antigen, as it was packaged in sealed glass ampoules.

About 3,000 sera were examined in this laboratory and in American hospitals in the vicinity. With the Kahn test, about 80 percent of the dancing girls and geishas were positive, and 50 percent of the civilians tested were positive. The Murata test gave only 1 positive result for each 10 positives with the Kahn.

When the hospital was finally completed and ready to open, it was turned over to the faculty of the Nagasaki Medical School. They appeared to be overwhelmed and a bit incredulous at this gesture. These men will form the nucleus around which Nagasaki medicine will be revived. Their task will require many years of conscientious labor in the face of adversity which could hardly be more severe.

THE BRITISH NATIONAL PHYSICAL LABORATORY

By H. BUCKLEY

Department of Scientific and Industrial Research, London

INTERNATIONALLY, the best-known scientific institution in Great Britain is undoubtedly the National Physical Laboratory. It is the largest of the research stations of the British Department of Scientific and Industrial Research, covering more than 60 acres in the thickly populated London suburb of Teddington and employing more than 2,000 workers During the 46 years of its existence it has gained a world-wide reputation for its scientific research on a wide range of subjects

The purposes for which the National Physical Laboratory was founded were "to bring scientific knowledge to bear practically on Britain's everyday industrial and commercial life; to break down the barrier between theory and practice; to effect a union between science and commerce." These are the prime duties of the Department of Scientific and Industrial Research as a whole, and so it was natural that the laboratory should become one of the ten major research stations controlled by the Department.

The director of the laboratory is always a scientist of international standing, selected by the Royal Society and the Department in consultation. At present it is Sir Charles Darwin, a grandson of the famous biologist of the same name. As a soldier in the war of 1914 18, he was awarded the Military Cross.

The progress of modern industry depends on an ever-increasing accuracy of measurement. No single industry, far less any single firm, can undertake the determination of the numberless standards and physical constants required in industrial processes. It is a form of research that can only be undertaken by a national organization, and in Great Britain this is the primary work of the National Physical Laboratory.

The Laboratory is responsible for the maintenance of the national standards of length, time, and mass, the temperature scale, and of various electrical standards. The accurate determination of the physical constants of materials and the study of the properties of engineering materials and equipment are also the responsibility of the National Physical Laboratory. Tests and calibrations of apparatus such as volumetric glassware, clocks and watches, thermometers of all types, optical instruments and so on are carried out, and a considerable volume of research is done for other government departments and research organizations and also for industry.

The National Physical Laboratory is organized in ten divisions, dealing with physics, engineering, electricity, radio, metrology, metallurgy, light, aerodynamics, ship design, and mathematics. In the short space of this article it is impossible to cover all the work of all the divisions; almost every branch of science is embraced and the scope of the research carried out is very wide

Since the work of the Laboratory involves precise measurement, a considerable amount of research is made into methods of measurement. The Metrology Division is responsible for the maintenance of the primary standards of length, mass, and time and for their derivatives such as volume, density, and pressure. The Division insures that agreement is maintained between its own metric standards and those of the International Bureau of Weights and Measures. Measurements accurate to one-millionth of an inch can be made, and these were of value during the war of 1939 45 when a large amount of precision testing of engineering gauges was carried out for industry.

HIGH VOLTAGE LABORATORY

A GENERAL VIEW OF HIGH VOLTAGE EQUIPMENT AT THE BRITISH NATIONAL PHYSICAL LABORATORY

The Metrology Division, however, is not the only part of the Laboratory concerned with precise measurement. The Electricity Division, for instance, has a section dealing

with the realization of the primary international and the centimeter-gram-second units of current, voltage, and resistance. It is responsible for the maintenance of these standards and also of radio standards. For this latter work a multivibrator wavemeter with quartz crystal control has been developed, its accuracy and constancy are such that frequency comparisons can be made to within one part in ten million. The Acoustics Section of the Physics Division has done much research work on the measurement of noise, and the Radiology Section of the same Division has charge of the British radium standard. Nearly a quarter of the world's estimated pre-1939 supply of radium has been measured in this section.

And so it is throughout the National Physical Laboratory. Maintenance of standards of measurement is one of the primary tasks, but that is only a portion of the work The study of the properties of materials is of equal importance, and most of the divisions are concerned in this work As an example, the Engineering Division has done much research on the causes and conditions of failure of materials, commonly known as "fatigue," it has also done the pioneer work on "creep," the flow which occurs when metals at high temperatures are subject to forces of various types The primary work of the Metallurgy Division is research into the physical and mechanical properties of metals and alloys, their constitution, and structure.

Most of the divisions do research which has a general application to industry as a whole, but two deal almost entirely with specific industries The Ship Design Division carries out experiments on models with the object of improvement of ship design and propulsion Before the 1939-45 war more than 80 percent of new British mercantile ship construction was only begun when model tests had been completed in the Division. The resulting economies in fuel for ship propulsion have saved the shipping industry at least a million pounds a year For the construction of the Mulberry Harbors for the invasion of France, the Division carried out experimental work necessary to decide on methods of construction.

The Radio Division carries out the research program of the Radio Research Board of the Department, and this program includes the fundamental study of the propagation of radio waves. This has involved extensive research into such problems as radio direction-finding, fading, atmospherics, and the physical properties of the ionosphere. To a large extent the British success in radio-location, or radar, as it is better known, before and during the war of 1939–45 was due to research on the ionosphere, carried out on behalf of the Radio Research Board.

Mathematics has become increasingly harnessed to the needs of government, industry, and research Often problems are encountered whose solutions are extremely complicated. When a formula has been found, it becomes necessary to compute the value of the formula for all likely values of the variables or to invent a mechanism which, on being set in accordance with these values, automatically and mechanically produces the answer The Mathematics Division undertakes research into new computing methods and machines and acts as consultant on mathematical matters to both the British Government and British industry. It is organized in three sections, dealing with computing, statistics, and the development of calculating machines. Its interest is wide, and the officer in charge is an expert on quality control in industry. Thus the new Division maintains the tradition of the Laboratory of keeping in touch with the needs of industry as well as modern developments in science.

OLD AGE: MINUS AND PLUS*

By GEORGE LAWTON
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MOST of us are so afraid of old age and so hostile toward it we assume "aging" means decline and that "change with age" is another way of saying "loss."

It may be hard to believe sometimes, but there are "plus" changes with age. We all know that a loss of one sense compels the other four to do the work of the missing one. In the same way, as we grow older, *the loss in one capacity may be precisely what makes the gain in another possible.* It is only after the departure of certain youthful pleasures that particular pleasures of later maturity can arrive.

But we must be prepared to exploit all these gains to the fullest. And we must also try to find compensations, substitute and indirect satisfactions, for the unavoidable "minus" changes which the years bring.

Let us assume you were normal mentally and emotionally in your youth—a big assumption—and that you are growing older with the usual quota of physical ills and financial reverses. But thus far you have managed to steer clear of catastrophe. What normal losses and normal gains may you expect?

It was Shakespeare in *As You Like It* who gave us a classic version of what it is to grow old. But we all know this by heart. I prefer to quote Shakespeare in an unusual translation made by Commander Alan McCracken, U. S. N., during the thirty-three months he spent in three different Japanese camps in the Philippines. Commander McCracken prefaces his translation with this explanation:

[This] passage . . . I translated into Chinese ideographs while at a Cabanatuan Prison Camp. The ideographs, translated back into English, read.

All the world a stage and all men
  and women mere play actors
Have exit, have entrance. One man
  during lifetime play on the stage many part
Acts of play designated Seven Ages
First New-born Infant—wail loudly—
  vomiting inside wet nurse arms.
Next Schoolboy—morning face shine—
  drag foot toward schoolhouse—imitate snail.
Next Lover—sighing resemble furnace—
  compose poetry dedicated beautiful woman
    eyebrows.
Next Soldier--curse—quarrel—seek
  reputation's bubble inside cannon mouth.
Next Judge—well-fed stomach grow fleshy—
  expression of eyes severe—
  mouth spout out wise saying.
Sixth age, Declining years—spectacles
  above nose—stockings plenty oversize
  skin shrink—big strong man voice re-
  turn child's voice—thin whistle sound.
Last act end this strange eventful history--
Second childhood—teeth go—sight go—
  taste go—everything go.

After so charmingly quaint a description of the aging process, I am afraid what I have to say will seem like a letdown, however exhaustive it may be. We may also yearn to linger over the early maturity stages in Shakespeare's schedule. However, we have stern business ahead of us, and reader and author, after taking one last look over their shoulders, will have to turn to the "Judge" as he comes on life's stage.

What inexorable processes are working themselves out as the Judge disports himself? First, let us take those human abilities which are most apt to decline with time. Then, before we become too unhappy, we can turn to a twilight zone, one where the changes may seem a loss to some

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and a gain to others—depending on one's philosophy of life. Last,. after wending our way through a Dantean Hell and Inferno, we will emerge into a true "Paradise" —unless we dropped out on the way—and contemplate the pure gains, the cloudless brilliance of the plus qualities.

Shakespeare speaks of the Judge's "severe expression." The Judge doesn't feel severe; he merely looks severe. One of the problems of age is that we feel one way but look the opposite. Shakespeare's observation underscores the fact that it is in appearance that we most notice the changes with age.

The second half of life might be considered one prolonged period of "dehydration." The body is less able to absorb and retain water, and the inner contraction of the tissues causes the surface muscles to buckle (wrinkle). Chemical deposits narrow the aperture of the blood vessels, and they grow more rigid. When that happens in the eyeball, and the almost inevitable farsightedness also is present, the look becomes more severe.

There are other changes. The hair on one's head thins out and what remains turns gray. Natural teeth may have to be replaced by the creations of the ceramic artist, today more beautiful than the originals. Vision and hearing grow less acute, and we resort to mechanical aids.

There is more to this sorry catalogue of cosmetic changes, but the reader will be glad to pass on to another familiar but less painful fact: the decrease in available energy and the way we have to cut down on the length of our working day, as well as to reduce the number of assignments we once could handle with dispatch.

Our muscles also begin to go back on us. Tasks on the job or in the home, once easy, now become monumental. The sports we formerly managed with skill and relish, we must drop. Our interest in them may also go, but if it continues, we are now in the audience. After a while, we are not even in the grandstand watching. We are in the overstuffed chair at home, listening.

Something else happens to the Judge. He finds his "reaction time" increasing, though he probably wouldn't call it that. In a mood of letting down his hair (?), he might admit he wasn't as quick on the pickup as he once was. His eyes and ears might still be good receivers of impressions, but it takes him longer to interpret and to respond to what they report. Even if he thinks as quickly as he ever did, the teamwork between eye and muscle is slower and less accurate than it used to be. The golf ball no longer responds to his subtlest wish as it did on those glorious afternoons of yesteryear.

But what if the golf course is no longer the scene of the Judge's major triumphs! He simply has transferred his athletic prowess to the dining table. Pleasures of the gut take the place of pleasures of the tennis court. And the trophy of his skill is the "fleshy stomach" of which Shakespeare wrote.

The Judge, if he can look at himself objectively, will note that his eyes and ears notice less of the world around them, which he is apt to attribute entirely to a slight worsening of his vision and hearing. But he also finds that he has to concentrate more, if he wants to fix impressions, and this becomes an awful chore for the Judge. He is apt to get irritable and blame the external impressions for this increased need to concentrate on them, not realizing that he has become more inward turned with the years and is beginning to think more of the past than he does of the future. The search for alibis may become his favorite mental exercise and regret his most popular hobby.

What with the Judge's physical and emotional decline, the outside world begins to seem faded and trival to him, though it teems with excitement and challenge, as it always has to one who happens to be young and happy or to an older person actively a

part of the present. Remembering new facts becomes more difficult in later maturity because the brain is like the wax surface of a phonograph disc which has hardened and no longer takes fresh impressions easily. It is no wonder then that the Judge finds it hard to *remember* what he has just seen or heard, though an even greater problem is to *see* or *hear* it in the first place.

Fatigue and, even more, the griping about it, is an obvious sign of decline with age. If you want a perfect setup for producing these signs, to a situation where concentration is necessary add speed as another requirement and include in the situation plenty of noise and hubbub.

All of us as we get older discover that we cannot pin-point our ideas with quite the same precision that we once did. If when younger we tended to miss the target in our talking and writing, we do it even more now.

There is a hoary but mistaken tradition that at the Judge stage and later, we talk *more*. What happens is that we talk the same amount but say less in the same number of words uttered in the same time. We are hitting all around the target and need more trials before we make a strike.

At twenty we live mostly in the present and partly in the future. At forty, past, present, and future receive about equal consideration. At sixty-five the thoughts and conversation of most of us tend to revolve about the past, with the present receiving a little attention, the future hardly any. Sentiment, nostalgia, loneliness, the search for the meaning of our life and of human existence in general—all make even normal older persons think more of the past. Generally it is remembered incorrectly. Idealization is the usual error: "the good old days;" less often, the past is "worsified" and made harsher than it actually was.

At sixty a man can expect on the average to live 14.73 years more; at eighty, 5.27 years; at one hundred, 1.37. No matter how long or short one's life expectancy, to be still interested in the possibilities of one's future is a major youthful trait that is always appropriate, because it is basic to the life process. Occasionally we see it in a person of eighty or ninety despite illness or infirmity. To lose interest in setting up personal goals, some distant, some near, and to give up the struggle for their achievement—this is to grow old, regardless of *when* it happens.

One of the best examples of wholesome perpetual youthfulness is Winston Churchill, who on his seventieth birthday was described thus: "He still remains a young man with a bright future." Bernard Baruch, during an interview in August 1945, told about his uncle, who decided to retire and sold his business, but, seeing it do so badly in the hands of others, he bought it back at eighty-seven. At ninety-nine he had tripled his assets. The last sentence of Charles Hanson Towne's autobiography, *So Far, So Good* (1945), is the eager reflection: "I wonder what will happen tomorrow."

Emerson said, "We don't count a man's years until he has nothing else to count." The older person has a shorter future, it is true, than a young one. That is why Jean Paul Richert maintained: "What makes old age so sad, is not that our joys but our hopes cease." However, while the young person can expect a long future, he has a shorter past to learn from and less skill in extracting lessons from it.

The Judge indulges in more self-analysis and self-scrutiny than the "Soldier" or "Lover," though not more than the "Schoolboy." What makes him seem more inward turned than even in adolescence is the change in the reason for self-analysis. The young person seeks additional knowledge of himself in order to increase his working efficiency or to establish smoother and happier relationships with people. But those men and women we call "old" reflect on themselves in treadmill fashion,

for they lack all desire or hope to improve their abilities or adjustments.

What aids and abets this tendency to purposeless self-scrutiny is the preference for passive, spectator roles and activities, often not a matter of choice what with the lessened energies and physical handicaps which accompany aging. We must remember, however, that "spectatoritis" is a common disease of Americans. Our country makes it easy to grow old, except on the farm and in the small community, for these provide more purposeful activities for older people than the city.

Our Judge, beset by diminishing physical energies, reduces all his voyages of discovery. More and more he succumbs to the seductive allure of the familiar and the routine. Such experiences are snug and easy and, while perhaps dull, are safe. New experiences, however, though they may be adventurous and full of zip and go, also involve thinking, physical effort, and risk. Caution and comfort become rallying cries as the old man races off to do battle but without leaving his easy chair. The older person wouldn't put it that way. He talks about "newfangled" methods or "foolhardy" experiences.

It is almost inevitable that as we prefer the armchair and rocker to an active and aggressive role in the community, we should devote ourselves to collecting things. Instead of going to the world or giving ourselves to it, we try to bring the world to us and hold it close where we can see it and examine it at our leisure. That is why at the Judge phase we are so passionately devoted to the collection of money, furniture, and honors. The hoarder, the miser, and the string-and-paper-bag collector are merely extreme examples of this "second-childhood" tendency, the old person and the young child both collecting for the same reason, however much the objects of their affections may differ.

Just as the Judge is not aware of the relationship between the decline in doing and experiencing and the interest in collecting things, so he does not see how withdrawal is making him a zealous defender of what he already knows and possesses instead of an acquirer of new facts and skills. He is a knight in shining armor defending a personal *status quo*. To scramble metaphors centuries apart, the Judge sets up an electronic eye which automatically warns him that a new idea has come near and a grave danger faces all he holds most dear and sacred, i.e., familiar.

An Arctic explorer always took with him an ancient and repulsive crone because the evening he found her rather attractive and appealing he knew it was time to pack for home. Every aging executive or professional man should see a similar warning significance in his exclamation:

> I've been in this field for thirty years. But I'll be damned if I'm not more and more convinced every day that the young fellows now coming into this business have the strangest and wildest ideas. What is the world coming to! Why, when I started out...

That speech is a signal to the speaker that he either should turn his job over to others or else submit to old-age rehabilitation.

Some old people may be feeble physically, but there is no battle they fight with such fierce, protracted, and bitter enthusiasm as when they resist a change in their daily routine or usual approach to a particular problem. The index file must have a red sticker on the left side and a blue one on the right—until the end of time. Toast has to have a certain spongelike softness, and the egg must lie on it exactly so, and woe be unto him who forgets it. Tea must have one and a half sugars, no more or less; dinner must be served at 6:15; the time for a walk is Sunday between 2:00 and 3:00.

When an old person indicts an idea on the ground of its newness, whether it be in business or private life, he is merely proving that with age there is an increase in rigidity of thinking. Old people are fond of com-

plaining of their poor memory, the way they forget names, or their stupidity in arithmetic. Actually, these are minor personal inconveniences, regarded with affection, not regret. The older person's loss in emotional and mental flexibility, however, is of major personal and social consequence.

Rigidity has its physical and psychological causes. But most older men and women are not interested in any system of rehabilitation or re-education which might make them more flexible. What they are afraid of is not the impossibility, but the possibility, of change. After all, most of us feel age should have some compensations, one of which is freedom from ever having to exchange any of our old ideas or habits for new ones.

In early maturity, there is a great surge toward socializing and good-fellowship which is part of the sex drive and the search for a mate. As this drive and search lessens, so does our fraternizing impulse. In later maturity, there is a less urgent need for the emotional release afforded by confession. Nor do we need friends, as we did in our youth, to help us verbalize our thoughts and feelings and thereby understand them.

In old age, our increased enjoyment of solitary experiences is another example of second childhood. In youth, we have not yet taken our place on the stage; in old age, we have stepped off and are leaving it farther behind each day. In both instances, the external world of major struggle and change is a far distant performance.

If we live long enough, we exchange the Judge phase for the "thin-whistle-sound" stage. That is when we begin to lose the secondary sexual characteristics: the earlier masculinity or femininity of voice, complexion, walk. We become not merely a third sex, a merging of male and female, but a fourth, a kind of neuter gender, a part of the natural world, like an old tree. Here, too, there are exceptions, and we all know the old person who is still an old *woman* or an old *man*.

Just how much of a decline there is in sexual urge and ability with age is not easily learned, and for several reasons. First, we do know that older men and women vary more in their sex behavior than in perhaps any other kind. Second, in the case of women there is no objective test of ability to perform as in the case of men. Then, the sexual performance of a man is very much influenced by success or failure in his emotional and vocational life.

At the Judge stage, the human animal no longer is quite so driven to obtain sexual release of a purely physiological nature as at earlier stages. And in the thin-whistle-sound phase, there is apparently for most men another great reduction in urgency and a greater reduction in ability to perform. What scientific evidence we have shows that the decline in performance ability in physically and emotionally normal older persons is less than generally supposed.

If it is difficult to determine decline in sexual urgency and ability with age, it is even more difficult to chart the actual degree of decrease or increase in *interest*, particularly in women. The attitude of older men and women toward their sexual lives is more dominated by convention than is their attitude toward other matters. The little we do know shows hardly any decrease in interest. But we do see a great decrease in the expression of this interest.

THE "DOUBTFUL" GAINS WITH AGING

Shakespeare's Seven Stages represent a straight ascent to the peak of adulthood and a straight decline. About the last stages we read with melancholy and reach the "sans-everything" description with a feeling of dismay mixed with inverted gratitude: "Thank God, I'll never be that way. I'll be gone long before."

The picture, however, is not so black. Shakespeare omitted all the asset aspects of aging, and they do exist. This is no mere

whistling in the dark. If a man or woman was well adjusted when young and encounters few external major reverses in later maturity, he will decline in some respects, it is true. But in other ways he has improved on his earlier achievements, particularly those of a mental, emotional, and social kind. Among such gains, however, are some which a few persons might deem losses. A scientific student of human behavior cannot arbitrarily decide the issue, since the side of the ledger on which we place these changes depends on one's "value-judgment," or point of view.

Suppose we rewrite Shakespeare, then, and insert subdivisions in the later stages. What do we get? Our very first doubtful change is the matter of decline in sexual urge and ability which we have already listed under losses.

Schopenhauer said he was grateful to old age because it had brought release from a monster tyrant: sex. Some persons might consider this escape from sexual tension as one of the plus aspects of old age.

We must note that with the passage of time we tend to become critics. We assay the past and foretell the future. This is the fact—but is it a gain or another decline?

We might apply the same question to the way in later maturity we seek the significance of our earthly span. The time par excellence for speculation on the possibility and nature of another existence is when our earthly one is drawing to an end with the speed of a rocket plane.

As we stand poised on our jumping-off place, we grow concerned with our position in history. That isn't so bad. But when we believe the problem should be equally crucial to others, particularly the younger generation, we can become an awful bore. Nothing daunted, we busy ourselves making sure that in our passage from *Who's Who* to *Who Was Who* (fortunately only our survivors see this last), due credit be given all our achievements. Posterity becomes a person, and with affection and a tear for ourself, we visualize "posterity" passing by a big handsome stone edifice with the magnificently lettered: "Dedicated to the Memory of John A. Doe, a great and noble soul." There are days of anxious indecision as we try to decide whether it is to be a monument, memorial, or mausoleum. A pleasure known to the aged and totally alien to the normal young is the loving and craftsmanlike joy of working over one's epitaph.

All of us have observed how our sense of time's passing depends on our mental state. When we are depressed and inactive, a day is endless; at a gay party, an evening is but a few minutes. When we are fearful of what the morrow will bring, the hours fly; when it holds nothing, the hours crawl. Albert Einstein, founder of the relativity theory, explained duration technically when he pointed out that to a man with a pretty girl on his lap, one minute was preposterously brief, but to a man sitting on a hot stove, one minute was an eternity.

Biological time, the rate of cellular repair and reproduction, as P. Leconte de Nouy points out, flows more slowly with advancing years. Wounds take longer to heal and fractures to mend.

But physical time, "clock time," goes faster the longer we live. De Nouy observes: "Young and old united in the same space live in a separate universe where the value of time is radically different." That is the fact. But how shall we interpret it—gain or loss? The explanation, of course, is that to a child of ten, one year represents one-tenth of his total experiences; to a man of seventy, one year is one-seventieth of his life and so only a fragment, gone a moment after its arrival.

THE PURE "PLUS" CHANGES WITH AGING

We now come to Shakespeare's most serious omissions. For besides the out-and-out declines and the doubtful gains, the *normal* person, aging *normally*, un-

dergoes changes which are definitely plus.

The older person, because he has lived long, has had varied experiences, each one repeated. Even Mrs. Judson, with an I. Q. below average, is forced to develop a little insight and is able to perceive a few relationships and connections she had not recognized before. Youth, no matter what its other virtues, and it has many, cannot have had a rich and diverse experience, just as it cannot in the nature of things have such possessions as old friendship.

Because of the variety and the repetition of our experiences and our memory of many trial-and-error experiments in living, the strategy of tackling problems often improves with age. This is what we mean by the improvement in judgment. Whenever it is important to know tactics, the many ways of gaining a given goal, then it is that the older person is valued. Our Judge, the older doctor, the craftsman, the political leader, the statesman, the artist in living—each can more than hold his own in meeting competition in his particular field.

That the Judge was developing more insight, Shakespeare did not show. Nor did he tell that the Judge also was devoting less time and energy to the struggle to make money, support a family, and obtain sexual satisfaction. As his voice turned more and more soprano-like, the Judge ate and slept less and was less interested in doing and possessing. The material world was no longer his oyster. Willy-nilly, at the Judge phase we start becoming a little more spiritual whether we like it or not.

Along with this change, there comes another which is yet one more way in which we enter a second childhood. In old age, we can devote ourselves to activities independent of rewards and become artists and contemplators. We like things for their own sake and are able to enjoy what we are doing while we are doing it. We no longer think only of results but can also enjoy the process by which they are achieved.

There is another reward for living long, one which makes the middle-aged look forward to old age. At last, we can do much of what we have always wanted to do. This may represent interests of youth, reluctantly abandoned, or brand-new choices.

This wonderful sense of freedom to choose entirely on the basis of what *we* want is possible mainly because of a long experience with our likes and dislikes. Also, we have more free time and free interest because we no longer devote most of our days and evenings to money-making, career-building, or love-making—whether in prospect or retrospect. Finally, our reduced energies and slower tempo of activities force us to focus on what deserves top priority in our lives. The hourglass is running low, and it is imperative that we enjoy the activities and experiences most important to us and scrap the minor ones. "When the half-gods go, the Gods arrive."

There is one major plus quality to old age, so important that Shakespeare used it to embrace most of middle and old age, and we have therefore been using it as a kind of theme song. This is the so-called judicial characteristic. We become more objective in our approach to problems. This is due only partly to experience; it is a by-product also of a decreased personal stake in the result of our judgment, reduced biological "drives," and fewer personal ambitions. The fires of the emotional life have been banked but not extinguished, and we can stop longer to think. Here again we see how a gain with age is made possible by the loss of some youthful characteristic.

One of the great possible gains with later maturity is the increase in self-acceptance. By that time we know our strengths and our weaknesses and no longer are dismayed or confused by what we are, though that does not necessarily

mean we approve of our weaknesses or stop trying to eradicate them.

That is why a "serene old age" is a misleading phrase. It seems to argue a passive acceptance and resignation. In later maturity we are more conscious of the difference between inner conflicts and outer ones. We do not mistake one for the other. The so-called serenity of old age is not a meekness; it is a calm knowledge of priorities, a gathering of energy to fight one's major battles.

Another great plus change with old age is the desire to be of service to others, owing partly to the decline in personal ambition, partly to a desire to gain the present friendship and future good opinion of that posterity whose recollection of us we sometimes feel may be the only kind of immortality we shall ever enjoy. But part of the altruistic impulse in older people is pure disinterested benevolence. In old age, many of us want to be kind and good simply because we want to be kind and good. It is the only outlet we have for our tender impulses and parental protective feelings.

The absence of socially approved outlets for our good impulses may do us as much harm as the lack of similar outlets for our hostile, destructive, and sensual impulses, our so-called sinful and evil sides. The apparent cantankerousness of some older people stems from their having no vehicle of social expression for their do-good impulses. They are also thwarted by the lack of recent practice in group participation after a long period of being family-centered or self-centered.

One of the finest gains we associate with old age is the development of perspective, the capacity for seeing vistas, a prime ingredient in wisdom. We are able to synthesize and correlate varied units of our experience and our formal knowledge. In the apt words of Virgilius Ferm:

> A person is said to have wisdom when he is able successfully to peer beneath or beyond the superficial and seemingly self-evident, to see subtle relationships, and when he knows how to coordinate seemingly isolated items and view them in their true perspective and to bring this insight to bear upon the problems and tasks which confront him. A man may have much knowledge and not be wise; however, he could hardly be wise without adequate knowledge. Wisdom is more than information, it is information crowned with understanding (*First Adventures in Philosophy*).

So much for the losses and gains with age. Perhaps even at best there still seem to be more minus quantities than plus. Even if numerically equal, some persons may feel that all the plus qualities of age fail to compensate for the loss of a single important youthful characteristic—boundless energy, or freedom of physical movement, or attractiveness, or sexual potency.

Faust, we know, sold his soul to the devil so that he might obtain the magical reversal of one major decline. The plus characteristics of old age were ashes in his mouth. But Faust, for all his lore, was a maladjusted old man, as he had been a maladjusted young one.

Grow old all of us must. One goal for us when middle-aged is to postpone and, if possible, reverse the declines which accompany aging. But we have a much more important goal. We must also anticipate the plus changes of age (not youth) which are on their way. And we must exploit these gains to the fullest when they arrive. Then we all will say, as a seventy-year-old woman recently wrote to me: "I need no adjustment to old age. The last ten years have been the happiest of my life."

A PHYSICIST LOOKS AT MORALITY

By ROBERT A. McCONNELL

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IT IS a matter of principle in science that one should not hesitate to state the terms of a problem even though no possibility of its solution has been envisaged. To have understood a dilemma is often to have taken the first step toward its resolution.

It is with this in mind that the writer examines the rational basis for morality and finds it wanting. He disclaims pretension that his brief exposition can add significantly to so massive and profound a subject.

It appears to the writer that the probability of atomic war within two decades can be conservatively stated to exceed one-half. In an age of technologically possible plenty and technologically unlimited destruction such a war will be without economic justification. Its occurrence must be blamed rather upon the failure of man's will-to-peace. Its avoidance will be only through assertion of the supremacy of man's spirit.

To this end, it is the need of our time that man should achieve a moral rebirth. Upon this need our many leaders are agreed. The professional guardians of men's souls cry out: "Only religion can save the world." The nationalistic demand that America rise to meet her destiny. Our intellectuals call upon democracy to capture to its ideals that spiritual devotion which Nazism evoked in Germany.

These dicta sound a common truth: Man must conquer himself even as he conquers his environment. Alas, they also sound a common note of helplessness. Our leaders stand without authority, like mariners in front of empty sails whistling for the wind.

With what arguments are we told to reform our ways of life? The proponents of religion offer dogmatic morality, clothed usually in the trappings of ancient superstition. Some vigorous philosophers in the spirit of our industrial culture reject the forms of religion, proclaiming instead an animistic and frankly intuitive moral code A third group appeals to reason, ignoring the gaps where reason fails or filling in with metaphysical logic of a religious nature.

Let us pass over religion and intuition. Let us examine reason as a basis for morality. How much does reason tell us? Starting with assumptions universally acceptable to thinking man as to the value of life, it will be agreed that one can deduce the validity of the principle of common effort, the principle that man should help man. This leads directly to most of the Ten Commandments: Thou shalt not kill... Thou shalt not steal... .These are the rules by which society preserves itself. The learnedly materialistic insist that these are the whole of morality: that all acts, all values which cannot be measured in terms of man's welfare are amoral. But is this so?

A simple illustration may raise some doubt. Is it moral for a man to take his own life? One may assume that by suitable collateral conditions the question is removed from judgment as a matter of common welfare. A completely detached observer would then still hesitate to say that this is not a moral question. Yet there is in the field of demonstrable knowledge no basis upon which to answer it.

The issues at stake go far beyond the trivial question of suicide. One may ask in general: Is the end the only justification for the means? Or is there an absolute moral code to which we must adhere: is there an end of which we are unaware? It is no answer to say that evil means must

almost inevitably lead to an evil end when the end result is considered in its totality. One may all too easily find cases where the probability of evident good would by every reasonable standard outweigh the probability of knowable evil. Is there an answer? What sophisticated seeker after truth can say that he knows?

Or still again, that most vital question of our time: What is the proper relationship of man to state? How highly shall we prize individual freedom at the expense of collective welfare? Does the collective good transcend every right of the individual? Or must government seek that more difficult path toward the millennium by which it will not trespass upon the fundamental liberties of each of us? We answer this question, as indeed we must, forcefully and without hesitation. Yet we *could* answer it either way. This may be the ideological question over which the coming war will be fought. Do we know the right answer? Is there any basis for a right answer in the same sense in which we can approve of the principle of common effort? Is not our knowledge of man's relationship to the cosmos therefore seriously defective?

In view of these moral uncertainties, is it surprising that common man is slow to respond to sermonizing? His leaders do not agree among themselves. Moral science, unlike the physical, has been forced to answer questions while lacking basic knowledge upon which to formulate a reasoned answer. Can man be blamed if he chooses to assume that all wisdom is contained within these two quatrains?

> Myself when young did eagerly frequent
> Doctor and Saint, and heard great argument
> About it and about: but evermore
> Came out by the same door where in I went.
>
> A Book of Verses underneath the Bough
> A Jug of Wine, a Loaf of Bread—and Thou
> Beside me singing in the Wilderness—
> Oh, Wilderness were Paradise enow!

This represents one possible point of view. Suppose instead, however, that we boldly try to follow reason to its end as though unfettered by our finite humanity. Shall not we then conclude: If war is to be banished between nations, if civilization is to advance without retrogression, we must find that key which will establish the whole of morality upon a rational basis?

HENRY ADAMS AND THE REPUDIATION OF SCIENCE

By CHARLES I. GLICKSBERG
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AT THE very start of his career, Henry Adams had discovered, as the result of his reading of Lyell and Comte, that man was not exempt from the laws which governed nature. It was a momentous intellectual discovery. On the basis of this insight, Henry Adams sought to determine the relation of man to nature and of nature to man. Gradually he came to the conclusion that the concept of order in the universe was a man-made fiction which received no substantiation from the chaos that ruled nature. The course of history revealed incessant conflict, internecine wars, restless and unpredictable change. There was but one golden period when the dream of spiritual unity was fulfilled: the twelfth century. Then the Mother of God held supreme sway over the hearts of men and canalized all their energy, all their capacity for faith. At last men were devoted with single-minded zeal to a religious ideal, and the evidence of their absolute faith was everywhere to be found in architecture, manuscripts, statuary, stained-glass windows. The study and interpretation of that century in its relation to the modern age of scientific skepticism became Adams' consecrated task.

Though he had speculated boldly on the possibility of establishing a science of history, his attitude toward science underwent a profound transformation; initial skepticism toward the scientific synthesis was replaced by a definite feeling of antagonism. The Middle Ages proved more fascinating and more worthy of allegiance than the God of the Dynamo. St. Thomas Aquinas, he concluded, was a better guide to life than Karl Pearson. The Thomistic philosophy exemplified unity on a grand scale; the modern world marked the enthronement of anarchy. Science, though it could confer on man the power to control nature, could not give him the blessing of faith.

Early in life Henry Adams' imagination had been quickened by listening to the lecture that Agassiz delivered on the glacial period and paleontology. But he was more excited by the theory of evolution, which drew him like a magnet. True, he had to take the atomic theory and Darwin's Law of Natural Selection on trust for he was not in a position to verify them, but they seemed to lead to some generalization which would make everything clear. If he could not understand Darwinism in all its implications, he might at least gain some valuable clues by plunging into the study of geology, where he might find proof to confirm the theory of evolution. He read the *Antiquity of Man*, by Sir Charles Lyell, and also his *Principles of Geology*. Here was an impressive statement of unbroken evolution, demolishing once for all the foundations of religion. He risked the venture of reviewing Sir Charles Lyell's book—his first connection, in print, with the world of science.

This exploration, however, led to difficulties. Was it defensible, he wondered, to assume uniformity from the start? The law of evolution often broke down, and it did not illustrate the principle of natural selection. Was the theory that Lyell advanced more than pure inference? All Henry Adams could detect was the process of change. The evidence supporting the doctrine of evolution was far from conclusive. Evolution did not seem to evolve; the uniformity that was postulated was by no means uniform; the work of selection was not selective. Though Adams wished to believe in the new doctrine, which other Darwinians accepted as a religious dogma,

he found on examination that he had no faith in it. Darwinism could not hold him. Even scientific faith was alien to him, and he outdid the scientists in his stubborn questioning of facts and theories. A skeptic at heart, he tried to make out that at the time he was a Darwinian for fun! The fun, however, could not hide his fear at the consequences that might flow from the scientific revolution. Was it not possible that, though man had mounted science, the steed might run away with the rider?

I firmly believe [he wrote on April 11, 1862] that before many centuries more, science will be the master of man. The engines he will have invented will be beyond his strength to control Some day science may have the existence of mankind in its power, and the human race commit suicide by blowing up the world.

As events have proved, this was by no means a fantastic conjecture.

Henry Adams was no scientist. It is doubtful whether he ever visited a laboratory or, during his stay at Harvard as Professor of History, ever fraternized with members of the science department. It is certain that, with the exception of his interest in geologic formations (while in the South Sea Islands, he made observations of coral beds as a check on Darwin's theory), he never performed any experiment that called for exact observation or the use of technical instruments. For facts as such he professed unmitigated contempt. He was interested primarily in the philosophy of science, the ultimate cosmic conclusions to which science could lead. In short, he was an armchair, or philosophical, scientist.

His growing disenchantment with the scientific method, once he grasped its inevitable limitations, was to be expected. One who thirsted for the Absolute could not rest satisfied for long with Positivism or Pragmatism. The meaning he sought to win could come only from unconditional faith, and here science was powerless to help him. All it could give him was the foundation of facts and a hypothesis by means of which to interpret them. Science, Henry Adams concluded, was but an illusion, less satisfying by far than other illusions that men once entertained in the bitterness of their need. If one had to choose, the symbol of the Virgin was infinitely more consoling than the symbol of the Dynamo.

There was a conflict in Henry Adams' soul between the artist and the scientist. The scientific discipline was too intellectual and too impersonal to supply him with the certitude of absolute faith for which he yearned. Though he might strive for plenary faith, religious absolutism was forever beyond his reach; he could not possibly recapture the religious experience except vicariously as he sat in the immense vaulted cathedral at Chartres and gazed in a kind of mesmeric trance at the marvelous stained-glass windows, beholding in his mind's eye the innumerable pilgrims who had knelt on these floors, the multitudes of sinners and sufferers that had come to the all-compassionate, all-forgiving Virgin, Mother of Sorrows, for help and healing. It was the aesthetic force of the religious experience embodied in the art of the Middle Ages that fired his imagination and stirred him to the depths.

He was the type of man who is dominated by the intellect and yet rebels against the tyranny of reason. As a historian he had a high respect for the accumulation of exact and thorough information, but that was not the primary aim of historical research, nor was scholarship in his eyes the aim and end of life. What he sought in the pursuit of history was the formulation of general principles arising out of past events and capable of fully explaining them. Just as a scientist draws up a theory which is a kind of postulational framework into which the observed facts fit, so Henry Adams, the would-be scientist of history, endeavored to discover a theory which would apply to all the known historical data and at the

same time account for the baffling phenomenon of change.

Throughout his life he sought understanding and he sought it in the only way a member of the Adams clan could seek it—through the disciplined faculty of reason. One had to depend on that in the last analysis or else everything proved a quicksand of uncertainty. This predominance of intellectual over emotional strength was bound to have its consequences, not so unfortunate as in the case of Charles Darwin or John Stuart Mill but equally marked. After the death of his wife, his skepticism became an inveterate mental habit. He retired more and more within his own shell, and his emotional nature had nothing to feed on.

THERE is a secret associated with his novel *Esther*, published anonymously in 1884. Why did this book mean so much to him in later years? With the exception of *Mont-Saint-Michel and Chartres*, there is no other book that he speaks of so highly. Why? We are forced to the conclusion that it had for him a special personal significance. The novel is much more than a story; it supports a thesis. Henry Adams intellectualized everything he touched, and this novel, like *Democracy*, is no exception. It shadows forth the ideas and convictions that were later to find expression in *Mont-Saint-Michel and Chartres*. First of all, it is a noble tribute to the nature of woman—intuitive, emotional, of sensibility all-compact—who arrives at truth in her own mysterious way. Second, it gives utterance for the first time to his perception of the conflict between religion and science, intuition and reason, feeling and intellect, science and art. This problem, in which he was engrossed for a number of years, he never succeeded in solving to his own satisfaction.

In *Esther*, Henry Adams was seeking to resolve in imaginative terms the problem that tormented him all his life long. The search for a unified meaning ended in defeat—a defeat that he pretended to regard with irony, but it did not ease the ache or fill the inner void. The suicide of his wife intensified his longing to escape from himself and what seemed to him a purposeless existence, a life that had been a "failure." To live without having achieved inner harmony, a pattern of meaning—that is what he meant by failure. Perhaps the universe possessed no meaning that the human mind could grasp; perhaps the quest for intelligible cosmic explanations was an aberration. Skepticism was the refuge of the orphaned intellectuals of the nineteenth century looking without hope toward the twentieth century. Skepticism led inevitably to science; he could not escape its impact. But if the universe was devoid of all meaning, if all human aspirations and ideals were no more than illusions and the sole reality was but a flux of energy, then there was but one conclusion to draw: a wise man would select those illusions which brought the blessing of inner peace. Illusion for illusion, the worship of the Virgin was as reasonable a faith as the belief in the electron. The geologist in *Esther* declares: "Mystery for mystery, science beats religion hollow." Neither religion nor science is true, he announces. Even science begins by demanding faith in the incredible. Solemnly he assures Esther, who is passing through a mental crisis, "that the doctrine of the Trinity is not so difficult to accept for a working proposition as any one of the axioms of physics."

Henry Adams' pilgrimage may thus be viewed as a progression from intellectualism to the artistic outlook that encompasses all things, resolves all contradictions. Student of law and history and science, he had searched for a clue to the working of the universe, only to give up the quest in despair, but the failure is finally resolved in the synthesis of art. Henceforth, once this liberating vision

had been vouchsafed to him, he viewed all things—history, civilization, politics, religion—as a spectacle. If all things were compounded of illusion, then one illusion was fundamentally as good as another—perhaps even better. That is to say, those illusions were best which gave consolation, certitude, peace eternal.

When in 1893 he visited the Exposition in Chicago, he was startled by this display of mechanical power. Here was a break in the chain of causality, a gap in historical continuity, that baffled explanation. There was the dynamo which could generate infinite quantities of energy. Lingering long among the dynamos, Henry Adams was convinced they gave to history a new phase. Men who had no knowledge of science or technology had to come to terms with these monstrous forces.

If one could trust neither nature nor art, as Adams discovered after he set out on his travels, one was perforce driven back upon science. But the only certitude that was scientifically reliable was the field of statistics. Even here he was disappointed. The statistical method did not culminate in any terminus of faith. The equation grew to such a point of complexity that it was hopeless to try to solve it. Then came the journey to Caen, Coutances, and Mont-Saint-Michel, where at last he was able to find himself. The law he had depended on as a historian had proved futile. After admiring the cathedral at Chartres, Adams concluded that he had all along been following the wrong trail. When, in a broken spirit, he was prepared to admit that life had no meaning and that in any event it no longer mattered, he came to closer grips with his life-problem and undertook the study of the twelfth century.

Before he took the road to Chartres and abandoned the Dynamo for the Virgin, he had tried to formulate a science of history. This is dealt with challengingly in "The Tendency of History," sent to the American Historical Association on December 12, 1894. No matter how extensive its field of research, history must constantly seek a unifying principle. Adams hunted for some principle that would transform the multitudinous odds and ends of facts into a coherent system. Since science is predicated on a cause-and-effect assumption, any science of history, he declared, "must be absolute, like other sciences, and must fix with mathematical certainty the path which human society has to follow." Whatever the consequences that might follow, if science offered the sole avenue to truth, it must be accepted and taught.

The problem is carried a step further in "A Letter to American Teachers of History." The traditional Newtonian universe neither loses nor creates energy. To go the whole hog in science and assert that the energy expended in the course of historical development was identical with physicochemical energy—that was asking too much. It was Darwin who spilled the applecart—who made it imperative that in the future all phenomena, organic and inorganic, human and animal, be studied as a science. As Henry Adams is careful to point out, however, Darwin never sanctioned the doctrine that organization of increasing complexity was evolution *upward;* that evolution was in any sense the same thing as progress. The notion that the organisms which survive furnish an example of growth from "lower" to "higher" forms is a vulgar misinterpretation.

Opposed to the evolutionist, however, was the Degradationist brandishing the second law of thermodynamics like a bomb. Physicists, astronomers, and geologists predict the extinction of the world, while society blindly staggers on, unshaken in its faith that it is moving onward and upward toward perfection. What defense, if any, is the historian to put up against the assault of the physicist? If thought, reason, and will are electrocolloidal processes akin to mechanical energy, then the second law

rules supreme and must dominate the philosophy of history. Henry Adams perceived that matter is energy itself. If so, then from the point of view of physics there can be no exemption. Man as a natural force has no other function "except that of dissipating or degrading energy." Human beings have steadfastly rebelled against the dominion of scientific law but in the end they must yield. The teacher of history could not dispute the ground with the physicist. His only salvation lay in becoming a physicist himself and learning how to use experimental methods. "The Rule of Phase Applied to History" explicitly declares that "the future of Thought, and therefore of History, lies in the hands of the physicists, and that the future historian must seek his education in the world of mathematical physics. Nothing can be expected from further study along the old lines."

The revolutionary implications of this manifesto have not yet been given their due weight. Here was a member of the Brahmin caste, a littérateur and historian, a student of medieval architecture, arriving at the heretical conclusion that the future belongs to science. If society may be postulated as a current of thought, then it may be studied as we study gases and fluids in general. That is the substance of the theory. The object is to determine some definite change of direction in human thought. Unfortunately for the theory, there is no valid connection, as modern historians have indicated, between the laws of physics and those of history. Adams' attempt to base a science of history on the knowledge of physics has been upset by later explorations of the atomic structure. Henry Adams' desperate quest was foredoomed to failure. Analogies drawn from science could not be legitimately applied to the onward movement of history. Henry Adams grasped at a philosophy of history based on the second law of thermodynamics because it fitted in admirably with his pessimistic conception of modern civilization. Armed with the law of entropy, he could explode the anthropomorphic faith in the doctrine of progress. Society, like the sun and the nebular dust, was under the relentless sovereignty of this law; its energies would sink lower and lower until it reached a point of absolute extinction.

When this conclusion disappointed him, as it was bound to, Henry Adams turned to the Church as a valid substitute symbol for the machine age. As Henry Steele Commager points out in his essay on "Henry Adams" in *The Marcus W. Jernegan Essays in American Historiography*, his adoration of the Virgin was

> in part an emotional reaction, an act of faith; but Adams could not be satisfied with a reaction merely emotional; he had to rationalize his faith in the power and the grace of the Virgin. . . .He who had lived, passionately, the life of reason, who had inherited from generations of Adams' a reverent respect for reason, made this gesture of faith an exercise in historical logic.

What was the relation, Henry Adams asked himself, between the force of the Virgin, a psychological projection, and X-rays? Was the force of the Virgin as potent as X-rays? The work he beheld at the Louvre and at Chartres convinced him that the energy emanating from the Virgin exercised a more compelling sway over the minds and hearts of men than all the steam engines and dynamos mechanical power could put into operation. All the mechanical forces in the universe could not, like the Virgin, achieve the miracle of Chartres. It was the task of the historian, Henry Adams now felt, to trace the evolution of this spiritual energy.

In the effort to follow the track of this energy, Henry Adams was driven into the maze of scholastic science. He read Thomas Aquinas, Descartes, Montaigne, Pascal. In the search for truth he had to choose between God, who represented unity, and Satan, who stood for multiplicity.

All he had learned in the past was radically wrong; he would have to begin from scratch. The laws of science he had once accepted were now brought into question. Science could not demonstrate how chemical or mechanical energy could by a process of evolutionary change be transformed into thought. Henry Adams had traveled a great distance since the days when he had been a trusting disciple of Darwin and Lyell. It was about time to confess his distressing state of ignorance. Evolution was not salvation. Science had to be defied. He had to push further ahead, to penetrate the beyond. Of the two kinds of ignorance, that of science and that of the Virgin, he preferred the latter. It was more human, more reassuring, more universal. The twelfth century at least furnished the proof of unity, which Adams craved.

What science said might be true, but its truth, if accepted, implied an act of mental suicide. The taboo against questioning the sphinx of the unknowable was logically convincing, but the mind of man was subject to spells of unreason, to powerful impulses that could not be logically explained, as was evident in the incessant speculations about the universe, which were neither reasonable nor logical. If the unknowable were unknowable, would not man long ago have realized the futility of his efforts? The persistence and passion of the search were a priori indications that there was something there. Henry Adams failed to see that the alleged "ignorance" of science was a stimulus to further inquiry, not an invitation to abdicate the function of empirical reason. All he had gleaned from his extensive reading of science—Ostwald, Mach, Poincaré, and Pearson—was that it had finally demonstrated its ignorance.

Henry Adams' importance lies not in the originality of his speculation but in the candor and courage with which he faced the major problems that troubled his age. His conclusions were untenable because his purpose, in his later years, was not to understand but to believe. He wished to pluck the heart out of the scientific mystery so that he might arrive at the finality of faith. His motives in pursuing the study of science were therefore "impure." His "education," in one sense, represents an aspiration to reach the infinite, a desire to know the unknowable. He was only too glad to yield himself to the Virgin. One thing was certain: he could not abandon the quest. In *The Education of Henry Adams*, he writes: "Every man with self-respect enough to become effective, if only as a machine, has had to account to himself somehow, and to invent a formula of his own for his universe, if the standard formulas failed."

Henry Adams stood helpless before the prodigious advance of science, incapable of judging the correctness of its conclusions, passing over what he could not understand but hopeful of catching a vital clue that would complete his education. His search was not an intellectual exercise but a cosmic quest, a hunger for salvation, for religious unity. Like Tolstoy, but without the latter's emotional intensity, he exhausted all the available explanations of the universe. Since science was the distinctive contribution of the nineteenth and twentieth centuries, he set himself earnestly to the task of mastering it. The twelfth century, however, was a blessed escape from the vertiginous dynamism of the twentieth. In the world of Thomas Aquinas he could feel at home. Science promised so much and yielded so little fruit. Darwin had led him astray. As far as he was concerned, the only book it had been worth writing was *Mont-Saint-Michel and Chartres*.

In this volume, Henry Adams wanted to show with what intensity the vital energy of that period expended itself in the highest forms of religion and art. Compared with that epoch, the present age, which had lost its primitive creative instinct, was decadent.

Regarding the triumphs of this age, he declares in his "Prayer to the Virgin of Chartres":

> Yet we have Gods, for even our strong nerve
> Falters before the Energy we own.
> Which shall be master? Which of us shall swerve?
> Which wear the fetters? Which shall wear the crown?

His "Prayer to the Dynamo" is a poem of a different order. It addresses the Dynamo as a "mysterious power," a "despotic master," a "tireless force." Either humanity or the dynamo must bend to bear "the martyr's cross." Whether it be matter or mind, the Dynamo is blind, deaf to prayer; and yet man continues to pray. There is no answer to the question that man asks. Only silence. All those who have taken this weary journey through the deserts and abysses of science will have no choice at the end but to come back to the Virgin and surrender to her grace and love.

The Middle Ages provided both escape and corrective. Throughout *Mont-Saint-Michel and Chartres,* it is the incandescent glow of faith that appeals to Adams, the feelings beautifully expressed in stone, arch, spire, glass, music, image, and verse. The writing of this book, which involved years of arduous yet ardent research, satisfied a profound personal need. The Virgin was the supreme miracle, and Adams gave up the effort to understand it by means of the unaided reason. Man has failed in his efforts to capture the secret of the absolute. Science now repudiates any attempt to smuggle philosophical essences into its investigations. The twelfth century at least strove to achieve the truth that God was the ultimate universal and that truth existed outside the mind of man. On ultimate issues, Haeckel and Farady and Clerk-Maxwell are as much in the dark as the theologians of the Middle Ages. There is this much to be said for Thomas Aquinas: he succeeded in fusing God and man, mind and matter, the universal and the individual, into a harmonious whole.

Henry Adams studied mysticism but never became a complete convert to mysticism; he studied science but never became a scientist. What science discovered in the behavior of atoms he attempted to apply to the behavior of human beings in society, but he soon saw that it was impossible to reduce either nature or society to order. That is an illusion rooted in the mind of man. It is a dream of knowledge that is repeatedly destroyed by the brute force of chaos, yet man clings to the dream. Like Karl Pearson, Henry Adams had argued that the laws of science are not true in an absolute sense; the forces of nature function without apparent purpose or direction: a violent illustration of anarchy in action. If the universe is nothing more than a loose bundle of anarchical energies, if purpose is an anthropomorphic illusion, it means that man's history, too, is a record of confusion and chaos.

In accordance with his theory of the dissipation of energy, Henry Adams came to regard reason as an enfeeblement of energy, being but a modification of primordial instinct. Instinct, not thought, is the source of vital energy. That is why he distrusted the thinking process and fought against his own overdeveloped intellectualism. It is this desperate denial of the significance of consciousness, reason, and science which led Henry Adams to compose his "Prayer to the Virgin of Chartres."

All those who turn to science with "impure," utopian expectations are sure to be disappointed. Science does not promise salvation to its devotees; the best it can give them is but a partial glimpse of the ever-elusive and bafflingly complex totality of truth. The struggle through which Henry Adams passed is of considerable significance because it portrays so poignantly the crises and conflicts of contemporary intellectuals. Today, as in Henry Adams' unhappy old age, it is

alleged that scientific thought illustrates a trend away from naturalism. There is an unmistakable emphasis on the importance of the irrational, and there are repeated efforts, with which Henry Adams would probably have strongly sympathized, to impose a "spiritual" interpretation on nature. Recent developments in the philosophy of science, notably the Heisenberg principle of uncertainty, are looked upon by a number of lay thinkers as confirming their intuitions concerning the nature of the universe. At last, like Adams after he knelt in prayer before the Virgin, they are free from the iron reign of determinism.

Unfortunately for these people, the development of modern physics offers no support for their consolatory conclusions. The principles of modern science do not lend themselves to such casuistic stretching. The *sine qua non* of physical theory is that for every term used we must provide a description of the physical operations by which it is to be tested. A scientific proposition has meaning only if it states the means for its verification. If these are not given, the statement belongs to the realm of metaphysics. Judged by this rigorous canon of operational validity, Henry Adams was a metaphysician rather than a scientific thinker.

In one respect and in one respect only does science support some of the contentions of Henry Adams, who was seeking a clue to the heart of the universe. Science is a postulational system. The law of causality, for example, is but the expression of a definition; it is therefore neither empirical nor a priori but part of a heuristic terminology. Theoretical science is thus essentially a work of the imagination, but from this it does not follow that it is irresponsible in its constructions. Pure science, declares Phillip Frank in *Between Physics and Philosophy*, "states nothing about empirical nature; it only gives directions for portraying nature." Henry Adams was right, too, in recognizing that science progresses by eliminating the anthropomorphic and the subjective. But then, asked Adams, how on such a basis explain the action of the mind if it must be interpreted in mechanistic terms? Put in this form, the problem appears insoluble. A mathematical world outlook seems to be more in tune with a philosophy that stresses the "spiritual" element in the structure of the universe. But properly understood there is actually nothing spiritualistic in twentieth-century physical science. The role of the psychic cannot be smuggled in with impunity. Nor can we take comfort of a metaphysical character from the theory of indeterminacy. The scientist simply asserts that truth, if it is to be accepted scientifically rather than metaphysically, exists only in its concrete embodiments. Our statements are true if there are valid methods of testing them.

What judgment, then, can science pass on *Mont-Saint-Michel and Chartres?* The way out of this dilemma is to bear in mind that language does not always perform a communicative function. It may be employed as a means of arousing feelings or attitudes. It may be suggestive, not logical and informative in character. In *Experience and Prediction*, Hans Reichenbach describes another function of language: the relaxive function. "Language may release us from an inner constraint, may slacken a tensed mind—be it the oppression caused by physical or psychical pains, or the delightful tension of joy, or the nervous constraint of productive situations of a creative mind." That was the function language performed for Henry Adams when he composed his "Prayer to the Virgin of Chartres" and his glorification in prose of the mystical power of the twelfth century.

Henry Adams touched the life of his time at many vital points and reacted with sensitive awareness to all its complex stresses and strains. He had to cope with two newly emerging "destructive" forces:

industrialism and science. It would have been comparatively easy for him to have played the game as the world plays it—to compromise, to make a god of expediency. His destiny was precisely this—to stand apart and suffer, to search for a "truth" difficult and impossibly out of reach. His pursuit of science was that of a gifted layman. He could not proceed far without feeling lost and damned. Since science left him high and dry, he turned to religion for the truth which saves. His overdeveloped skeptical intelligence, tired of the fruitless struggle, yearned for the absolutism of faith. All that science could give him was the promise of power; he preferred to take the road that led back to the Virgin, confident that others would, in the hour of disillusionment, follow his example. His confidence, however, was shaken again and again, and it was the frustration of his efforts to achieve a transcendent synthesis that would supplant the dominant philosophy of science which accounts in part for the fact that he persisted in looking upon his life as a "failure."

WAR ECHOES FROM LUCRETIUS

Man, the high of mind and proud of heart,
Is soonest fallen in the wake of force.
Mountains hold their scarred forms;
Flowers survive, and birds sing at the ruins.
But man, the monarch, fallen, melts—
To seep his humble way to dust inanimate.
A tale no sooner told than tells itself afresh
Nothingness alone fails feel the cosmic urge
To tread untiringly the upward way.
No clod of earth insentient but minds itself as man
And yearns and strains to mount anew the spiral steep.
Speck binds itself to speck, but darkly knowing,
To feel the flush of touch,
Viable with cosmic zest, clairvoyant of sentiency.
Nothing fails of return in Nature's rounds,
Though all is touched the while with sad impermanence.
The rose surmounts the mould and, unceasingly,
Life finds a darksome track through death
To light the earth with life—and life again.

T. V. SMITH

THE PRINCIPLES OF POOR WRITING

By PAUL W. MERRILL

Mount Wilson Observatory, Pasadena

BOOKS and articles on good writing are numerous, but where can you find sound, practical advice on how to write poorly? Poor writing is so common that every educated person ought to know something about it. Many scientists actually do write poorly, but they probably perform by ear without perceiving clearly how their results are achieved. An article on the principles of poor writing might help. The author considers himself well qualified to prepare such an article; he can write poorly without half trying.

The average student finds it surprisingly easy to acquire the usual tricks of poor writing. To do a consistently poor job, however, one must grasp a few essential principles:

I. Ignore the reader.
II. Be verbose, vague, and pompous.
III. Do not revise.

IGNORE THE READER

The world is divided into two great camps: yourself and others. A little obscurity or indirection in writing will keep the others at a safe distance; if they get close, they may see too much.

Write as if for a diary. Keep your mind on a direct course between yourself and the subject; don't think of the reader—he makes a bad triangle. This is fundamental. Constant and alert consideration of the probable reaction of the reader is a serious menace to poor writing; moreover, it requires mental effort. A logical argument is that if you write poorly enough, your readers will be too few to merit any attention whatever.

Ignore the reader wherever possible. If the proposed title, for example, means something to you, stop right there; think no further. If the title baffles or misleads the reader, you have won the first round. Simmilarly, all the way through you must write for yourself, not for the reader. Practice a dead-pan technique, keeping your facts and ideas all on the same level of emphasis with no telltale hints of relative importance or logical sequence. Use long sentences containing many ideas loosely strung together. *And* is the connective most frequently employed in poor writing because it does not indicate cause and effect, nor does it distinguish major ideas from subordinate ones. *Because* seldom appears in poor writing, nor does the semicolon—both are replaced by *and*.

Camouflage transitions in thought. Avoid such connectives as *moreover*, *nevertheless*, *on the other hand*. If unable to resist the temptation to give some signal for a change in thought, use *however*. A poor sentence may well begin with *however* because to the reader, with no idea what comes next, *however* is too vague to be useful. A good sentence begins with the subject or with a phrase that needs emphasis.

The "hidden antecedent" is a common trick of poor writing. Use a pronoun to refer to a noun a long way back, or to one decidedly subordinate in thought or syntax; or the pronoun may refer to something not directly expressed. If you wish to play a little game with the reader, offer him the wrong antecedent as bait; you may be astonished how easy it is to catch the poor fish.

In ignoring the reader avoid parallel constructions which give the thought away too easily. I need not elaborate, for you probably employ inversion frequently. It must have been a naive soul who said, "When the thought is parallel, let the phrases be parallel."

In every technical paper omit a few items that most readers need to know. You had to discover these things the hard way; why make it easy for the reader? Avoid defining symbols; never specify the units in which data are presented. Of course it will be beneath your dignity to give numerical values of constants in formulae. With these omissions, some papers may be too short; lengthen them by explaining things that do not need explaining. In describing tables, give special attention to self-explanatory headings; let the reader hunt for the meaning of $P^{\prime}r_0$.

BE VERBOSE, VAGUE, AND POMPOUS

The cardinal sin of poor writing is to be concise and simple. Avoid being specific; it ties you down. Use plenty of deadwood: include many superfluous words and phrases. Wishful thinking suggests to a writer that verbosity somehow serves as a cloak or even as a mystic halo by which an idea may be glorified. A cloud of words may conceal defects in observation or analysis, either by opacity or by diverting the reader's attention. Introduce abstract nouns at the drop of a hat—even in those *cases* where the *magnitude* of the *motion* in a downward *direction* is inconsiderable. Make frequent use of the words *case*, *character*, *condition*, *former* and *latter*, *nature*, *such*, *very*.

Poor writing, like good football, is strong on razzle-dazzle, weak on information. Adjectives are frequently used to bewilder the reader. It isn't much trouble to make them gaudy or hyperbolic; at least they can be flowery and inexact.

DEADWOOD

Bible: Render to Caesar the things that are Caesar's.

Poor: In the case of Caesar it might well be considered appropriate from a moral or ethical point of view to render to that potentate all of those goods and materials of whatever character or quality which can be shown to have had their original source in any portion of the domain of the latter.

Shakespeare: I am no orator as Brutus is.

Poor: The speaker is not what might be termed an adept in the profession of public speaking, as might be properly stated of Mr. Brutus. (Example from P. W. Swain. *Amer. J. Physics*, **13**, 318, 1945.)

Concise: The dates of several observations are in doubt.

Poor: It should be mentioned that in the case of several observations there is room for considerable doubt concerning the correctness of the dates on which they were made.

Reasonable: Exceptionally rapid changes occur in the spectrum.

Poor: There occur in the spectrum changes which are quite exceptional in respect to the rapidity of their advent.

Reasonable: Formidable difficulties, both mathematical and observational, stand in the way.

Poor: There are formidable difficulties of both a mathematical and an observational nature that stand in the way.

CASE

Reasonable: Two sunspots changed rapidly.

Poor: There are two cases where sunspots changed with considerable rapidity.

Reasonable: Three stars are red.

Poor: In three cases the stars are red in color.

RAZZLE-DAZZLE

Immaculate precision of observation and extremely delicate calculations....

It would prove at once a world imponderable, etherealized. Our actions would grow grandific.

Well for us that the pulsing energy of the great life-giving dynamo in the sky never ceases. Well, too, that we are at a safe distance from the flame-licked whirlpools into which our earth might drop like a pellet of waste fluff shaken into the live coals of a grate fire.

DO NOT REVISE

Write hurriedly, preferably when tired. Have no plan; write down items as they occur to you. The article will thus be spontaneous and poor. Hand in your manuscript the moment it is finished. Rereading a few days later might lead to revision—which seldom, if ever, makes the writing worse. If you submit your manuscript to colleagues (a bad practice), pay no attention to their criticisms or comments. Later resist firmly any editorial suggestions. Be strong and infallible; don't let anyone break down your personality. The critic may be trying to help you or he may have an ulterior motive, but the chance of his causing improvement in your writing is so great that you must be on guard.

FINAL SUGGESTION FOR POOR WRITING

Do not read:

ALLBUTT, CLIFFORD. *Notes on the Composition of Scientific Papers* Macmillan, 1923.

FLESCH, RUDOLF. *The Art of Plain Talk.* Harper, 1946.

GRAVES AND HODGE. *The Reader Over Your Shoulder.* Macmillan, 1943.

QUILLER-COUCH, ARTHUR. *On the Art of Writing.* [V]. Putnam, 1928.

Suggestions to Authors of Papers Submitted for Publication by the United States Geological Survey. U. S. Gov. Ptg. Off., 1935.

HEART OR HEAD

Tell me how is beauty read
To best advantage? By the heart
Which ahs and sighs and will not part
Or by the analytic head?

Heart, possessive, seeks essentials
Feels reciprocal dependence,
Waives sartorial resplendence,
Gives but cannot be impartial.

Head, aware, through symbol logic
Unifies its world of objects,
Stars and atoms in its projects,
Guides electro-mass panurgic.

Heart when headless pants for breath
Head when heartless conjures death.

JOHN G. SINCLAIR

THE AAAS-GEORGE WESTINGHOUSE SCIENCE WRITING AWARDS FOR 1946

By WILLARD L. VALENTINE
Chairman, Managing Committee

FROM funds provided by the Westinghouse Educational Foundation, A.A.A.S. in 1946 for the first time awarded a $1,000 prize to a newspaper writer for the excellence of his scientific reporting.

The winner of the contest was James Graham Chesnutt, reporter and rewrite man, of the *Call-Bulletin*, San Francisco. The judges also selected two entrants for honorable mention: Herbert A. Shaw, Jr., of the *Dayton Daily News*, and Stephen White, of the *New York Herald Tribune*. The prize and certificates were awarded on December 27 at a luncheon in Boston.

The arrangements for conducting the contest were in the hands of a Managing Committee: James A. Baubie, of Westinghouse Electric Corporation, Pittsburgh; Otis W. Caldwell, General Secretary, A.A.A.S.; Watson Davis, of Science Service, Washington; Morris Meister, President, National Science Teachers Association; F. R. Moulton, Administrative Secretary, A.A.A.S.; G. Edward Pendray, public relations councilor, New York, N. Y.; Robert Potter, of the *American Weekly*; and W. L. Valentine, Editor of *Science*. This committee worked out the details of publicizing the contest, made the rules, and obtained the entries. The success of this work is attested by the fact that there were 137 entries, which were well distributed geographically.

The actual judging was done by a committee selected according to a procedure established by the managing group and included two scientists, two newspaper editors, and two representatives of the public. The scientists were James B. Conant and A. J. Carlson, president and past-president of the A.A.A.S., respectively. The newspaper profession was represented by Wilbur Forrest and W. S. Gilmore, president and past-president of the American Society of Newspaper Editors, respectively. The public was represented by Elbert D. Thomas, U. S. Senator from Utah, who has long been a member of the A.A.A.S., and Sally Butler through her office as President of the National Federation of Business and Professional Women's Clubs. Dr. Meister acted as chairman of the Judging Committee.

The judges met in New York City on December 4 to select the winner after the entries had been screened by Dr. Meister and the writer.

The judges made their selection from eighteen stories, sent to them several days before the meeting in New York. The stories had been uniformly reproduced and coded in such a way that the judges were not aware either of the names of the entrants or of the newspapers, and they could not tell whether they had been printed on the front page or on some other page in the newspaper. Using this method, the judges agreed easily on the top five stories, which they reported immediately when they went into session in New York. These judgments were made wholly independent of each other when the judges were many hundreds of miles apart. The final winners were ultimately selected after discussion of the stories and then unanimously recommended for the awards. Their stories are reprinted on the following pages.

The success of the 1946 contest made certain that it will be continued in succeeding years, and its scope will be enlarged in 1947 to include magazine science writers in addition to newspaper writers. Thus two prizes of $1,000 each will be offered in 1947 from these funds.

THE PRIZE WINNER

JAMES G. CHESNUTT
The Call-Bulletin, San Francisco

In an immaculate white laboratory, as spotless as a hospital nursery, a great boon to humanity has been born and is being nurtured through exacting experimentation. It is a specific preventive for the plague, one of mankind's worst scourges through the ages—the dread "Black Death" which takes the various forms of bubonic, pneumonic, and septicemic plague.

The efficacy of this chemical preventive, in animals, has been proven conclusively and dramatically by means of a unique "mouse village," in experiments conducted by one of America's foremost researchers—Dr. Karl F. Meyer, director of the Hooper Foundation for Medical Research of the University of California. And, already, this chemo-prophylaxis-sulfadiazine is being used in China as a preventive of human plague, on Dr. Meyer's recommendation, as a result of the research conducted at the Hooper Foundation. It means that a cheap, easily obtainable, and practicable preventive is available for mass prophylaxis in plague areas, where expensive serums and vaccines could not be used. It means, far beyond that, that a powerful step has been taken toward breaking the time-enduring chain of flea-rodent-human which has saddled mankind with the plague for centuries, with periodic outbreaks of mass mortality like the Great Plague of London, which killed 68,000 persons.

"With chemo-prophylaxis," Dr. Meyer said, "and with the elimination of fleas by use of DDT and other chemicals such as R-1080, we can break the chain. Then we can knock out the rats (for years the only preventive against plague) and the plague is licked for good—obliterated from all closely inhabited areas on the face of the earth." Dr. Meyer, a scientist and not given to extravagant overstatement, does not contend that he has found a "cure" for plague. The real cure lies in successful prevention. Nor is he the first to experiment with the sulfonamides—the "sulfa" family—to combat plague bacilli. But he and his research associates, Albert Lawrence Burroughs and Stuart F. Quan, are the first to demonstrate, with the dramatic "mousetown" experiments, that plague can be controlled and prevented under circumstances approaching those of a human community.

The experiments are complex and many-sided, but the results—so far—startlingly clear.

In the first experiment, conducted about a month ago, 100 mice were put on the east side of "mousetown" and another 100 on the west side. Those on the east side were given plain water to drink. The west-side mice were given sulfadiazine in solution of a concentration equivalent to three grams for a human being in daily consumption. Next, 800 plague-infected fleas, the deadly vectors which spread the disease, were placed in the pens—enough for four fleas per mouse in both pens.

Shortly, plague epidemic was raging in the east pen—the "plain-water" side of town. Mice died at the rate of fifteen to twenty a day. Most of them died from septicemic plague, as the fleas became additionally infected by feeding on the diseased mice, and the remaining water-dieting mice were overwhelmed with plague bacilli. Within nine days, the entire population of the "slum" area, without prophylaxis, was wiped out—to the last mouse. Twenty-five new mice, uninfected, were introduced as "immigrants." All of these were dead within four or five days.

Meanwhile, in the west pen, sulfa-bibbing mice continued to live in normal and

healthy community existence, although bitten by the same infected fleas which were free to commute back and forth between pens. Ten of these sulfa-fed mice died, but Dr. Meyer does not consider this significant, as there was no epidemic and the ninety survivors were healthy. The stricken ten, he feels, probably did not drink enough of the sulfa brew, or received a "fantastic dose" of the bacilli through being bitten repeatedly by doubly and trebly infected fleas.

"The contrast between those two pens was so amazingly striking," he said, "that you know positively that sulfadiazine *is* prophylaxis against plague."

A visit to "mousetown" today, where new experimentation is being carried out in furtherance of last month's findings, is indeed an amazing experience—to use Dr. Meyer's own word—and an unforgettable one. The door to the room in which the experiment is being conducted, a lethal chamber to a careless intruder, is kept locked and the key sequestered behind another door in a closet. Upon entering, one must remove the shoes as though entering a Japanese house and don a heavy pair of wool boot socks. Even the soles of these socks must be wiped on white cloths before crossing the threshold. The floor is surgically white and clean and sprinkled with crystals of DDT. This explains the shoe-removal ceremony.

"We must keep the floor white, so we can see any flea that might escape," Dr. Meyer explains. "Also, we don't want to scatter the DDT too near the pens; we don't want to kill the fleas in there."

In a corner of the room, flanked by two windows, is Mousetown, U. S. A. It is a rectangular box with plate-glass sides, open at the top, about eight feet long by four feet wide and three feet high. Dividing the pen into two equal sections is a partition, open screen mesh for about eight inches from the floor and the rest sheet metal. The floors (or, rather, streets) of Mousetown are of soft pine shavings. There are several cardboard "houses," and only a few mice visible.

The absence of population from the scene is explained by Dr. Meyer, sleeves carefully rolled to the elbow for flea surveillance, and using long metal tongs or forceps. "They like to burrow under," he said. "See, here they are. You can see the fleas on that white fellow. That one is quite sick with plague."

In the midst of each pen are two inverted pint flasks, like whisky bottles, the drinking fountain arrangement to furnish plain water to the white mice in the eastern district and to the black mice on the west side

The black mice, all survivors of the first test, are also on a plain-water diet now. With no protection other than their own immunity—*if* they have built up such immunity from their first experience—they are undergoing a double barrage of plague-infected fleas. "For," Dr. Meyer points out, "the fleas prefer the black mice. They flock over to that side because they have protection by coloration there; they can't be seen so easily as on the white fur. See! Look at that black fellow scratch. . ."

A visitor, and they are very few indeed, is grateful for Dr. Meyer's assurance that the two-foot high fence of slick glass, sealed with phenol and vaseline, prevents the fleas from going "over the hill." They can jump only eight inches high, Dr. Meyer says.

Although plague may be carried by a variety of fleas (notable exception is the common dog flea) Dr. Meyer's experiments have been conducted exclusively with the rat flea, known as *Xenopsylla cheopis*, large colonies of which are carefully raised in a special room of even temperature and humidity.

The white mice on the east side of town present a colorful picture in motion. There are some with daubs of red dye on their fur and others with yellow splotches. The

red-marked mice, Dr. Meyer explained, have been vaccinated with a plague vaccine formed of cultures of "killed" plague bacilli. The yellow-marked mice have been injected with the standard serum, developed at Hooper Foundation during the war and used by our armed forces. This is prepared from the globulin fraction of the blood of immunized rabbits, combined with sulfadiazine.

> We want to learn, by this experiment [Dr. Meyer said] whether these black mice, the survivors of the first test, did perhaps contact the infection and cured themselves with sulfadiazine, thereby building up immunity against infection. Also, we want to see what the reaction is of those which have been vaccinated and those injected with serum. See, that one with the red mark (vaccinated) is dying. He's a very sick mouse.

The serum, which was developed by the Hooper Foundation researchers during the war and manufactured on Dr. Meyer's recommendation, is made by injecting rabbits with living, but virulent, plague bacilli. The rabbits thus produce excellent antibodies, far better than those in horse serum, the researcher said.

> There is an interesting angle there [Dr. Meyer continued]. If you have a case of bubonic plague and treat it therapeutically with sulfadiazine, you can cure it and save life in about 50 percent of the cases. If you treat it with serum, you'll save, let's say, 40 percent. Now, when you use sulfadiazine and serum combined, you double the cures. That's why the serum is useful in combination treatment. Sulfadiazine alone, used therapeutically, will hold the plague bacilli in check and probably destroy a lot of them, but it liberates poisons and does damage to the tissues. The serum, probably, will neutralize those poisons. So, the solution would seem to be to give sultadiazine as a prophylaxis, and then more sulfa, plus serum, if the disease is contacted. Just think of it—in a plague area you could put human beings on sulfadiazine of three grams a day and immunize them. About 15 or 20 cents worth per person would do the trick!

The sulfadiazine prophylaxis is now being used in China by Dr. R. Pollitzer, an "old China hand" and plague expert now connected with UNRRA, Dr. Meyer said, upon recommendation of the San Francisco scientist.

The effectiveness of sulfa drugs against the plague bacillus was first shown in 1939 by research by Schutze of Great Britain and was further developed in India by Sokhey, who used sulfathiazole. Sulfadiazine is now used because of its lower toxicity and higher blood level maintained. The case fatality rate in India, Dr. Meyer said, fell from between 60 and 75 percent to 30 percent under sulfathiazole treatment, and to only 20 percent with sulfadiazine.

Plague is a disease which has no given or restricted point of entry into the human system. Bubonic plague may enter the system through a scratch or opening in the skin, contacted by the feces or regurgitation of sick, plague-infested fleas; pneumonic plague, caused by the identical bacillus, but affecting the lungs, may enter by means of oral spray from an infected person. Use of sulfadiazine as prophylaxis, according to Dr. Meyer, would eliminate this source of transmission—both by putting the known contacts on a sulfa diet and by making other persons immune.

"When the mucous membrane of the mouth and nose is covered with this infection-inhibiting drug," he said, "the chemical will keep bacilli from entering by this means."

His contribution to the ageless battle against plague is by no means Dr. Meyer's first distinguished work in the field of scientific research and explorative medicine. Pathologist, bacteriologist, author, and doctor of medicine, he is one of America's most widely known and respected researchers. A native of Basel, Switzerland, and a naturalized American, he studied at the universities of Berlin, Munich, Paris, Zurich, and Bern. After serving in the Transvaal, Africa, and at the University of Pennsylvania, he came to the University of California in 1914, where he was Professor of Bacteriology and Protozoology, joining the

staff of the Hooper Foundation the following year. He became acting director of the foundation in 1921 and has served continuously as director since 1924.

Plague, the disease against which he has been waging laboratory warfare for years, was first mentioned by an earlier physician, Rufus of Ephesus, in the first century, when it was discovered in Egypt and Libya.

It made its entry into Europe in the sixth century, where it colored the course of history by such outbreaks as in London in 1664-65; Vienna in 1679, with 76,000 dead; Prague in 1681, with a death toll of 83,000. The greatest tolls of history, however, were taken in the Orient and in India, where in the plague of 1896-1906 as high as 900,000 victims died yearly.

The *Encyclopaedia Britannica* says regretfully that "unfortunately, there is no plague specific" and adds: "Plague prophylaxis consists in warfare against rats and ground squirrels." The forward stride of science, aided immeasurably by Dr. Karl Friedrich Meyer and his associates of Hooper Foundation—plus several hundred anonymous citizens of Mousetown, U. S. A.—apparently has changed that gloomy outlook.

HONORABLE MENTION

Herbert A. Shaw, Jr.

Dayton Daily News

Based on information contained in the most up-to-date almanacs, there are four seasons to the year—spring, summer, fall, and winter. But to an estimated 5,000,000 unfortunate residents of these United States there's a fifth season—that of "hay fever." For from on or about August 15 until the first killing frost of autumn hay-fever sufferers seem to breathe nothing but air filled with tiny grains of pollen from ragweed.

True, there are other types of pollen, from both trees and grasses, that bring on the red eyes and running noses associated with hay fever, but in 90 percent of the cases "*Ambrosia elatior*," the botanical term for ragweed, is the chief offender.

Hay-fever symptoms were first described in the early nineteenth century by a doctor in England where there is no ragweed and no fall hay fever. This physician noted that the symptoms were evident only during the haying season in the spring and hence the name "hay fever."

Actually "hay fever" is a misnomer, but so popular has the term become that to change it now would be next to impossible. Doctors prefer the term "pollinosis" since the affliction is definitely caused by plant pollen.

Dr. Bostock, the English physician who first described hay fever's symptoms, erred quite a bit in his diagnosis—as did many of those who tried to prescribe for it in the years that followed. Bostock noticed that his symptoms were more noticeable on bright sunshiny days and came to the conclusion that sunshine was the offender. During the season he barricaded himself in his home, pulled down the shades, and found that he got along quite comfortably. This, it was pointed out years later, was perfectly natural since by closing the house he kept the grass pollen out.

Pollen was not thought to have anything to do with hay fever until years later—1859 to be exact—when another English physician, Charles H. Blackley, came to the conclusion that pollen was the root of all evil.

Blackley, a hay-fever sufferer, explained his pollen theory thusly:

One of his youngsters had gathered a bouquet of grass blossoms—similar to Kentucky blue grass—and placed them in a vase in a room seldom visited by the doctor. A few days later he entered the room, noticed the blossoms, and stopped to examine them. In doing so he struck the bottle and a cloud of pollen rose from the grass blossoms. The incident brought on an attack of hay fever. He then reasoned—and correctly—that pollen was the culprit.

Once pollen was discovered to be the base of the hay-fever affliction any number of "sure cures" was offered.

MOST prominent and popular among the early cures was the change of climate. This still persists. Seashore and mountain resorts early started spirited competition by assuring hay-fever sufferers that a vacation at their site during the season would insure complete freedom from any symptoms.

For those who couldn't afford the privilege of a trip to the seashore or mountains, there were miraculous home cures that included whisky, arsenic, belladonna, candy, cocaine, electricity, horse-radish, ice water baths, opium, quinine, Turkish baths, and tobacco, to mention but a few.

One gentleman, who remains unnamed, had this to say for the whisky treatment: "Whisky, I think, is about the only medicine that gives any decided relief and this is not always sure. It helps to pass away the time and deadens the acuteness of the attack."

A woman in Illinois helpfully came forth with the advice that molasses candy had helped her.

One frustrated gourmet who thought that eating habits definitely had some effect prescribed a diet of raw oysters, chicken, stale bread, and cucumbers.

Reverend Henry Ward Beecher, a long-time hay-fever sufferer, was told by his friend Dr. Oliver Wendell Holmes, that the "gravel" treatment was most heartily recommended.

"Gravel" said Dr. Holmes, "is an effective remedy. It should be taken about eight feet deep."

It might be said of hay-fever sufferers that they brought the whole thing on themselves by becoming civilized. For hay fever is largely an affliction of civilization because ragweed is the product of the cultivated fields.

America, much to the chagrin of the sufferers, is the home of the ragweed. No one loves the plant, bees shun it. Not even plant lice will associate with it.

As mentioned above, the chief offender in the hay-fever line is the "short ragweed," *Ambrosia elatior*. It must have been named by a botanist who did not suffer from hay fever for translated "ambrosia" means "food of the gods."

Second on the offending list is the "great ragweed," *Ambrosia trifidia*. In third place is the Russian thistle, or Russian tumbleweed.

Short ragweed, once known as Roman wormwood, is a slender plant with finely divided grayish-green leaves. Both male and female flowers are born on the same plant. In the North the plants, at maturity, attain a height of only a few inches. However, in the South, aided by a long growing season, they attain heights of from six to eight feet. This type of ragweed is found in almost all parts of the United States.

Big brother to *Ambrosia elatior*—*Ambosia trifidia*—shares a great part of the blame for fall hay fever. Although not as adaptable as its smaller kin, it nevertheless gets around. For the most part it is found in bottom lands of rivers and creeks. Farmers often refer to it as horse-weed. Its average height is at least three times that of short ragweed with upper limits of fifteen feet.

Goldenrod, which for years has borne the brunt of suspicion as chief offender, has

been ruled out by the authorities. Just how the supposition started is not known, but most likely it came about because goldenrod is plentiful and conspicuous in most parts of the country during the ragweed season. If a hay-fever sufferer were to take a deep sniff of a goldenrod plant he would most likely get a violent reaction, possibly because ragweed pollen was present on the plant. The reason is simple. Goldenrod pollen is heavy and sticky and thus cannot float in air. Atmospheric tests prove that goldenrod pollen is almost never found in the air.

The same can be said of the rose with regard to the belief that there is a rose fever early in the summer. Roses do not produce wind-borne pollen. Their pollen is carried entirely by insects. Scientists say that the offense attributed to the rose can probably be laid to grass pollen since grass comes into bloom about the same time.

When father starts in to explain the facts of life in terms of birds, bees, and flowers, the common ragweed will furnish him an excellent example of the latter. Some plants are self-pollinating; that is, the male sperm and female ovary are contained in the same blossom; others are cross-pollinated by insects such as bees, which carry the sperm from one flower to the ovary of another; while the third type—one of which is ragweed—depends upon wind pollination.

It is with the latter that hay-fever sufferers are concerned. To be airborne the pollen must be fine, dry, and powdery. Ragweed fits all three qualifications. Its pollen is microscopic and very powdery. A light breeze will send it soaring through the air in search of a mate. Borne aloft by rising warm currents of air, this pollen dust quickly spreads far and wide.

Just what does this ragweed pollen grain look like? Under a microscope—the only way it can be seen—the grain is an irregular circle with jagged edges.

In substance its chemical composition is as follows:

Alcohol soluble	42 9%
Moisture	5.3
Crude Fiber	12 2
Pentosans	7.3
Ash	5.4
Dextrin	2.1
Protein	24 4

Determination of the amount of pollen in the air, which in turn tells the hay-fever sufferer whether or not he is going to have a bad day, is accomplished by coating microscope slides measuring one inch by three inches with a very thin film of vaseline or some comparable oily substance. These slides are placed in a horizontal position and outdoors over a 24-hour period. In Dayton the count is made over a period from 9 A.M. to 9 P.M.

After exposure the slide is treated with a red dye which is absorbed only by the ragweed pollen. The slide is then placed under the microscope and the red grains counted. The results indicate the number of pollen grains per cubic yard of air. When the count rises above 50, hay-fever sufferers usually get a reaction. This is intensified as the rate climbs upward. In 1944 and 1945 in Dayton the count rose to a high of 1,800 on two occasions. The local average during the season is about 100 grains.

For the benefit of hay-fever sufferers who have looked in vain for a home remedy, it may be comforting to know that for more than a century medical researchers have been working on the project. Most hay-fever sufferers who have not availed themselves of medical advice believe that the mere presence of pollen grains in their nasal passages is responsible for their discomfort. Such is not the case. Hay fever is caused by a reaction in the tissues of the body when pollen poison meets an antagonistic substance in the blood or tissues.

Sensitive people have hay fever when they are exposed to pollen and do not have hay fever when they are not exposed. The effect of the pollen is not local but permeates the whole system of the sufferer. Attacks may be successfully prevented by carefully graduated doses of pollen before the usual season of sneezing. This method, though how it works is not completely understood, produces results.

PRESENT-DAY allergy specialists say that the first sensible way to treat hay fever was attempted in 1900 by a Dr. Curtis. He worked on the theory that injections of the plant that caused the fever into the sufferer would cure. He then prepared a solution of leaves, tops, and pollen of the ragweed. Two years later a Dr. Dunbar carried Curtis' step a little further by extracting protein from the ragweed pollen. He called the extract "Toxin."

Dunbar reasoned that an injection of the toxin should cure fever. To prove his point he gave a huge hypo dose to an assistant. That it produced results is putting it mildly. The assistant recovered, however.

Another popular cure was originated by one Dr. Weichardt, who reasoned that since herb-eating animals were continuously inhaling pollen while eating and suffered no ill effects, they must be immune. From the blood of these animals he prepared a serum called "Graminol," which was injected into suffering hay-fever patients. Though popular, the serum was ineffectual.

Treatment of pollen allergies which in turn bring on hay-fever attacks are now for the greater part in the hands of allergy specialists. Before any type of treatment is started these specialists conduct tests on the subject's skin to determine just what types of pollen bring on the attacks. To do this the allergist prepares solutions of the pollens and injects these under the skin of the arm or back. If the solution after injection causes swelling, the subject is said to be allergic to this type of pollen. Once the type of pollen to which the victim is allergic is discovered, allergists start on a program of immunization which may extend over several hay-fever seasons.

These treatments, consisting of the injection of ever-increasing amounts of pollen extract, usually start several weeks before the expected symptoms appear. Over the season anywhere from ten to thirty injections are made.

Variances of this plan are practiced by allergists. Some give injections through the year; others only during the season. The year-round treatment usually enables the allergist to increase the tolerance of the patient. Any allergist will frankly tell his patient that he cannot promise a cure. Hay-fever patients have been cured before and they are now being cured in ever-increasing numbers. Some persons are cured in a short time; some never.

On the drug side of hay-fever remedy, two new preparations, "benadryl" and "pyribenzamine" developed only recently, have shown some promise. Both drugs are administered by mouth in pill form and are similar in chemical construction. Neither is considered a sure cure, and both have rather unpleasant side effects and reactions in many cases.

Benadryl has been used experimentally by doctors at the Mayo Clinic. Their report on the drug points out that it does not cure hay fever but provides symptomatic relief in most cases treated. Relief of symptoms is often noted within an hour of standard dosage (50 mg.). Some patients are almost completely relieved by two or three doses daily. Side effects of benadryl include such reactions as sleepiness, dizziness, dry mouth, feeling of nervousness, and fatigue.

Pyribenzamine, which is so new it is not yet on the market, has undergone extensive research at the University of Buffalo. Of 313 patients treated there

for allergic rhinitis of all types including seasonal hay fever, three out of every four showed definite improvement. Again physicians emphasize that the drug is not a cure-all and in some cases is not effective at all. It is best used in conjunction with desensitizing injections. Results are not as good when the drug alone is used

Among remedies for hay-fever sufferers which do not entail medication or injections, are home air-conditioning systems and air filters. Both do alleviate suffering by filtering out to some extent the amount of pollen in the air, and by completely changing the air in a room or home at frequent intervals. To receive effective relief through these means, however, the patient is forced to remain indoors for the duration of the pollen season. Once outside he is at once exposed to the brunt of the pollen attack and suffers the consequences.

HONORABLE MENTION

STEPHEN WHITE

New York Herald Tribune

Probing for the first time in man's history into the cold reaches of interplanetary space, Army scientists have made radar contact with the moon, the Signal Corps announced yesterday. On January 10, a few minutes before noon, a radar pulse was directed at the rising moon from the Evans Signal Laboratory in Belmar, N. J. Two and one-half seconds later a returning pulse was clearly detected on a radar scope. In that time it had made a round trip of 480,000 miles—to the moon's surface and back.

At intervals of a few seconds other pulses were sent from the transmitter, and each time they were followed, two and one-half seconds later, by the telltale pulse on a blue-white cathode-ray tube. A loudspeaker, tied into the system, picked up the echo as a sound—not a very tuneful sound, but an echo from the moon.

There have been previous reports of moon-echoes from radio waves. Sir Robert Watson-Watt, British radar pioneer, announced such work many years ago, and indicated he had gone even beyond the moon. But his experiments could not be verified, and his results are far from certain. The Signal Corps is sure.

The January 10 experiment was not the last. For seven successive days the test was repeated on the rising or on the setting moon, for the antenna could not be pointed high in the sky. There were failures, but time after time the echo came back.

To science, this success has practical values. It is known for the first time that the various layers of the atmosphere—some of them bearing electrical charges—can be penetrated by a radio wave. As the work is pressed forward, refined measurements can be made of the speed of light, the speed of the moon, the distance to the moon. Ultimately, the moon can be mapped.

The Army, too, has a stake in this success. Rocket missiles travel outside the earth's atmosphere, and it was not certain radar could track them. That doubt has been removed. Similarly, long-range jet or rocket missiles can now be radio-controlled from the earth.

On the more fantastic side, the experiment makes it possible to communicate

with the moon. With a little more power, the pulses can be modulated: made to carry signals or voice. Or, when the rocket ship comes to be, the passengers can keep in constant touch with the ground, before they set up housekeeping in the moon and build the first earth-to-satellite radio network.

Project "Diana," as the work was imaginatively named, began at Belmar in August, under the direction of Lieutenant Colonel John H. DeWitt. Colonel DeWitt's interest in moon-tapping was not a new one. In 1940, as chief engineer of Station WSM, Nashville, Tenn., he had tried the experiment on his own. But his equipment could not handle the work, and he failed.

His assistants were E. King Stodola, a graduate of the Cooper Union Institute of Technology and head of the research group at Belmar; Dr. Harold D. Webb, of Franklin, Ind.; Herbert Kaufman, of New Orleans, and Jacob Mofsenson, of New York, a graduate of City College. All but Mr. Mofsenson were radio men before the war; he was new at radio when he joined the Signal Corps and brought to his work the somewhat irrelevant training of a diamond salesman.

It was Dr. Webb and Mr. Kaufman who first detected the moon-echo. They decided not to risk the doubt of their fellows and waited twenty-four hours, when they could repeat the test in the presence of their fellows. The echo came back, and their verity was assured.

There is no doubt that these echoes come from the moon. The presence of the Doppler effect is one proof. This is the effect of motion on wave frequency. If a man throws rubber balls, one a second, at a wall, they will bounce back at the rate of one ball a second. But if the wall is moving toward the man, the rate at which the balls bounce back will be faster. If the wall is moving away, the rate will be slower.

The Doppler effect has a bearing on the moon-echo because, at moonrise, the earth is traveling toward the moon at a rate of 750 miles an hour, and at moonset is traveling away from the moon at the same rate. Further, the moon's orbit is not exactly circular, and hence at all times it is traveling away from, or toward, the earth as a whole.

Therefore, radio waves to the moon do not come back at the same frequency as that at which they are sent. To receive the echo this frequency adjustment must be made. It checks exactly with theoretical calculations.

The Signal Corps announcement was made simultaneously in Washington and at the annual meeting of the Institute of Radio Engineers, in New York. In the New York press conference all those who took part in the original research explained their work to the press.

They were careful to make no Buck Rogers predictions. "We are working now on more powerful sets," Colonel DeWitt said. "But it does not look now as if we will be able to go beyond the moon. These sets," he added, "are powerful enough to send a signal to Mars. But the receiver would have to be on Mars to pick it up. We haven't enough power to get an echo."

Mr. Stodola explained that success could be traced to the tremendous sensitivity of the receiver, which is sixty times as sensitive as the ordinary radar receiver. Operating on an extremely narrow band, it can pick up energy equal to one candlepower at the distance of the moon.

The moon radar differs from conventional radar in several ways, although like all radar it works on the principle of pulse transmission. But where conventional radar uses a pulse of extremely short duration—perhaps twenty-millionths of a second—the moon-set uses a one-half-second pulse. Thus, each pulse carries tremendous energy. Conventional radar sends out thousands of pulses a second. This

set sends out but one pulse in four or five seconds. To prevent the set from being burned out from the tremendous energy it transmitted, water-cooled vacuum tubes are used.

A double-sized conventional antenna is used, with sixty-four dipoles, giving a power gain of 200. A specially constructed "peep-sight" aims the antenna at the moon, and pulses are sent out on a frequency of 111.6 megacycles.

The five men who made the test work were introduced to the press, and later to the Institute of Radio Engineers, by Major General George L Van Deusen, chief of engineering and technical service, Office of the Chief Signal Officer. To the engineers he described some of the technical details, including the surprising fact that, basically, the set was no different from "the model SCR-271 radar set which was successful in detecting the approach of Japanese planes at Pearl Harbor."

In reaching the moon, the science of radar completes a full circuit. It was this same effort to reach the moon, made by Sir Robert Watson-Watt, that was interrupted by a strange disturbance which occurred at the same time every night. Sir Robert investigated, and learned an important fact.

The disturbance, he discovered, was actually caused by a plane coming into Croydon Airport, near London, on a nightly schedule. Radar was born with that discovery, for if an echo could detect a plane, it was a valuable defense weapon.

In discussing the possible uses of the moon-echo, Colonel DeWitt pointed out that it could be picked up by the proper receiving set anywhere in the hemisphere. He made the fanciful suggestion that if regular transmission between New York and Argentina were to be interrupted, the moon-echo would be available to serve as a channel by which messages could be sent, with a delay of 2.4 seconds. There would be no charge for rerouting, he said.

in the field by offering certain suggestions regarding the many problems that are presented, but most of all to intelligent laymen who may be encouraged to exert their influence to the end of setting up a more respectable defense than has ever yet been offered against the menace of schizophrenia.

The nature of the subject matter and the excellence of its presentation should guarantee the consummation of this hope.

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A SCIENTIST'S TOOLS

Scientific Instruments. Herbert J. Cooper, Ed. 304 pp. Illus. $6.00. Chemical Publishing Co. Brooklyn. 1946.

THIS book includes contributions from fifteen authorities in the various lines treated and has been ably edited by one of the contributors, Herbert J. Cooper, B.Sc., A.R.C.Sc.I., A.M.I.E.E. The book is wholly British in its make-up, but the American reader need not be too disturbed by the statement on the first page of Chapter I that "... it can be safely said that British optical glass and British design work are the best in the world."

A work of this kind cannot be expected to be all-inclusive. The editor faces the impossibility of such an achievement when he says in the preface:

> The book is in no sense meant to be an exhaustive treatise on instruments, but it is hoped that it will be valuable to the student and to the research worker, as well as to many people who are using scientific instruments and require a working knowledge of them without going into the details of design.

Every reader will doubtless regret the omission of some instruments in which he is particularly interested, as the reviewer regrets the omission, for example, of such as the planimeter and the photoelectric spectrophotometer, now so widely used in analytical laboratories.

The book is divided into 5 sections, 4 of which deal with optical instruments, measuring instruments, navigational and surveying instruments, and liquid testing. The fifth section is labeled Miscellaneous and deals with acoustical instruments, calculating machines, hardness indicators, and thermionic valves. There are 270 illustrations, practically all of which, especially the line drawings, are of excellent quality. The text is, in general, very well written. The reviewer noted but 11 typographical errors, all of little consequence except perhaps for the misspelling of the name "Langevin" in one place. One figure was slightly incorrect in its drawing, and two others contain faults in labeling. There is a 9-page index, in which only one error was noted in a cursory examination.

The book contains a wealth of material and will undoubtedly prove of value to the type of reader for whom it is intended.

C. M. SMITH

Silver Spring, Md.

Comments and Criticisms

MATHEMATICAL ALLUSIONS IN POE

As an admirer of Poe, it was with pleasant anticipation that I turned to page 227 of the September MONTHLY to read the article on "Mathematical Allusions in Poe" Imagine my disappointment on learning that (according to Professor Wylie) my hero was not a mathematician

For some minutes after a first reading, I sat in humble resignation, wiping away copious tears of disillusionment Then, with great fortitude, I reread the article more carefully. This second reading led to the happy conclusion that the author had not proved that Poe was not a mathematician. In this connection, permit me to call your attention to a serious slip on the part of Professor Wylie: he has confused Poe's use of what is not today a conventional term with misconception (on Poe's part) of a fundamental concept.

On page 230 of the MONTHLY, Poe is quoted as follows "The form of the moon's orbit being an ellipse of eccentricity amounting to no less than 0 05484 of the major semi axis of the ellipse itself, and the earth's center being situated in its focus . ." Wylie then says: "In this, although the decimal itself is correct, there is a fundamental misconception that cannot be argued away . . . , for the eccentricity of an ellipse is not expressed as a fraction of the major axis of the figure at all. In fact, it is not a length of any description but a dimensionless combination of both the semimajor and semiminor axes a and b, respectively, defined by the expression

$$\text{eccentricity} = \frac{\sqrt{a^2 - b^2}}{a}."$$

Today, it is almost universal practice to define "eccentricity" as a ratio rather than as a length; but there are exceptions. In Baker's *An Introduction to Astronomy*, 1935, we find on page 52 "The eccentricity of the ellipse denotes its degree of flattening It is represented by the fraction of the major axis that lies between the two foci " This is identical with Poe's use of the term. Again, in *Simon Newcomb's Astronomy For Everybody*, 1932, we find on page 127: "The sun is not in the center of the ellipse but in a focus, which in some cases is displaced from the center by an amount that the eye can readily perceive. This displacement measures the eccentricity of the ellipse . . . " A "displacement" is a length and not a dimensionless number.

So much for present-day usage. Now for a *pièce de résistance* which Poe could hand us if he were alive; it raises the sad fact (mentioned more than once in issues of the MONTHLY) that too many scientists and mathematicians are ignorant of classics pertaining to their own subjects.

Poe's use of the expression "eccentricity" is in exact agreement with Laplace's use of the term. In the celebrated *Mécanique Céleste*, Vol. I, An. VII, we find, beginning at the bottom of page 113: *"Les orbes des planètes sont des ellipses dont le centre du soliel occupe un des foyers; or si dans l'ellipse, on nome* $\bar{\omega}$ *l'angle que le grand axe fait avec l'axe des* x, *si, de plus, on fixe au foyer, l'origine des* x, *et que l'on désigne par* a, *le demi-grande axe, et par* c, *le rapport de l'excentricité au demi-grande axe; on aura*

$$r = a(1-e^2)/[1 + e\cos(v-\bar{\omega})], \ldots "$$

The e in this equation is identical with the dimensionless e which Professor Wylie calls the "eccentricity " But with Laplace e is the ratio of *l'excentricité* to the semimajor axes. Therefore, for him as with Poe, the eccentricity is the actual length from center to focus

To dispel any doubt as to the significance of the above quotation from Laplace, it may be added that as late as 1835, in his *Exposition du Système du Monde*, page 10, Laplace gives the definition *"la distance du centre a l'un des foyers, est l'excentricité de l'ellipse "*

If Poe read Laplace's works, he must indeed have been a mathematician of high caliber. However, I do not pretend to have proved that he was. My thesis i that the article in the MONTHLY fails to prove the contrary.—EDWARD C MOLINA.

HUMANISM

A letter of Mr. E. D. Myers (SM, August 1946) characterizes a letter of mine (SM, April 1946) as "uninformed" and "naive."

It is said to be uninformed because, whereas I contend that "from any objective modern standpoint it must be conceded that the moral content of religions is the only lasting contribution that they have made to man's advancement," A. J. Toynbee's *A Study of History* is claimed to demonstrate that "several religions have furnished the social institutions and organizations within which several civilizations have come into being." Even if it be

a fact that I have underrated the survival value of past contributions of religions to man's advancement, that does not vitiate my conclusion that "however important the myths, fables, and folklore which make up the supernaturalist trappings of religions and encrust their ethical ideals may have seemed in the past, *they have now ceased to have any further social value.*"

But it is by no means certain that I have so underrated any such contributions, because their ephemeral quality is clear: Some of the civilizations founded thereon have already passed from the scene; and the institutions and organizations of others have profoundly changed or have become obsolete under the impact of cataclysmic events and revolutionary knowledge. When Toynbee arrives at the thirteenth and final part of his monumental work—of which so far only five parts (in six volumes) have been published—it may well be that, from the long-range viewpoint of the historian, he will conclude in agreement with the writer that the moral content of religions *is* the only *lasting* contribution they have made to man's advancement.

My letter is characterized as naive because my suggestion that Christians and Jews discard all but the ethical elements of their faiths would, it is asserted, "not only be impossible but would achieve none of the results described" by me.

Impossible? Those most closely in touch with the situation, our ecclesiastics, apparently do not share this opinion as they ceaselessly warn us that the astounding growth of secularism is the peril of our age. Understandably, they dislike secularism because secularism neatly dispenses with their services in that it rejects supernaturalism and considers moral codes as natural human developments rather than as "divinely ordained." Millions, formerly adherents of the two faiths, have already had the good sense to adopt the secular outlook; my suggestion is simply that those still remaining in the faiths do likewise—and, according to the apprehensions of our ecclesiastics, that is what they fear will happen. Such action, which would dispel the age-old religious antagonisms of those professing the two faiths, could, I submit, not fail to have a favorable effect in harmonizing their relationships.

It is charged, furthermore, that I am "deeply committed to current superstitions about 'objective standpoints,' the supremacy of 'modern science,' 'rationalism,' and—perhaps the callowest of all—'humanism' (in the Bahm sense)."

As to "objective standpoints," I note that my critic, oddly enough, is also "deeply committed" to this "current superstition" because he characterizes Toynbee's view, upon which he relies heavily for support, as "*one of the most objective modern standpoints*."

Apparently, however, I do not share the company of my critic in my commitment to the "current superstitions" about "the supremacy of modern science." Bahm (whose views seem to be even more distasteful than mine to my critic) clearly points out (SM, April 1946) that our traditional faiths are religions based upon "ancient science" and that "the cultural lag in our ecclesiastical systems" results from the fact that many religionists still "insist upon the truth of centuries-old and now-obsolete science." As the "supremacy of modern science" over ancient science is hardly open to challenge, it appears self-evident that religious views based upon modern science are likewise superior to those based upon the primitive concepts of ancient science.

As to the "current superstitions" about "rationalism," "rationalism" has been aptly defined as: "the mental attitude which unreservedly accepts the supremacy of reason and aims at establishing a system of philosophy and ethics verifiable and independent of all arbitrary assumptions or authority"; while "superstition," according to the *Century Dictionary*, is: "An ignorant or irrational fear of that which is unknown or mysterious; especially, such fear of some invisible existence or existences; specifically, religious belief or practice, or both, founded on irrational fear or credulity; excessive or unreasonable religious scruples produced by credulous fears." As "rationalism" is manifestly an antonym rather than a synonym of "superstition," it is difficult to understand how any commitment to "rationalism" can involve "superstition." However that may be, as we are living in the intellectual climate of the twentieth century—rather than in that of ancient or medieval times—I scarcely need offer an apology for basing my views upon reason.

And, finally, and apparently most heinous, is my "commitment" to "humanism" (in the Bahm sense), i.e., "*science*"—the *modern* kind—plus "*values*," alleged to be "the *callowest* of all" the "current superstitions" to which I am committed. I learn from careful investigation that the mature religions (as contrasted with such a "callow" or immature religion as "humanism") abound in such unimpeachably *non*superstitious dramatis personae, histrionic properties, and practices as: gods (single and triple), devils, true gods, false gods, tribal gods, father gods, mother goddesses, son gods, ghost gods, angels in good standing, fallen angels, demons, witches, spare ribs, serpents seductive, serpents from rods, blood sacrifices, circumcisions, chosen peoples, outlanders, arks, covenants, stone tablets, high priests, lowly prophets, pregnant virgins, wise virgins, foolish virgins, saviours, destroyers, saints, heavens, hells (and auxiliary cleansing establishments), water turned to wine, wine turned to blood, bread turned to body, re-

surrections, ascensions, descents, polygamists, celibates, infallible men, fallible women, books verbally inerrant and of plenary inspiration, books canonical, books apocryphal, the worship of gods, the eating of gods, prayers, exorcisms, benedictions, maledictions, aqueous treatment by immersion and aspersion, oily treatment by anointment or inunction, absolutions, indulgences, fastings, feasts, obeisances, genuflections, ikons, relics, stars, crosses, chrismons, medals, beads, amulets, etc., etc., etc.

Of course, it is a question of personal tastes, and my tastes, I concede, are—like my suggestions are said to be—"foolishly simple." Thus, as the above represent maturity in religion, it is easy to understand why *I prefer mine "callow"*!—HAROLD R. RAFTON.

SCIENCE BOOKS FOR THE NONSCIENTIST

Your September issue has a list of "The Ten Most Important Books in the History of Science," by Justus J. Schifferes. I have seen many peculiar lists of "greatest" and "most important" books, but this one is in a class, a very low class, by itself.

Of the nine books actually listed, three have to do with medicine. There is here a far from subtle reflection of the author's prejudices. Worse still, of these same nine books, no fewer than six are by English authors. Under the guiding hand of Mr. Schifferes, the history of science becomes practically the history of England.

Consider a few of the books that Mr. Schifferes introduces as "turning points in the history of science." Bacon's *Novum Organum*, for some mysterious reason, is customarily worshiped as the work of a great mind, of a great systematizer of all knowledge. But in 1620, the publication date Mr. Schifferes gives, there was little material to systematize—and Bacon did not necessarily know all of that little. Chemistry, geology, botany, and other great branches of science were more in embryo than in infancy. Hence, Bacon's work could have little influence on working scientists, who were forced to labor painfully and very often blindly to gather the facts which now figure in the first few pages of our various textbooks. When science had enough material at its disposal for genuine systematization, Bacon's schemes were so far behind the times as to be completely irrelevant.

Malthus' essay on population is another excellent example of a far from excellent choice. Malthus did influence Darwin, but no more than Lyell, whose work on geology was better entitled to a place on the list. (If Mr. Schifferes had substituted Lyell's name, I should have had no complaints that he was an Englishman.) In essence, however, Malthus' magnum opus had little to do with science or the methods of science. It served mainly, not as a "turning point in the history of science," but as an apology for the nineteenth-century gentlemen who hailed war, famine, and disease as benevolent influences in human history. If it turned anything, it was the stomachs of the genuine students of population.

Jenner's book on vaccination and Descartes' *Discours*, on the other hand, are genuine contributions, but they are minor turning points, not major. Jenner's work, for example, does not compare in general scientific importance with that of Pasteur, unrepresented, possibly because he did not influence science by means of a book. Mr. Schifferes might certainly have omitted both these works.

What could he have substituted in their place? I know that some of the greatest contributions to science have not been in the form of books. Still, there were many possibilities. Why not something by Aristotle? It seems to me that he had a slightly greater influence on science than Bacon. So did another ancient Greek known as Euclid. So did Galileo, who also wrote a book. There was Linnaeus, whose name is still dimly recollected by botanists, there were Lagrange and Laplace, there was Liebig's important contribution on agricultural chemistry. There were economists like Petty, Adam Smith, and Ricardo. (This field, where Mr. Schifferes, in its early stages, had an actual right to consider the history of science and the history of England synonymous, he seems to have overlooked entirely.) There was Engels, whose *Anti-Duhring*, unknown to most American scientists and dealing largely with subjects which many of us consider not related to science at all, has had tremendous influence on scientists in Europe. And, in our own times, there once did live a man by the name of Freud, who had a certain influence on psychology. Freud, for the record, did write books.

All these names seem not to have occurred at all to Mr. Schifferes. I grant that some of these books might be difficult to obtain in original editions and to preserve "under glass." Perhaps also a list of ten may be too short to permit an accurate picture of the development of science or may offer too great difficulties of choice. But it was Mr. Schifferes himself who settled on the beautifully round number, "ten." And, in any case, there is no justification whatever for his publishing the sort of list that distorts the history of science.—JOSEPH SAMACHSON.

The Brownstone Tower

Comparison of this issue with that of December will disclose minor changes in type face and sizes. These changes were made principally to facilitate production of the SM by our new printer. Copy for this issue had to be submitted to the printer about two months prior to our publication date, which is on the twenty-fourth of each month. It remains to be seen whether an occasional timely article can be published with less delay. Galley proof will be sent by the printer directly to the author, who will send his corrected copy to the editor and his order for reprints to the printer. Each author is entitled to 100 free reprints, but in order to get them he must order them from the printer on the form that he will receive. He may buy as many additional reprints as he pleases. Reprints of articles in a given issue will be made at the time the issue is printed and will be delivered within thirty days after the publication date. Subsequent orders for reprints cannot be accepted by the printer.

We hope that the reprints to be provided gratis will serve as an incentive to those who might write for the SM. In addition, the author of an outstanding article may see his paper appear again in a digest magazine or an anthology. The SM, which holds the copyright, customarily receives a check from the publisher who has been granted permission to reprint the chosen article. The check is then made payable to the author, who is thus both honored and rewarded. Articles chosen for reprinting are likely to be those that attracted attention among the readers of the SM. For example, the essay by R. W. Gerard on "The Biological Basis of Imagination," which excited the admiration of the editors and of many readers, was so chosen. It was a distinguished contribution to Volume 63. But the most popular article we have published in the past three years, as indicated by requests for reprints, was "The Mathematics of Committees, Boards, and Panels," by Bruce S. Old. If the readers had been polled for the purpose of selecting a prize-winning paper, Dr. Old would probably have won by a majority.

Another article selected for reprinting was a contribution to Science on the March by plant pathologist K. Starr Chester, "Victory on the Potato Front." Although we feel that we have published some excellent articles in this section, we must admit that it has not worked out fully as planned. We had hoped, perhaps unreasonably, that each contributing editor would serve as a reporter of advances in his field of science and would supply three significant contributions each year. As some contributing editors have been unable to do so, we feel that Science on the March has not adequately covered the advancing front of science. Therefore, beginning with this issue, the section Science on the March is discontinued. It will be resumed only if arrangements can be made to assure coverage of significant advances in the various fields of pure and applied science. Short articles such as have been published in Science on the March during the past three years are still wanted for publication as principal articles. Perhaps an annual review of progress in each of twelve major fields of science would more nearly spell Science on the March.

F. L. Campbell.

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2,4-D, A POTENT GROWTH REGULATOR OF PLANTS

By H. B. TUKEY

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THE plant growth regulator and herbicide 2,4-D has all the earmarks of being to the plant world what the insecticide DDT has been to the insect world. It appeared with suddenness, it has been remarkably effective, it is easily manufactured, it is relatively inexpensive, and it is safe and easy to handle. Its possibilities are only beginning to be explored.

But it is not alone the material, 2,4-D, that is important. It is what it represents. It represents a chemical attack on weeds, not by caustic agents that kill the plant by contact but by means of materials which regulate the growth of a plant and cause it to behave in such a manner that it is no longer suited to its environment. It is an effective approach toward weed control and promises results of far-reaching importance to agriculture throughout the world.

What, then, is 2,4-D? What are its properties? How is it used?

Correctly, 2,4-D is the abbreviation for 2,4-dichlorophenoxyacetic acid, which, as the name implies, is a halogenated phenoxy compound containing two chlorine atoms substituted in the 2 and 4 positions and having an acetic acid side chain. In popular usage, however, the symbol "2,4-D" has been used to mean any preparation that contains 2,4-dichlorophenoxyacetic acid or any of its salts, esters, amides, or related compounds. Thus, some manufacturers sell the water-soluble sodium salt of 2,4-dichlorophenoxyacetic acid, some sell the oil-soluble methyl ester, others sell the water-soluble ammonium salt, and still others sell the amide. Each formulation has certain virtues which the manufacturer extols, but all depend for their effectiveness upon their derivation from 2,4-dichlorophenoxyacetic acid.

2,4-D is manufactured by the chlorination of phenol (carbolic acid) to give a mixture of compounds with chlorine substituted in various positions in the ring. Fortunately, a high proportion of the mixture has chlorine in the coveted 2,4-positions (2,4-dichlorophenol) and is readily separated from the others by distillation. The next step is to neutralize the phenol compound with sodium bicarbonate to produce a sodium salt of 2,4-dichlorophenol, which is then combined with chloracetic acid to give 2,4-dichlorophenoxyacetic acid. The ease of preparation of 2,4-D and its low cost make it attractive for large commercial operations.

Its physical properties, too, enhance its usefulness. When refined it is a white powder with no offensive odor or caustic or corrosive action on skin or container. It seems nontoxic to animals at concentra-

tions commonly applied. Cows have been grazed on pastures sprayed with it with no harmful effects. Although found in the blood stream of the animal, it has not appeared in the milk.

2,4-D is applied in various ways. The acid itself is soluble in water only with difficulty. It must first be dissolved in alcohol and then in water or in some other solvent, such as one of the polyethylene glycols (Carbowax 200, 900, 1,500, etc.) which have proved so effective. The sodium and ammonium salts are, however, water-soluble and are generally as effective as the acid. Various esters are soluble in oil and may be used with formulations that involve oils, as emulsions.

Commonly, the active ingredient is applied in a water spray at the rate of 1 part per 1,000 by weight for herbicidal purposes. This very low concentration is in contrast to those of 1 to 10 and 20 more commonly met with in agricultural sprays. The trend is to use the material evenly distributed over a given area in more concentrated form so as to reduce the bulk required. This calls for refined apparatus and care in making treatments. Application in aerosol form, as when dissolved in dimethyl ether or some other liquefied gas, has proved effective, just as has DDT in aerosol bombs. For pastes and salves and other heavy concentrations for special purposes, lanolin may be used in combination with other solvents involving concentrations as high as 1 to 100 or 1 to 200.

But how did this come about? Who discovered 2,4-D? Who developed it?

As in any discovery and development of this kind, a number of individuals and laboratories have been concerned. If someone is omitted from the discussion which follows, it is not an error of intent.

It all started with growth regulators themselves. In a sense, any material that affects a plant may be called a regulator of growth, but by common agreement the term "growth regulator" has been left for a group of chemicals which do not enter into the make-up of the plant, as do fertilizers, but which in minute amounts produce characteristic changes or effects in growth, such as curvatures of stems and abnormal "formative" developments of foliage.

Hormones were recognized in animals before they were in plants. Beginning in the 1920's with the work of Boysen-Jensen, it became established that there were hormone-like materials in plants. Kögl and Haagen-Smit in 1937 were the first to isolate and identify one of these materials from plants. In the course of their investigations they identified indoleacetic acid as having growth-regulating properties. A great exploration began immediately among the lists of chemicals for others which had growth-regulating properties. Many workers in many countries were active in this field in the 1930's. Included in the group in this country were Went, Thimann, Haagen-Smit, Bonner, Van Overbeek, Avery, Burkholder, Zimmerman, Hitchcock, Kraus, Mitchell, Hamner, Marth, and Skoog. Zimmerman and Hitchcock, of the Boyce Thompson Institute for Plant Research, especially, kept methodically at the task of testing old materials and synthesizing and testing new ones.

Among the chemicals which were tested was one known as 2,4-dichlorophenoxyacetic acid, which was included in a list of materials having growth-regulating properties in a patent issued to Lontz and assigned to E. I. du Pont de Nemours and Company. It proved to be very potent in its regulating activities—so potent, in fact, that it was often injurious and was recommended to be used with caution.

During this period of activity in the pursuit of plant growth regulators, various practical uses were discovered, such as the rooting of cuttings and the prevention

of preharvest drop of apples. As early as 1941, however, it had been specifically suggested and demonstrated by Kraus, of The University of Chicago, that growth regulators had herbicidal properties and that if potent enough and cheap enough could be used in commercial practice. He initiated work at The University of Chicago with Beal and Hamner and with Mitchell, Hamner, and Marth at the U. S. Department of Agriculture. In 1944 he finally convinced the Chemical Warfare Service of the possibilities. Under the direction of Norman, a project was organized at Camp Detrick, under security policies, which included work in the U. S. Department of Agriculture and at Ohio State University. Among those identified with this work were Thompson, Swanson, Weaver, De Rose, Smith, Minarik, Boyd, Ennis, Allard, Taylor, Newman, Mitchell, and Brown.

In England, at the same time, work was going on independently and along similar lines in the laboratory of Blackman. Researchers included Nutman, Thornton, Quastel, Slade, Templeman, and Sexton.

To CONTINUE the story, in early 1944 Beal had published results of laboratory tests which suggested herbicidal possibilities with 2,4-D, and in the spring of 1944 Mitchell and Hamner, also from laboratory work, had published similar findings. In the summer of 1944, a paper appeared by Hamner and Tukey which reported successful field trials with 2,4-dichlorophenoxyacetic as a herbicide on bindweed and which spurred activity in this field of investigation on the part of those not bound by security regulations.

During this time Jones, of the American Chemical Paint Company, had been carrying on field trials independently. In late 1945 he was issued a patent on the material as a herbicide and vigorously pushed its manufacture and introduction to use. Other commercial companies took up the work, so that within a year production was measured in thousands of pounds rather than in grams, and the price dropped from $125.00 a pound to $3.00. Within less than 2 years, the commercial value of 2,4-D reached into millions of dollars.

Publications would have appeared likewise from the group working at Camp Detrick and from other laboratories had it not been for security regulations which prevented. These are now appearing and should be given full credit when history is finally written.

Field work in 1945 and 1946 from widely separated parts of the country and from Europe indicated great success with 2,4-D as a herbicide. The material seemed to move rapidly into the plant, to be transported in the direction of movement of synthesized material, and to be favored in its effectiveness by photosynthetic activity. Accordingly, greatest responses were secured from plants in sunshine. The action was the arrest in development of some parts of the plant and the stimulation of cell division and cell enlargement in other parts, especially in those regions which are young, active, and meristematic, such as cambium. Respiration was markedly increased, and starch reserves were depleted. The plants became bent and twisted and otherwise showed growth responses which were finally associated with inhibition and death. In some instances, plants were killed without apparent acceleration of growth in any part.

The below-ground parts of bindweed and sow thistle became proliferated, split, and decayed in the course of a week to 10 days. The pollen of flowers shriveled and became nonfunctional, leading to the use of 2,4-D to destroy ragweed pollen in an attempt to reduce hay fever. Many other plants, such as lamb's-quarters and pigweed, were killed.

Also, the material had been found selective in its action. For example, on blue-

grass lawns, infested with dandelion and narrow-leaf plantain, the dandelion and plantain were killed without injury to the grass. In general, it was found that grasses and cereals were not affected by concentrations that were toxic to broad-leaved plants; Bermuda grass proved an exception. This led to trials of grain fields with remarkable success. Wild radish, mustard, and similar weeds were eliminated from fields of young oats and wheat without injury to the oats and wheat. Rice was weeded by dusting from an airplane. The possibility was thus opened for wholesale treatment of cereals, including corn, although it was found that treatment must be made before the flowers formed in order to avoid injury to them. Alligator weed was killed in fields of sugar cane; water hyacinth was killed in ponds and ditches. With the ending of the war, some of the information which had been accumulated concerning the value of 2,4-D as a potent weapon in biological warfare was released.

2,4-D was found also to affect woody plants. As with herbaceous plants, it was found most effective in periods of active plant growth, in sunlight, at high temperatures, and in quantities sufficient to compensate for the size of the plant. That is, a small tree required less material than a large tree for a similar degree of response. Poplar trees which had been scored with a hatchet and treated on the cut surfaces with a paste or salve of 2,4-D split upwards for 30 feet. The bark proliferated and became spongy and soft and sloughed off. Pine trees similarly treated showed severe bending of the new growth and exudation of gum. Peach and cherry trees were killed by such treatment; exudation of gum from dead areas scattered over the entire tree followed. Elm trees were found sensitive, whereas hawthorn and juniper were less sensitive, and horse chestnut and oak were quite resistant.

Suckers from plum roots became twisted and finally died within a 3-foot radius of the cut surfaces of stumps to which a salve of 2,4-D was applied. Mesquite was killed by cutting close to the surface of the ground and then treating the new sucker growth with 2,4-D. Power companies and railroads found the material very effective against woody plants and climbers. By first cutting the plants and then treating the new sucker growth with 2,4-D, even stubborn woody plants were killed back into the roots and eliminated.

These were some of the results with 2,4-D which came rapidly to the front, extending all the way from selective lawn treatments to destruction of weeds in grain fields, tree eradication, clearing of rights of way and hedgerows, and actual clearing of land for farming.

A relatively new suggestion for 2,4-D is as a destroyer of weed seed in the soil. That is, instead of waiting for weeds to grow above ground and then treating them with a foliage spray, the 2,4-D is applied to the soil. Germinating seeds are more sensitive to 2,4-D than are seedlings or mature plants. Even grass seed may be inhibited and killed. After a period of time, depending upon the nature of the soil, the temperature, and the moisture, the 2,4-D leaches away or disintegrates, so that desired crops may be planted with no ill effects. The amounts applied have varied from 3 to 5 to 10 pounds per acre. Applications of 100 pounds per acre have freed soils of weed seed, but the toxic effect has persisted and the cost has been excessive. The period of time that must elapse after applications of the smaller amounts may vary from 2 to 8 weeks to over 6 months under cold or arid conditions. Some crops, such as peas and beans, are

more sensitive to residual 2,4-D than are others, such as corn and certain ornamentals. Results have been most satisfactory on sandy soils and least effective on muck soils. The details of treatment are yet to be worked out, but the idea appears promising.

Finally, 2,4-D has been found effective for uses other than as a herbicide and for killing weed seeds. If used with care it will effectively retard the preharvest drop of fruit. It can be used to promote the rooting of cuttings, especially those materials which root with great difficulty. It has been used to hasten the ripening of fruit. It will cause tomato flowers to set as fruit. It will reduce scald of apples in storage.

Undoubtedly other materials will be found that will enlarge the usefulness of growth regulators. The Camp Detrick group and their cooperators made and tested over 1,000 materials for their effect on plants. None has yet been found as generally potent or available or useful as 2,4-D, but possibilities are suggested and much has been learned. Some have been found, for example, which are effective against grasses and not so effective against broad-leaved plants—just the reverse of 2,4-D. And the shifting of only a single chlorine in the ring is shown to alter the effectiveness of the compound greatly.

All in all, the chapter dealing with 2,4-D and growth regulators is a fascinating one in the story of scientific discovery and development. It is one of those peculiar and too infrequent discoveries and developments in science in which many things have seemed "to go right." If a material is effective, it is often expensive, not available, difficult to manufacture, toxic to humans, explosive, or difficult to handle. But here is a compound which first proved effective against a few plants and selective in action, then proved effective against many plants, then proved not dangerous to humans at concentrations commonly employed and a "pleasure to handle," then proved so remarkably simple and inexpensive to manufacture as to become "the cheapest herbicide available today," and finally proved to have other uses than as a herbicide.

ESKIMO INFANTICIDE

By CLARK M. GARBER

Butler, Ohio

SURPRISING and tragic as it may seem, many Eskimo babies get only a brief glimpse of their mothers and the world's bright light before their barely-started existence as mortals is unceremoniously brought to an end. In other words, Eskimo mothers, under stress of unbearable economic conditions or through whims that have no foundation in reason, are known to destroy their babies. This merciless deed is accomplished in any one of several ways such as freezing, drowning, suffocation, etc. Girl babies are the usual victims because it is recognized that boy babies will grow up to become producers and providers.

Many of the world's prominent students of Eskimo ethnology have recorded their findings on this subject in widely scattered paragraphs of long out-of-print publications. I have collected all the important writings of an authentic nature on this subject and present them with my own observations and experiences for the enlightenment of the reader.

Jenness (pp. 165–166), in his illuminating discussion of the Copper River Eskimos, says:

> Often the parents are unwilling to rear their children, for a baby involves much hardship to the mother, especially in the summer when all the household goods are packed on the back. The child is only an additional burden to the mother up to the age when it can make a long day's journey on its own feet. Frequently the parents settle the problem by simply suffocating their baby and throwing it away.

In writing of his experiences and observations among the Alaska Eskimos, Dall (p. 139) says:

> Infanticide is common among them, both before and after birth. As an excuse, they say they do not want and cannot support so many daughters. Other women do not like the care and trouble of children, and destroy them for that reason. The usual method is to take the child out, stuff its mouth full of grass, and desert it.

Likewise, Dr. Sheldon Jackson (p. 115) mentioned the practice of infanticide among the Eskimos of Alaska. He said:

> Female infanticide is common among some of the tribes, particularly the Mahlemute and those of the Yukon. Many Indian mothers, to save their daughters from their own wretched lives, take them out into the woods, stuff grass in their mouths, and leave them to die.

Also, Dr. Boas (p. 580), in his account of the Central Eskimos, has something to say on this subject. He writes:

> Among all the tribes infanticide has been practiced to some extent, but probably only females or children of widows or widowers have been murdered in this way, the latter on account of the difficulty of providing for them.

Murdock (p. 417) is probably responsible for the only negative statement on this subject. He said:

> We never heard of a single case of infanticide, and, indeed, children were so scarce and seemed so highly prized that we never even thought of inquiring if infanticide was ever practiced.

This was probably just an oversight on his part, for had he taken the pains to investigate the possibility diligently, doubtless he would have found the Barrow Eskimos secretly taking the lives of their babies, especially girls.

In times of plenty very few cases of infanticide can be found. Famine and starvation, however, are not the only causes leading to infanticide. Occasionally, it happens that a fickle, mentally deficient

woman will give birth to a baby she does not want. In such cases there is a pronounced lack of motherly affection for the baby, and the mother may destroy her baby in a most diabolical manner. I have in mind the case of an Innuit mother of a very low type who killed her baby and fed it to the dogs.

Among the Innuits and Utes of Alaska, I have found both drowning and freezing the *modus operandi* for the destruction of unwanted children. On one occasion, I had departed from the village of Sfanagamute with my dog team, directing my course toward Nichtmute, the next colony. About half a mile out of the village, along the tundra trail, I came upon an infant lying among the snow-covered hummocks near the trail. Had I not possessed a well-trained lead dog, the infant would have been torn to shreds in less time than it takes to tell it. Stopping the dogs and making the sled fast, I picked the baby up and discovered that its body was still warm with life. After cleaning the grass from its mouth with my fingers, I wrapped it in furs, placed it on my sled, turned the dogs about, and headed back to the village I had just left. There I sought out the witch doctor and commanded him to find the infant's mother for me. When the baby's mother was brought to me, I gave the child into her arms and told her to care for it and raise it. The mother's apparent happiness at the recovery of the baby that she had put out to die convinced me that she had resorted to infanticide under stress of some economic necessity, perhaps starvation, widowhood, or illegitimacy. My subsequent inquiry brought to light the fact that famine and starvation had prompted the deed. I gave her such foods as I could spare from the provisions on my sled and provided her with an order on the trading post for additional supplies. Today, that same baby girl is a young married woman with a family of her own.

With the coming of the white man's civil code, the practice of infanticide by the Eskimos is dying out or is performed in utmost secrecy. Doubtless this accounts for Murdock's failure to discover evidence of the practice at Barrow. However, there are still existent colonies in which the deed is committed more or less openly.

Reasons for infanticide, as explained to me by Eskimo women and witch doctors, are the rigorous economics of their existence. Starvation, famine, or even a slight scarcity of food are the preponderant reasons given. However, there are other motives no less important, although they may not be so frequently cited as the factor of food supply.

Physical defects in newborn babies and multiple births often force the Eskimo mother to resort to infanticide. If a child is born physically deformed, it rarely survives the day of its birth. The general attitude toward deformed children is that they are simply no good; that they become helpless dependents and require ceaseless care to keep them alive; yet they are unable to contribute even the smallest effort toward the family's or colony's welfare. Furthermore, a child born deformed has little chance to stimulate any sort of affection in the breast of its primitive mother. In fact, the mothers of deformed children possess a distinct hate for their offspring and have not the least hesitancy in the matter of their destruction. During my long and intimate life with the western Eskimos, I found only two cases of living deformed children. In the village of Tanunak on Nelson Island, there lived a woman whose every child was born with its feet drawn back against its buttocks. By reason of missionary influence, these badly deformed children were allowed to live, and today they may be found hobbling about the village on padded knees. The other case, that of a clubfooted child, was found in the village of Kipnuk. It, too, was spared the inevitable fate of children born deformed.

Monstrosities have undoubtedly appeared among the Eskimos as a result of terrible malformations at birth. In the legends and stories of the western Eskimos there are many references to such monstrosities, most of them of the megacephalic type. The important or main theme in many of their stories and legends deals with children having heads of wolves, dogs, or seals, children with mouths extending from ear to ear, and children having the bodies and limbs of various animals. The fact that no such monsters are found living among them attests the efficacy of infanticide as a process of eliminating undesirables. An Eskimo child, in the settlements where primitive life still prevails, is indeed fortunate if born a male with sound and healthy body. Otherwise, the child's life probably would be short, especially if there were evidences of physical defects.

If twins are born to a western Eskimo mother and one of them happens to be a female, she is foredoomed to destruction. Twins are born to Eskimo mothers in about the same ratio as they are among the whites. However, one rarely, if ever, finds living twins among the Eskimos. Even though they do exist it is very difficult to identify them because they are usually separated at the time of their birth. If the twins are males, one of them is given away to some other family and promptly loses his identity because he then becomes a blood brother of the children of the adopting parents. In his discussion of this phase of infanticide, Jenness (p. 166) says:

> Where twins are born one at least must either be killed or given away, for an Eskimo woman cannot possibly rear both children at the same time. If one is a boy and the other a girl, it is invariably the girl that is made the victim. Boys, in fact, are seldom exposed, for they will support their parents when they grow up.

Some writers disclaim any knowledge of the practice of infanticide among the Eskimos. Perhaps they have not enjoyed the utmost confidence of the Eskimo people, which comes only after a long and intimate life among them. When they find it necessary to sacrifice the life of a child, it is not accomplished publicly, and rarely in a gruesome, murderous fashion. Particularly is this true if white men have established themselves in or near Eskimo colonies.

Let us examine the mental attitude of the average Eskimo mother who destroys her baby. Is she a murderess at heart? In some of the cases I have observed the mother has exhibited a pronounced love for her baby but had to squelch her affection in order that the older members of her family might continue their existence. On the other hand, some mothers have demonstrated a marked hardness and indifference toward the necessity for destroying their babies. Jenness (p. 166) likewise found this to be true among the Copper River Eskimos. He says, "A mother will do this, for apparently she has no spontaneous affection for her offspring at the time it is born." Fear of exposure may be an important factor in the killing of many unwanted Eskimo babies, although Eskimo girls who have babies out of wedlock usually rate only a severe scolding from their parents and are in nowise disgraced. But Stefansson (p. 215) states regarding the Cape Parry Eskimos, "Some women are afraid of their fatherless children and kill them at birth."

Although it is the child's mother who usually performs the actual killing, I have known cases in which the father either performed the deed or was instrumental in having it done. At the village of Kaskag on the Kuskokwim River, I knew a married man who had committed adultery with his stepdaughter. A child was born, and the father, having an intense dread of the penalty he might have to pay under the white man's civil laws, drowned the infant in the river. Under the Eskimos' primitive

status this deed would have gone unnoticed, or perhaps the child would not have been destroyed at all.

Naturally, the people of the civilized world look upon the primitive Eskimos' practice of infanticide as a diabolical thing. But, before we condemn them, let us consider well the cold, hard economics of their battle for survival and the manner in which many seemingly brutal practices are forced upon them by the exigencies of their very existence. Let us picture an Eskimo family or group of families living in a poor hunting ground or let us suppose a hunting season fails and leaves them stranded without any possible source of subsistence: they must then seek a more productive location. Such migrations, forced upon them during the long, severe winter, involve the transportation of the entire household. Utensils, bedding, tools, weapons, furs, hides, garments, and hunting and fishing gear must be hauled on the sled or carried on the back. Each member of the family must carry his share of the heavy load, and this does not exempt the women and children. If there should be a new baby in the family, the mother must decide whether or not the pack she must carry is more important than the infant. In most cases the decision is made in favor of the pack. Especially is this true if the infant is a female, in which case the child is put out on the tundra to perish.

Unfortunately, the primitive Eskimo apparently does not realize that there must be an adequate supply of girls if the increase and perpetuation of the race is to be assured. A persistent practice of infanticide has been known to produce a serious shortage of marriable girls, so that polyandry developed as a consequence. In one instance, I received word from the St. Lawrence Island natives that they were in need of a large number of marriable girls and was asked to send as many as possible from the mainland villages. Here is a concrete example of female infanticide carried to the extreme.

There is yet another phase of Eskimo infanticide that deserves special consideration. This is the destruction of the unborn by abortion. Throughout my close association with and medication of the Alaska Eskimos, I have not discovered a single case of voluntary abortion. If abortion does occur, it is more apt to be accidental than intentional. The very idea of destroying a child during gestation is repugnant to primitive Eskimo women. Not until they came into contact with the white man did they acquire the knowledge of how to destroy the evidence of their moral sins by prenatal infanticide. Several instances are known in which Eskimo girls were expecting to give birth to illegitimate children begotten by white men. To the uncorrupted, primitive Eskimo girls there is nothing evil in this sort of conception. But the white men, in order to save themselves from the penalties that would probably be visited upon them by the conventionalities of their own race, taught the girls whom they had wronged how to perform abortions on themselves. In many cases, such men have been known to pay dearly to have these abortions performed by others. Jenness (p. 167) says of the Copper Eskimos:

> One thing these natives have to their credit, however—they never resort to prenatal infanticide; I am not sure whether it is known even, although a native who quarreled with his wife when she was pregnant threw her down in the snow and rolled his foot on her stomach.

It cannot be denied, however, that abortions of an involuntary nature have occurred among the various groups of Eskimos from Alaska to Greenland. Dr. Boas (p. 426) found "abortion a contributing factor in the rapid diminution of the Eskimo population of Baffin Land." It is reasonable to believe that hardships, injuries, famine, and heavy work cause fre-

quent abortions among the pregnant women of Eskimoland. In fact, I have been called upon many times to treat Eskimo women for the hemorrhages resulting from involuntary abortion.

Eskimo witch doctors and medicine women, in their gross ignorance of the physiology of pregnancy and childbirth, sometimes, although unintentionally, cause expectant mothers to suffer unnecessary abortions. It is not uncommon for Eskimo women to consult the witch doctor for the relief of the common ailments of pregnancy. But the medicine man's ministrations are a far cry from the proper medical procedure in such cases. His method of treatment often takes the form of kneading the patient's abdomen with considerable force. Retching and vomiting are induced to remove the offending *jariax*, evil spirits, or devils, causing the sickness. This type of treatment is not only applied in cases of pregnancy discomforts; it is applied in many other kinds of sickness, so it cannot be labeled as a special treatment for producing abortion. The general attitude among the western Eskimos is that a pregnant woman has no desire to destroy the child within her body. To do this would constitute a serious offense against the spirit which controls the inspiriting of her unborn child. The Eskimos of the world inhabit the most difficult environment occupied by any of the races of mankind. They of course have no knowledge of the eugenics of childbirth and race perpetuation. Theirs is a blind, biological response to the rigid economic factors which control their lives from birth to death. Therefore, let us not judge them harshly for the strange and seemingly cruel customs and practices a merciless environment has forced upon them.

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AMINO ACIDS*

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AMINO acids are of fundamental importance partly because they are the molecular bricks of the proteins. There is no class of substance more intimately associated with the central processes of life than the proteins. Master hormones, antibodies, and enzymes are certainly proteins, and the genes almost certainly are. Many feel that the genes must figure importantly in a type of preventive medicine of the future; this is already true in the breeding of poultry which is resistant to disease and in the breeding of disease-resistant plants. Viruses, hemoglobin, membranes, muscle—even hair, skin, and nails—are proteins. The proteins are the building stuff of the body. They are also the agents that do the building and maintain the repairs. The substances which, in the laboratory, come closest to having the power of self-reproduction, are proteins. The proteins in turn are essentially larger molecular combinations of amino acids. This is the fundamental reason for the interest of the pharmaceutical and other industries in amino acids.

The amino acids which have been recognized as constituents of protein are almost two dozen in number. The earliest methods for their preparation depended largely upon hydrolysis, or chemical digestion, of proteins. Synthesis has been relied on increasingly for preparative purposes. Methods of manufacture must be selected on the basis of the specific purpose in mind. One very important criterion related to such selection is based on the fact that amino acid molecules are capable of existence in two forms, a left-handed and a right-handed form.

This relationship is quite analogous to the relationship between left-handed and right-handed gloves. It is a significant fact that amino acids which are found in nature are almost exclusively left-handed, or *levo*; the right-handed, or *dextro*, type is practically absent from protein. The human organism is capable of using only the levo form of many of the amino acids. When amino acids are isolated from protein by hydrolysis with acids or enzymes, they are of the desired levo form. When they are prepared by total synthesis they consist of exactly equal amounts of the levo and dextro forms. The latter are usually undesirable, which frequently means that synthesis has an inherent disadvantage. It is, however, possible to synthesize two pounds of some amino acids more cheaply than to isolate one pound and subsequently to employ two pounds of synthetic amino acid even though only one-half of it is active.

On one hand, cheaper methods of synthesis are continually being developed; on the other, isolation procedures are also undergoing improvement. When new proteins with unexpectedly high contents of certain amino acids are found, the costs of these amino acids are of course lowered. This sort of knowledge develops especially from systematic study of proteinaceous by-products; fish-processing fractions, for instance, have yielded pleasingly large quantities of some amino acids, such as arginine. It is interesting to note that the cheapest amino acid, glutamic acid,

* From an address delivered to the thirty-ninth annual meeting of the American Pharmaceutical Manufacturers' Association at Lake Louise, Canada, on June 10, 1946.

which is obtained from protein, is available in quantity at less than $1.00 per pound. The simplest amino acid to synthesize, aminoacetic acid, is on the market at over twice as much. Some of the sources of isolated amino acids seem also to be capable of improvement by crop breeding, somewhat as are other characteristics like acreage yield and blight resistance. In cooperation with the United States Department of Agriculture, we are carrying out in our laboratories in Iowa a project for increasing, by breeding, the content of lysine, tryptophan, and other amino acids in corn. In accordance with the belief that it is unsafe to predict whether synthesis or isolation will eventually provide the most economical methods of production for individual amino acids, we are also attempting to develop better syntheses of lysine and tryptophan. It is our intention, however, to follow up the breeding possibilities for each amino acid which may be of commercial value.

One reason for the interest associated with increase of lysine and tryptophan in corn is closely related to what has been the principal concern of the pharmaceutical industry in amino acids—as a therapeutic form of protein nutrition.

A little history of this subject may be in order. The tissue proteins are constantly wearing away and need to be replaced by fresh protein. In the normal human being, this is accomplished after digestion of food protein in the stomach and intestines. The resultant amino acids and small combinations of amino acid molecules, which are called peptides, are carried to various sites and are synthesized into the particular tissue, hormone, or other protein required. When there occurs in the digestive apparatus an abnormal condition such as intestinal obstruction, peritonitis, peptic ulcer, carcinoma, or nonfunction due to near-starvation, it is desirable to by-pass the usual digestive "disassembly line." Proteins such as casein and egg albumen which we get in normal food cannot be injected directly into the blood stream because they will produce serious reactions.

Human blood proteins can be injected, but they are quite expensive. One answer to this problem is to introduce into the blood the building units which are normally there after a meal and which are normally employed for the purpose—amino acids and peptides.

These principles were appreciated by the European biochemist Emil Abderhalden over thirty-five years ago. Abderhalden and co-workers, in one of their experiments, hydrolyzed a quantity of beef in the presence of enzymes from the gastrointestinal tract. The patient was one Konrad Gegner, a twelve-year-old boy who had drunk the contents of a glass of lye. The lye induced a stricture of Konrad's esophagus which finally prevented his taking food by mouth.

From September 1 to September 15, 1909, Abderhalden's hydrolyzate was administered to Konrad by rectum. During this period the patient retained nitrogen and actually gained a little weight. Abderhalden had demonstrated that human protein nutrition could be effected by nonoral administration of predigested protein. This seems to be the first record of protein hydrolyzate utilization by a human patient.

Abderhalden also realized that in his mixture some of the amino acids were critical. If tryptophan were destroyed, for instance, the resulting mixture was not properly retained by dogs.

It was not until twenty-six years later, when pure individual amino acids had become available in sufficient quantity, that McCoy, Meyer, and Rose, working with rats, determined just which amino acids are critical. Today eight amino acids are known to be required simul-

taneously in order that any of them be properly retained by adults. These eight are isoleucine, leucine, lysine, methionine, phenylalanine, threonine, tryptophan, and valine; they are known as essential, or indispensable, amino acids. The word "essential," used in this way, is not always interpreted in the manner originally intended. Many of the amino acids which are not called essential are actually indispensable to life. For instance, thyroxine, the amino acid which is substantially the active principle of the thyroid gland, is one without which we could not live. The body is capable, however, of making thyroxine from phenylalanine, a so-called essential amino acid. "Essential" in this connection thus means that the amino acid is required but cannot be synthesized by the body rapidly enough; it must be included in the diet. It is furthermore true that for over-all protein nutrition, a minimum amount of each of the eight essential amino acids must be present in the mixture.

Nor is this the whole story. Many investigators have observed that completely hydrolyzed protein is not equivalent in nutritional value to the original whole protein. It has recently been announced by Sprince and Woolley that many proteins liberate on proper partial hydrolysis a factor (strepogenin) which enhances the value of amino acid mixtures in nutrition. Strepogenin is not an amino acid itself; it appears to be a peptide intermediate between protein and amino acids. When added to mixtures of the amino acids, strepogenin increases their value to mice and rats. When the strepogenin effect has been adequately evaluated, the use of synthetic amino acids for protein nutrition will be closer at hand.

A REASON sometimes quoted for the rising interest in amino acids is the belief that they will have a history similar to that of the vitamins. There are similarities and there are differences. One important difference is in the amounts required. Spectacular cures of vitamin deficiency diseases are achieved with milligram amounts of vitamins. The necessary daily intake of amino acids is, however, several hundred times, in weight, that of the vitamins required.

Much work is going on in an attempt to determine specific amino acid deficiency states comparable to that for vitamins. This field can be expected to move ahead with the aid of the new microbiological assay methods of Max Dunn and others. Interest in individual amino acids is, however, not restricted to their use in treating deficiency states. Some examples follow.

The largest market that exists for a single amino acid is undoubtedly that for glutamic acid. The relatively pure monosodium salt of glutamic acid has been used in the Orient for about thirty years as a meat flavor and provides a considerable industry there. In the past decade monosodium glutamate has become popular in the United States. The resultant large-scale manufacture partly explains the low cost of this amino acid.

Glutamic acid, because of its basic amino group, combines with hydrochloric acid to provide a source of hydrochloric acid in which the strong acidity of the latter is masked. Glutamic acid hydrochloride therefore is used as a therapeutic agent for the introduction of gastric hydrochloric acid.

Glutamine is a glutamic acid derivative which is of interest because it has the natural function of detoxifying certain organic acids. Glutamic acid, because of its low price especially, is also of potential value as a chemical intermediate. In our laboratory we have been able to synthesize plant hormones and weed-killers of an otherwise expensive type from glutamic

acid, and it appears that it will be more feasible to obtain a new series of compounds in this family.

Glutamic acid thus presents accomplished and potential utilities as an industrially useful material, as a chemical intermediate, as a biochemical intermediate, and as a pharmaceutical agent. These utilities, plus perhaps that of curing deficiency states, represent some of the chief ways in which individual amino acids are likely to develop markets.

Another amino acid of value for combating toxic substances of various sorts is aminoacetic acid, or glycocoll. This is known scientifically as glycine, but is labeled otherwise in the trade because another compound, used in photography and incorrectly dubbed "glycine," is poisonous. The detoxifying effect of glycocoll has been known for some time, and many of the early Ehrlich-type drugs were combined with glycocoll to diminish the toxic effects of the drugs themselves. In the modern era, the coadministration of glycocoll with such medicinals as sulfapyridine has been claimed to lower toxicity effects.

Another amino acid available at a relatively low price is tyrosine. Tyrosine has a chemical structure in common with antioxidants, so that its probable use for this purpose was predictable. Tyrosine, like other amino acids, is not oil-soluble, but on simple conversion to an ester it becomes oil-soluble and retains its antioxidant properties. The objection of toxicity raised to other antioxidants such as hydroquinone is not so valid here. The esters of tyrosine are accordingly receiving some attention as antioxidants in the pharmaceutical and food industries.

Biochemically, tyrosine is probably a precursor of thyroxine, the amino acid which is substantially the active principle of the thyroid. Tyrosine is believed to be converted physiologically into thyroxine in two reaction steps. Simulation of these reactions in glassware has resulted in low yields of thyroxine. Synthetic thyroxine, by a devious procedure, has been available for two decades but not at a price which could compete successfully with thyroid powder. Further improvement of the process, starting with tyrosine, could change this picture.

Tyrosine is also believed to be a natural precursor of epinephrine, an important hormone. In our laboratories we are preparing iodinated tyrosine derivatives which are of interest for X-ray photography.

Another amino acid that promises some special utilities as a detoxifying agent is methionine. Methionine has been successfully used to treat hepatitis of the type caused industrially by TNT or carbon tetrachloride. Reports are also appearing which indicate that methionine may be useful in treating infective hepatitis, or jaundice. New syntheses have made methionine available in quantity.

Cysteine, which is also of biological value as a detoxicant, is an intermediate in the commercial synthesis of biotin. There is evidence for other amino acids functioning biologically as intermediates for vitamins. The conversion of alanine to pyridoxine and the conversion of aspartic acid to a part of pantothenic acid are examples.

To return to specific uses of amino acids, the lack of arginine in the diet of the male rat has been shown to result in lowered production of sperm. This fact suggested arginine therapy for idiopathic hypospermia, and some cases have responded with definite stimulation of spermatogenesis. The entire study was related to knowledge of the long-established fact that spermatozoa are known to contain considerable quantities of arginine.

Histidine has for many years been used in the treatment of gastric and duodenal

ulcers. Protein hydrolyzates have been recommended for this utility also, and oral administration suffices in this latter case.

Histidine is also closely related to histamine, which is produced from the former both biologically and chemically. Histamine is important because it is a causative agent in allergy and shock. Histamine has been used as a desensitizing agent itself. Histamine is also the gastric hormone. The appetizing effect of soy sauce can be attributed to the fact that soy protein has been hydrolyzed: one of the constituent amino acids was histidine, and during the hydrolysis some small part of this is converted to histamine, which in turn induces the characteristic peristaltic hunger response.

It is worth noting that for many of the most common maladies, such as cancer, heart disease, and hypertension, there is no question but that proteinaceous substances and, therefore, amino acids are intimately involved. There are not sufficient data on which to render any prediction as to how amino acids will be utilized in the treatment of such diseases or, indeed, if they will be utilized as pharmaceutical agents at all. We can be sure, however, that the growing knowledge of the reactions of the amino acids in glassware and in the organism will contribute to the solution of these medical problems.

As an example of how such knowledge may add to understanding, one may consider the relationship of amino acids to antibiotics. Bacteria, pathogenic or otherwise, require, as we do, amino acids and proteins for their substance. Some bacteria can synthesize all of their own; others require substantially the same "essential" amino acids as we do. The form required is here also generally the left-handed type.

In the competitive struggle for existence among microorganisms some creatures have developed antibiotic substances that interfere with the growth of others. This in fact defines an antibiotic. One of these substances, gramicidin, produced by *Bacillus brevis*, is a molecular combination of amino acids, as are many other antibiotics. The striking feature of gramicidin, however, is that 45 percent of its amino acids are of the dextro, or "unnatural," form.

It has been found in our laboratories that the growth of many bacteria could be slowed down by the simple dextro amino acids and that products obtained from these structures by chemical conversion also had this effect, in some cases more so than in the parent structure. These simple substances are not so powerful as the antibiotics, but they offer some very suggestive leads. When the secrecy regulations covering penicillin were relaxed in December 1945, it was reported that the pencillin class of antibiotics is also derived from the peculiar right-handed amino acid structure. A fundamental relationship between dextro amino acids, evolution, and therapy thus seems to be involved. There is some evidence that the right-handed amino acids and the antibiotics function by interfering with the activity of enzymes which normally influence the reactions of "natural" molecules.

One further point: alkaloids such as quinine, morphine, pilocarpine, and others which are produced by some plants are believed also to arise in general from amino acids. This suspected relationship has led to some beautiful experiments by Robert Robinson, in England, in which a few alkaloids were synthesized in laboratory glassware, under physiological conditions, from the indicated precursors. It has also led to some attempts, in at least a few laboratories, to lower the cost of preparation of individual alkaloids by synthesizing them from amino acids.

In conclusion, definite lines of genealogy can be drawn from the amino acids to

proteins, vitamins, alkaloids, hormones, antibiotics, and other substances of biological importance. Some of these relationships are so basic that they account for a large amount of research activity in this field. The established value of amino acid therapy in the form of protein hydrolyzates is a matter of clinical record, notably in a series of articles in the *Journal of the American Medical Association*. Some individual amino acids have definite value as pharmaceutical agents and as chemical intermediates; other uses of this sort will without doubt be developed. The place of individual amino acids in the treatment of specific deficiency states is beginning to receive evaluation. There can be no question that the amino acids are of great value in furnishing understanding in various fields of therapy.

BEYOND TIME AND SPACE

A gentleman of England eyed
A falling apple, threaded a maze
Of tangled thought, and thence descried
The invisible sweep of the force that sways
The universe; an Austrian abbot
Tended garden peas and saw
The working of inscrutable law
That molds all life in age-long habit
Of growth; a lover of wisdom in
A Prussian town, strolling each day
Along the same somnific way
Explored the farthest realms within
The reach of reason.

A gentle Jew,
Inscribing symbols, quietly
Encompassed all infinity;
A wrathful one leafed slowly through
A dusty mass of data stored
In rows of books at London's hoard
Of learning, like a cleric drudge—
And found the lever that would budge
The world.

The probers who pursue
The quest of knowledge are the new
Explorers; their minds range past the reach
Of plodding flesh to scale or breach
Or vault the barriers that can
Immure the groping thoughts of man.

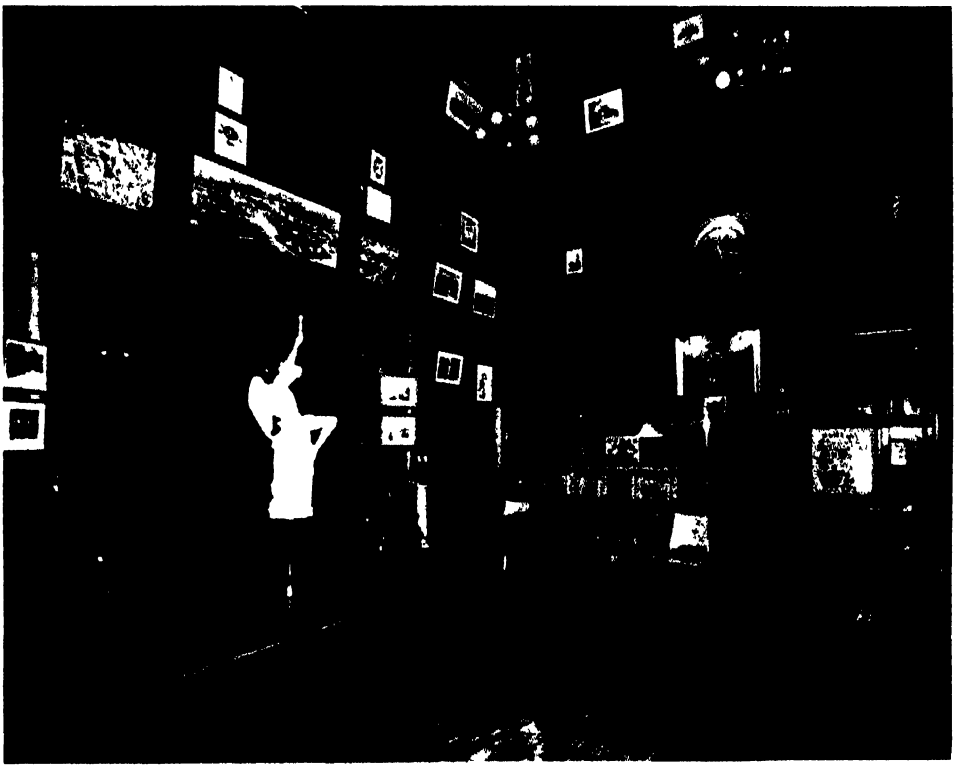
WILLIAM NEWBERRY

THOMAS ALVA EDISON

By CHARLES F KETTERING

AS HAS been its annual custom for the past fifteen years, America will this month again pay tribute to Thomas Alva Edison, the man who, many believe, contributed most to making the world a better place in which to live But this year February 11 has a special significance—it is the hundredth anniversary of Edison's birth

Edison's influence has of course been widely felt Of particular interest to members of the A A A S is the little known fact that in 1880 he made possible the launching of *Science* The full story of this venture into journalism will be told in the February 7 issue of that weekly I, personally, as a member of an industrial research organization, wish to pay tribute to Edison as the pioneer organizer of a research group In 1870, after he had received payment for improving the stock ticker, he set up business in Newark, N J Perhaps it was not so clearly evident to the young Edison as it is to us nowadays just how difficult it is to conduct research, engineering, and manufacturing all under one roof We realize today that these are three distinct stages in converting an idea into a reality, and the time interval separating each of these stages may run into years Edison's keen mind, however, quickly grasped the fact that something was wrong: he could not develop his ideas and manufacture things with the same facilities, so he turned over



PRESENT INTERIOR OF THE EDISON LIBRARY, WEST ORANGE, N. J.

EDISON AT FOURTEEN

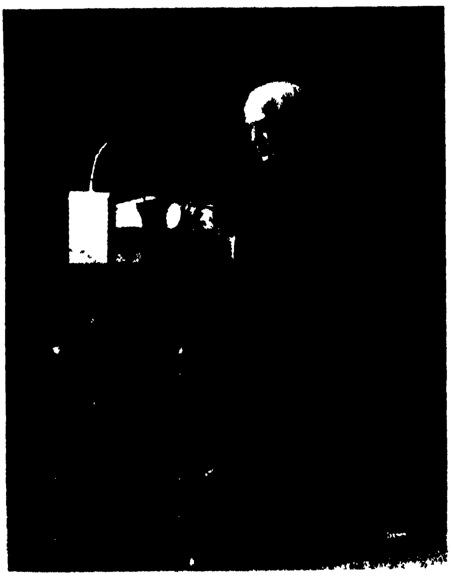
the factory to a competent superintendent and moved to Menlo Park where he could concentrate undisturbed on ideas But he had more new ideas than he had time and hands to work out As a result, he made probably one of his greatest contributions to mankind -he collected around him a group of brilliant men who could help him explore the many channels opened by his fertile mind Today, organized research is the basis of most technical progress, and we owe a great debt to the man who led the way over seventy years ago

These are things that touch upon the scientist's specific interests, but people in general are more interested in the inventions of Edison that have directly affected their daily lives. As our most prolific inventor, he surrounded us with many conveniences that now form an integral part of our lives He gave us light, the phonograph, the motion picture, his carbon button transmitter made the modern telephone possible, and he developed the fluoroscope as an adjunct to radiography.

In the twentieth century we take these things for granted, as we do also the industries that grew from Edison's handiwork—industries that have an estimated value of $20,000,000,000 and some 4,000,000 employees

In Edison's laboratory at West Orange, N J., there stands on a pedestal a cubic foot of copper History tells us that some thirty-four years ago the members of the copper industry gave a luncheon for Edison to express their appreciation of what he had done for their business. They asked the inventor what they might give him in acknowledgment of his great contributions. Edison thought a moment and then replied that he had never seen a cubic foot of copper - maybe they could give him one That is how this unusual exhibit came into his possession

That cube of copper stands today as a symbol of what can happen when an inventor brings from the limitless storehouse of the unknown a new fact On the day in October 1879 when Edison produced the first commercially practical incandescent electric lamp, the copper industry went about its usual daily routine, unaware that



EDISON'S MINE SAFETY LAMP

IT WAS PUT ON THE MARKET IN 1913

its future history was being written in a little laboratory in New Jersey.

On the trail of the new electric light came New York City's Pearl Street Powerhouse, the first central station. Copper went into the dynamo coils and commutators; it went into the switches and the many miles of transmission cable. The production of the metal went from 50,000 tons to the present production of approximately 1,000,000 tons. And the jobs came, too—manufacturing jobs, service jobs, and selling jobs. All because one man found a way of producing light by heating a carbon filament in an evacuated glass bottle.

There are few examples that so clearly show the difference between a *patent* and a *product* as the development of the incandescent light filament. The first lamp on which Edison obtained a patent in 1879 contained a platinum filament, but the inventor was not satisfied. He tried carbonized cotton thread and eventually produced a commercially practical incandescent lamp. He felt, however, that this was not the answer, and further investigation uncovered bamboo as an improvement. He spent thousands of dollars financing a search in the jungles of South America for the best type of bamboo for his purpose. As we all know, the search has never ceased—the adventures with tantalum and osmium and the struggle to make brittle tungsten into filament threads are too well known to repeat here.

With the coming of electric light and power hundreds of other things followed: radios, electric stoves, vacuum cleaners, refrigerators. New industries were born, and new jobs were created by the thousands.

As a creator of jobs Edison has had few, if any, equals. When in 1887 the idea of the motion picture occurred to him, he again set the stage for innumerable new jobs and occupations that no one at the time could even imagine. Who could have predicted that hundreds of thousands of miles of film

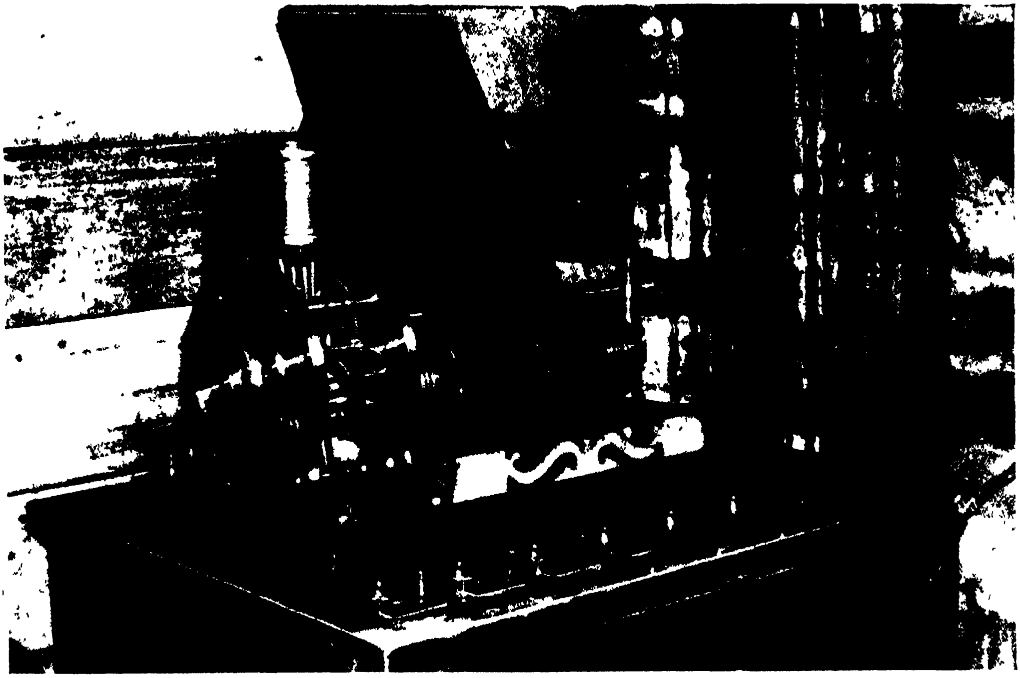
REPLICA OF ORIGINAL LAMP

MADE BY EDISON AT DEARBORN, MICH., AT THE CELEBRATION OF LIGHT'S GOLDEN JUBILEE

would be produced annually and that motion-picture theatres would be built in every town in the United States? The effect of the first flickering shadow on a silver screen has been felt in every home in the nation.

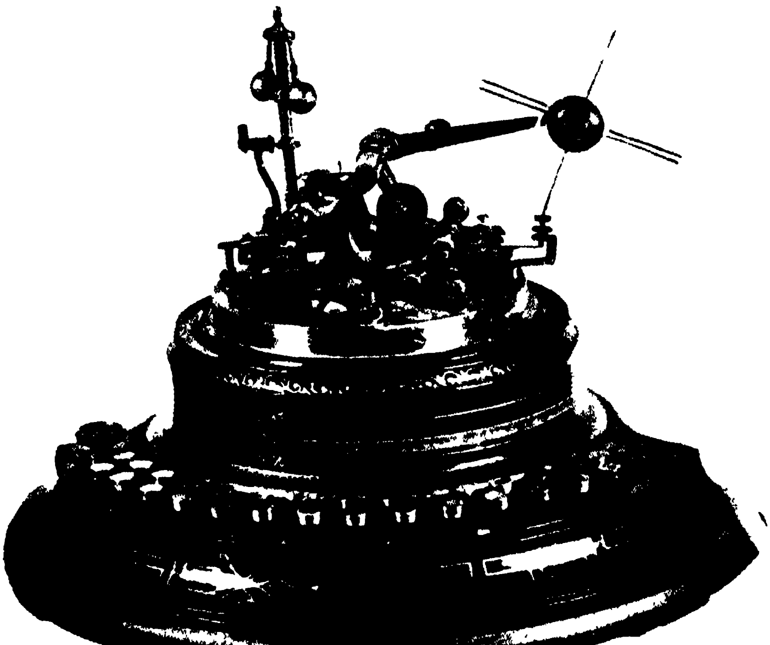
These are the high lights of the results of just *two* scientific studies made by *one* man. Edison was an outstanding example of the combination of the theoretical and the practical in one person. We are not always so fortunate in having such a combination and for this reason often have two or more men working on a project. At the time Edison was patiently working night and day, with comparatively crude laboratory apparatus, to discover basic principles, the public was of course quite unaware that this one man was shaping the America of Tomorrow. This is typical of any scientific development in its first stages: we never know just how important it may become. And so researchers are diligently doing the same type of work in their laboratories today.

Who knows what an idea is worth? In the case of Edison we cannot judge by his



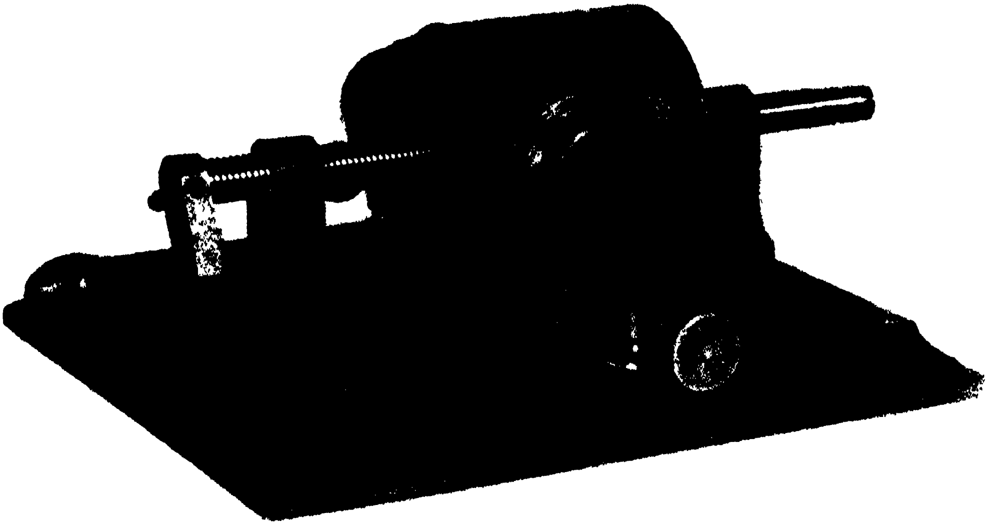
MODEL OF EDISON'S FIRST PATENT

THIS ELECTRICAL VOTE RECORDER WAS PATENTED IN 1868



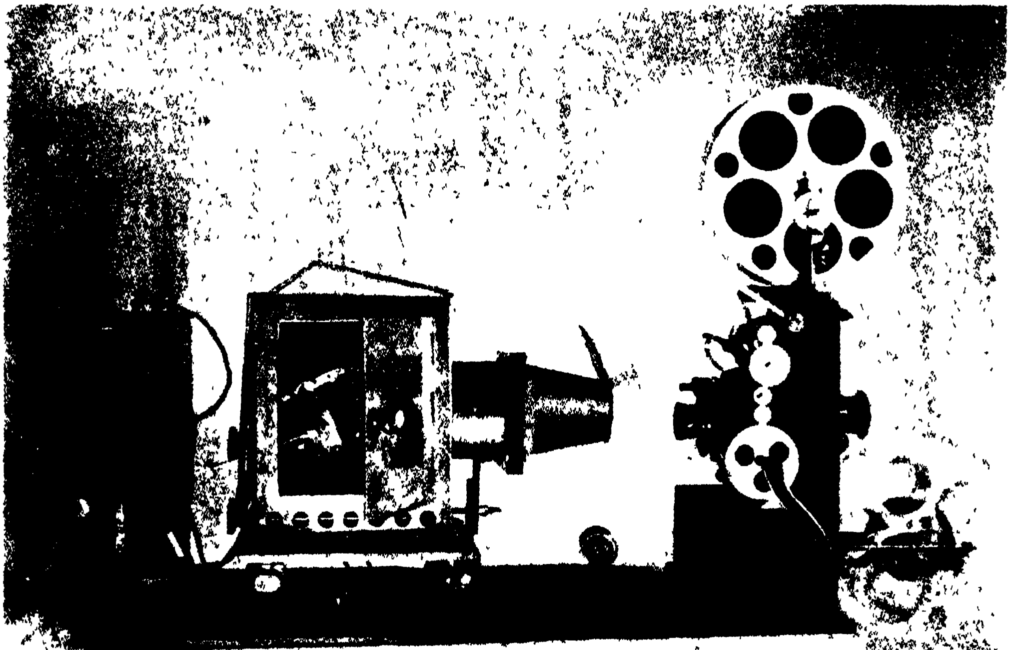
EDISON'S UNIVERSAL STOCK TICKER OR PRINTING TELEGRAPH

MANUFACTURED BY HIM IN NEWARK IN 1872 FOR THE GOLD AND STOCK TELEGRAPH CO



THE ORIGINAL TIN-FOIL PHONOGRAPH

ON AUGUST 15, 1877, THIS MACHINE FIRST RECORDED AND REPRODUCED THE HUMAN VOICE



EDISON'S PROJECTING KINETOSCOPE

ONE OF THE EARLY MODELS OF ABOUT 1902 OR 1903

THE FIRST MOTION-PICTURE STUDIO

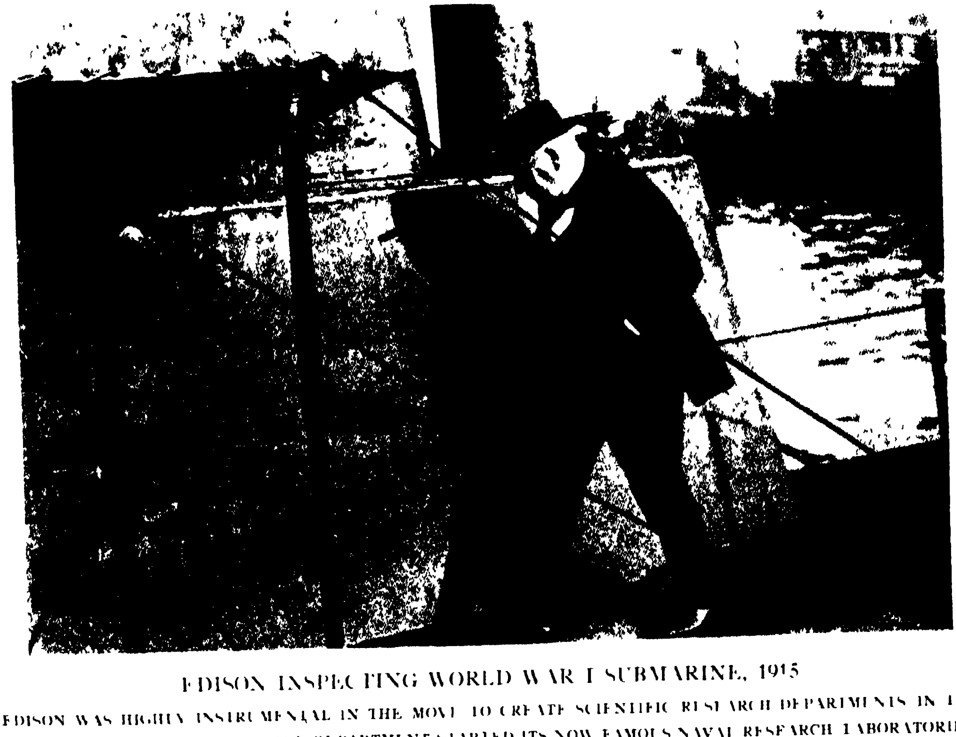
THIS BUILDING, CALLED "THE BLACK MARIA," WAS BUILT IN 1892 AND WAS AMERICA'S FIRST STUDIO FOR TAKING MOTION PICTURES. THE BUILDING COULD BE REVOLVED TO GET DIRECT SUNLIGHT ON THE STAGE

EDISON WITH AN EARLY SOUND-SYNCHRONIZING MOTION-PICTURE CAMERA, 1905

AFTER PERFECTING THE PRINCIPLES OF MOVING PICTURES, EDISON TURNED TO EXPERIMENTS FOR SYNCHRONIZING SOUND TO ACCOMPANY THE FILMS. PHOTOGRAPH TAKEN IN HIS LIBRARY IN WEST ORANGE, N. J

THE INSOMNIA SQUAD, 1910

EDISON FREQUENTLY WORKED FOR LONG STRETCHES OF TIME WITHOUT REST. HERE HE IS SHOWN WITH SIX OF HIS CO-WORKERS AFTER NEARLY 48 HOURS OF CONTINUOUS WORK ON THE "DISC" PHONOGRAPH RECORD



EDISON INSPECTING WORLD WAR I SUBMARINE, 1915

EDISON WAS HIGHLY INSTRUMENTAL IN THE MOVE TO CREATE SCIENTIFIC RESEARCH DEPARTMENTS IN THE ARMED FORCES. THUS, THE NAVY DEPARTMENT STARTED ITS NOW FAMOUS NAVAL RESEARCH LABORATORIES

EDISON'S HOME IN LLEWELLYN PARK, WEST ORANGE, N. J

personal gain because that was infinitesimal when compared with the great increase in our national wealth brought about by the use of his ideas. How can we personally evaluate his contributions? The only way I can possibly do this is to imagine that we were suddenly deprived of all those things for which he was responsible. Let us try to imagine our world without electric power lines—no lights, refrigerators, stoves, radios, or other electrical appliances. Let us think of our towns without motion pictures, telephones, and street cars. What would we not give to have these things returned to us?

In the hundred years since Thomas Alva Edison was born in Milan, Ohio, his inventions have changed modern civilization as have those of no other man. Many people say that he was a genius, but he himself once said, "Genius is 1 percent inspiration and 99 percent perspiration." He clearly recognized the great truth that lies back of every worth-while contribution of mankind—the foundation of civilization's progress—that man will always move forward as long as we have open minds and willing hands.

PROGRESS IN CORK CULTURE IN THE UNITED STATES

By GILES B COOKE

Research Department, Crown Cork & Seal Company, Baltimore

FOR more than 6 years the intensive, expanding program to grow cork trees in this country has made outstanding progress. Established by the late Charles E. McManus, former President and Chairman of the Board of the Crown Cork & Seal Company, the Cork Project is designed to add to the natural resources of our country and to provide in the United States a source for at least a part of the nation's cork requirements. Since its inception the Cork Project has developed from a few experimental plantings in 1940 to large, extensive plantings in 22 states in 1946. Interesting research has accompanied this rapid increase in cork planting, and already much valuable knowledge has been acquired. Thousands of little cork oaks are now growing throughout the warmer half of the United States, and tons of acorns of this much needed tree are planted annually in the potential cork areas of this country. With the splendid cooperation of federal and state foresters and the aid of local vocational agriculture teachers and county farm agents, the Cork Project has passed the trial stage and is now a proved, stable program. Six years of successful background have given confidence and determination to those planting and growing cork trees.

CORK-ACORN DISTRIBUTION

Annual cork-acorn collections have increased rapidly, and the entire domestic crop is needed for planting. Demands for seed greatly exceed the available supply In the fall and winter of 1945-46 more than 5 tons of cork acorns were obtained in California, and this quantity was several tons short of meeting the requests. Cork-acorn distribution during the past 6 years is shown in the following table:

Year	Pounds of Acorns
1940–41	500
1941–42	1,450
1942–43	7,500
1943 44	7,900
1944–45	13,800
1945–46	10,200

All these acorns, except for approximately 200 pounds collected annually in Arizona and the South, were obtained in California.

Courtesy, Crown Cork & Seal Co.

A CORK TREE IN WINTER

THIS EVERGREEN CORK TREE, NOW 3 FEET IN DIAMETER AND 60 FEET HIGH, PROVIDES SHADE AND BEAUTY AT "LAUREL HILL," COLUMBIA, S C

Some of the acorns were planted in California and Arizona, but the bulk of them was distributed in the Eastern, Southern, and Southwestern states. In the far South the acorns are planted early in the year—just as soon as distribution can be made. Approximately one-half of the total col-

lection is placed in wet cold storage and held for several months. These are distributed in the cooler areas each spring as soon as freezing weather is past. Not only has the annual distribution of acorns rapidly increased, as tabulated above, but also skill and technique in planting has improved. Knowledge obtained in the first years of the program has been used to give a higher percentage of germination of acorns and better survival of seedlings.

In addition to the California acorns, some seed has been obtained in Europe and North Africa. Cork acorns have been imported from Spain, French Morocco, and Algeria. A large shipment by boat in 1944 arrived in very poor condition, and subsequent importations have been of smaller quantities. Some acorns were brought from North Africa by airplane.

METHODS OF PLANTING

Three methods have been used for planting cork trees, and each has definite merits. Propagation of the cork oak may be effected by planting cork acorns in permanent locations, planting acorns in individual containers and later transplanting to permanent sites without exposing or damaging the roots, and planting acorns in a nursery, removing seedlings in bare-root condition the following year and transplanting in permanent sites. Up to the present time the cork tree has been planted only on areas of limited size.

The easiest and simplest way to grow

Courtesy, Crown Cork & Seal Co

A TRUCKLOAD OF ACORNS OF THE CORK OAK

HUNDREDS OF THOUSANDS OF CORK ACORNS LIKE THESE ARE COLLECTED AND DISTRIBUTED ANNUALLY THROUGHOUT THE WARMER HALF OF THE UNITED STATES

Courtesy, Crown Cork & Seal Co

CORK ACORNS ARE DISTRIBUTED TO CHILDREN

MEMBERS OF THE 4-H CLUB IN SCOTLAND COUNTY, N C, ASSEMBLE TO RECEIVE THEIR CORK ACORNS FROM F O McMAHAN, COUNTY AGENT.

cork trees is to plant acorns where the trees are to grow. More than one-half of the cork acorns collected are planted in this manner. Three acorns may be placed in each site; when planted in this way a good cork tree at every spot is obtained Protection against squirrels, other rodents, and livestock must be provided. This method avoids future transplanting, which is often accompanied by root damage and plant shock.

A limited number of the acorns are planted in cans or paper pots. When the seedlings are 5 to 8 inches high they are placed in permanent locations Survival of seedlings planted by this method is very high—over 95 percent. The small loss is due largely to damage incurred in transit This method is not practical when thousands of plants are distributed, but it is excellent for limited quantities

Bare-root seedlings have been transplanted successfully with good survival, but unless all conditions are just right the loss is often high. Very often, either during shipment or after being received, the roots become dry, which is fatal to the plant Also, in many cases the seedlings receive root damage, with resulting plant shock, and growth is retarded. When more is known about this method of planting cork trees it may be used more extensively.

CORK PLANTINGS

Many plantings of the cork oak for ornamental purposes have been made.

Courtesy, Crown Cork & Seal Co

CORK PROPAGATION

THIS ROOTED CORK CUTTING, TAKEN FROM A MATURE CORK TREE, IS THE RESULT OF PATIENT, CAREFUL RESEARCH.

The heavy, evergreen foliage and the light-gray bark give the cork tree a pleasing, attractive appearance throughout the year. Governors in 9 states, recognizing the beauty as well as value of this tree, have planted and dedicated a cork tree on the grounds of their respective state capitols. One-acre or larger groves of cork oaks have been planted by a number of colleges and universities. After a few years these groves will be valuable, beautiful additions, serving for study and research as well as being recreational areas. Other plantings have been made in parks, about schools and other public buildings, and along highways. Many private plantings about homes and on farms have been made to secure attractive shade trees.

Cork trees are planted every year in those states having a climate favorable for the growth of this variety of white oak. Two general methods are used, the choice of procedure being made by the cooperating state.

A substantial supply of acorns is sent to the state forestry departments. The greater portion is distributed directly to interested planters, and the remainder is planted in the state forest nursery. Seedlings grown from the acorns are usually distributed when one year old. The procedure varies slightly in the different states. In some states all the acorns are distributed as soon as received; in others some acorns are distributed and the rest used for growing seedlings. Both methods have merit, but up to the present time acorns have given better results with the average planter than seedlings.

In the past two years large quantities of cork acorns have been distributed to high-school boys and girls. Cork plantings by these interested young people are arranged through the Department of Forestry, the Department of Education, or the Extension Service in the various states. Cork acorns are sent at the proper planting time to the Vocational Agriculture Teachers,

Courtesy, Crown Cork & Seal Co

FROM AN ACORN

PLANTED IN THE WINTER OF 1942-43, THIS YOUNG CORK TREE AT COLLEGE STATION, TEX., HAS GROWN TO A HEIGHT OF MORE THAN 8 FEET.

for Future Farmers of America, or to the County Agents, for the 4-H Club members. The enthusiasm and determination these boys and girls are showing in growing cork trees is resulting in thousands of young cork trees for this country every year

PLANTING RESULTS

Because of the tremendous number of acorns and their extensive distribution, a check on the plantings is as yet incomplete. However, the known results are generally good. Acorn germination ranges from 50 to 80 percent, which is high considering the period in cold storage and, in some cases, the long distances traveled in shipment. Cork seedlings show a very high survival rate (95 percent) when shipped with earth in paper pots and planted without exposing the roots Bare-root seedling loss is much higher, owing chiefly to serious root injury in lifting or to the roots becoming dry while out of the ground.

In all the states outstanding growth has been made by certain trees, demonstrating clearly what the cork oak will do under favorable conditions. The fastest growth has been made where the growing season is long and rainfall generous.

STRIPPING MATURE TREES

More than 500 cork trees have been stripped of virgin cork since the initiation of the McManus Cork Project. This cork has been manufactured into various products and given thorough, exhaustive tests. Most of these trees are in California, but cork has been removed from approximately 20 trees in Arizona and the Southern states. Composition cork and corkboard insulation made from this cork were of excellent quality, and the domestic material was found equal in every way to imported cork of the corresponding grade.

Growth of reproduction cork on the stripped trees is very satisfactory. In 1944, on 35-year-old trees stripped in 1940, new cork formation was three-quarters of an inch thick. In 1946, 6 years of growth yielded cork almost one inch thick from these trees, and 1.5 inches of cork had grown on older trees. The second-growth cork was, like the virgin bark, found to be of excellent quality. Annual domestic cork stripping now involves the removal of reproduction as well as virgin cork, and before long third-growth cork will be taken from the California trees each year along with first and second strippings.

The following table gives a summary of cork strippings by years:

Year	No. of Trees Stripped	Yield of Cork (lbs.)
1940	248	10,561
1941	47	2,142
1942	63	3,466
1943	46	2,735
1944	54	3,216
1945	58	3,538
Total	516	25,658

RESEARCH

Along with the extensive planting of cork acorns and seedlings much valuable research on cork culture is conducted by the Cork Project.

Methods of storing cork acorns received early attention. The acorns are very perishable and must be given special care if they are to remain viable for 6 weeks or longer. Cold-storage tests showed the acorns can be kept several months at 34° to 40° F. if not allowed to become dry. This has been of great value in distributing the perishable seed at the proper planting season to the cooperating states During the past several years tons of cork acorns have been kept viable for 2 to 4 months in wet cold storage.

Cork acorns are collected with much care. To avoid leaves, grass, and other extraneous material the acorns are collected by picking. This is very important in storage as the presence of dead organic matter often re-

Courtesy, Crown Cork & Seal Co

STRIPPING CORK

SECOND-GROWTH CORK IS HARVESTED FROM A TREE AT CHICO, CALIF., JULY 1946. THE VIRGIN CORK WAS TAKEN FROM THIS TREE IN 1940.

sults in mold development when the acorns are removed from wet cold storage.

The cork oak has been successfully grafted to native oaks. Successful grafts have been made at the Fruitland Nurseries, Augusta, Ga., and the Masonic Homes, Elizabethtown, Pa. Mirov and Cumming have shown that scions of the cork oak can be grafted to both evergreen and deciduous American oaks. This method of establishing cork-oak groves is under observation, it will be some time before the complete story is known.

The rooting of cork cuttings is another method of propagation that has received serious attention. This method was first successful at the Fruitland Nurseries in 1943, and since then a limited number of rooted cuttings have been obtained each year. Tests are being continued until the method can be reduced to a routine. Cuttings, like scions for grafting, are taken from mature trees having thick, resilient cork and bearing a large crop of acorns.

Some of the cork plantings made in 1942 and 1943 have shown remarkable growth. One tree at Hastings, Fla., which was an 8-inch seedling in June 1942, measured 3.5 inches in diameter and 12 feet in height in June 1946. Evidently this tree has had very favorable growing conditions. In order to determine what plant food elements are needed by the cork tree a special experiment is being conducted. Cork seedlings in sand are being grown in the presence and in the absence of plant food elements. This research should show what elements a cork tree needs for good growth.

Bare-root cork seedlings are difficult to transplant. The cork oak has a long taproot, frequently with few lateral roots. Special research on the root pruning of cork seedlings in the nursery has been in progress several years. When these tests are completed survival of transplanted cork seedlings may be much higher.

CONTRIBUTIONS OF ENTOMOLOGY TO THEORETICAL BIOLOGY

By CHARLES T. BRUES
Biological Laboratories, Harvard University

DURING the past century, which has seen such a rapid development and changing outlook in the several biological sciences, the insects have served a major purpose in furthering theoretical deliberations as well as factual knowledge. This is a natural consequence of the abundance and diversity of these small animals, for they are the most numerous of all specialized organisms. With this in view, following a personal acquaintance with the insects extending over nearly half of this period, I have been tempted to recapitulate briefly and in a very general way their contributions to the biological mill, laying stress on matters where they have functioned particularly well as objects for close observation or experimental laboratory investigation

In dealing with theoretical biology, it must be admitted that one inevitably encounters wide divergence of opinion as to what may be hypothetical, theoretical, or generally accepted as established and fundamentally sound. Several concepts have passed through these successive stages only to retrograde and lapse into oblivion. Others which have withstood continued scrutiny remain theoretical only in the sense that their early history betrays the mark of prophetic expectancy, dampened by the inevitable skepticism that greets any innovation.

Many readers will at once assume that the greatest of all contributions by insects are those that have led to what is familiarly known among biologists as "Drosophila genetics," based at first on a single abundant species of small fruit fly and later extended to other members of the same genus. Such a supposition would be much nearer the truth were we concerned only with the most recent developments in biology.

A mutation of this little pomace fly (Drosophila), discovered by chance, formed the beginning of a most remarkable and rapidly unfolding panorama of biological discoveries, one which has gone far over a period of several decades toward furnishing an insight to the complicated mechanism of inheritance. It happens that the small flies multiply rapidly and abundantly when confined in bottles in the laboratory, where many successive generations and large populations may be studied with a minimum of time and effort. Furthermore, most probably because they belong to a recent group of insects at present in the throes of rapid evolutionary change, they appear particularly prone to produce mutations, anomalies, atavistic regressions, and imperfections of true hereditary nature. To all this is added the fact that there are only four paired chromosomes, within which all the hereditary characteristics are crowded, instead of the greater number present in most other animals and plants. Finally, the actual morphology of these chromosomes may be examined, owing to the very unusual circumstance that in one single organ (the salivary gland) of this particular order of insects (Diptera), and here alone, the chromosomes become so hypertrophied that their minute structure is clearly revealed by the microscope to a degree not paralleled elsewhere among living organisms. It would seem that Divine Providence had set the stage and pointed out the fruit fly as a chosen object for biological inquiry Nevertheless, it just happened this way, and an obnoxiously prolific insect has come to occupy the center

of the biological stage. A few competitors have been unearthed, but they are clearly second-rate, and none other has so beautifully fulfilled the requirements for a perfect experimental animal. The field of Drosophila genetics has fast expanded but appears to have by no means reached its final stage of cultivation. So far its fruits have added little directly to an understanding of evolution or speciation, but the application of taxonomic and mathematical methodology to studies of the numerous species of Drosophila is now breaching the wall that appears to cloister some hitherto wholly undisclosed workings of the evolutionary process. These revelations, particularly with reference to the fixation of mutational changes in population groups of dissimilar size, are already being applied to some evolutionary problems of great concern to paleontology. If nothing else, they have served to bring genetics and paleontology into closer accord, a most desirable accomplishment which bids fair to speed progress in both these branches of biology

Drosophila has thus given to the world a second contribution, this time to pure science. It was dependent upon, and followed closely on the heels of, Mendel's discovery of the independence of unit characters in inheritance.

The first contribution of Drosophila lies on a far lower intellectual level and should perhaps not be mentioned here since it came to earth, not as a child of theory, but as a practical gift to several of our earlier civilizations It relates to the now well-known fact that the common Drosophila flies disseminate the yeasts on which their larvae feed in decaying fruits and that they quite universally infect grapes with these organisms, thus preparing them for the fermentative process which results in the production of wine. This valuable service was totally unappreciated by the ancient wine makers, and its true significance was recognized only a few years ago when biologists began to probe into the dietary regime of the fruit fly By laboratory experiments they determined that yeasts form a major component in its meal of rotting fruit and that the aroma of the alcohol thus arising serves to whet the fly's appetite quite as well as our own. Incidentally, it must be noted that the studies of yeasts as food for Drosophila occupied an essential place in the early development of our knowledge concerning the nature and significance of accessory food substances. These in turn have played a major role in advancing our knowledge of the physiology of nutrition, not to mention the compounding of "vitamins and minerals" into pills with which the American public now doctors itself to the tune of some millions of dollars annually. Added to its large interests in the liquor industry, this is quite a financial coup for a little fly risen to fame from a cradle of rotten fruit

One of the original bulwarks of the principle of natural selection was the idea of protective resemblance whereby animals escape notice by predatory enemies as the result of a modified exterior which simulates some common object in the environment. In the case of insects this may frequently be a leaf, twig, a bit of bark, or even a bird dropping. Among all animals, the most outstanding examples of this phenomenon are to be found in insects like the phasmid leaf-insects and walking sticks of many genera. No less striking are certain butterflies, culminating in the genus *Kallima*, where the wings simulate dry leaves when folded over the back in their resting position. Commonest among such camouflaged insects are numerous green caterpillars whose bodies fuse visually into the play of light and shade on the vegetation they frequent. This multiplicity of camouflage presented everywhere by insects is by far the strongest evidence for the development of such characters through natural selection and was so recog-

nized early by Darwin and Wallace, both of whom had enjoyed the opportunity to observe many examples in nature. This was particularly true of the latter during his wide experience in the Malayan Islands, notable for the tropical exuberance of their insect fauna. It may be fairly said that through the years the insects have furnished the bulk of material which the work of numerous observers has welded into an elaborate framework demonstrating that protective coloration and protective resemblance represent very decisive phases of organic evolution, readily visualized as effected through the action of natural selection.

Closely associated with the idea of protective resemblance and usually mentioned in the same breath is that of warning coloration and mimicry, indeed, we may almost regard the latter as an offspring of the former. If so, we must unfortunately damn it as an illegitimate child, for it has been by no means uniformly received with good grace in polite biological circles Most of the outstanding examples are met with among the insects, particularly butterflies, moths, flies, and certain beetles, and they involve both form and color, often to a degree so convincing that all doubt vanishes and no questions are asked. In the case of butterflies the difficulty arises at the assumption that the model is unpalatable and that it successfully advises its predatory enemies of this fact by a warning pattern in brilliant color, gradually acquired through natural selection. This theory assumes a good memory in insectivorous animals, such as birds, lizards, and other insects, a careful choice of tasty morsels on their part, and perhaps even ancestral memory to speed up the process beyond a negligible rate. The existence of such peculiarities has never been proved. To further complicate the matter we know that certain of these butterflies are polymorphic forms of a single species mimicking different species of

Original

PHYLLIUM SICCIFOLIUM

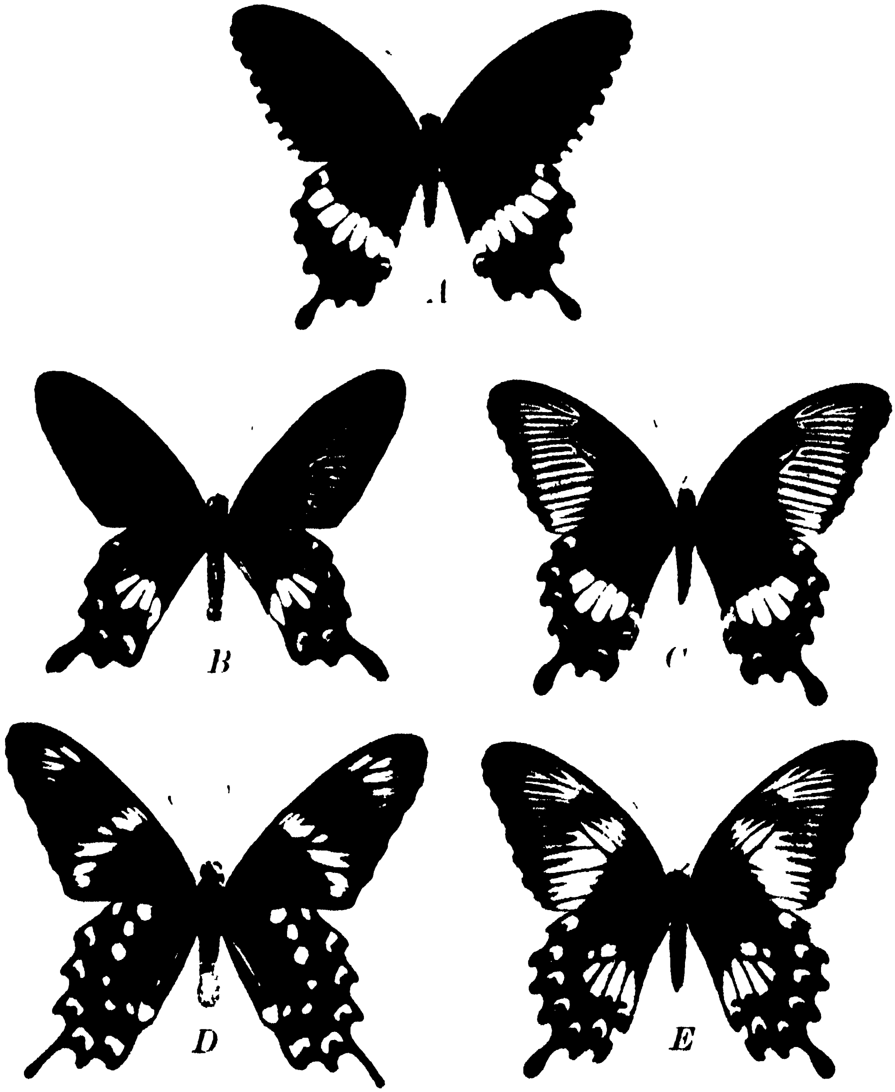
A LEAF-INSECT RELATED TO THE WALKING-STICK

other butterflies. Moreover, the polymorphic color forms are Mendelian phenotypes, differing from one another by unit characters.

In the case of moths, flies, and beetles that resemble stinging wasps, mimicry seems far more plausible, especially to those of us with tender skins, neurotic dispositions, or allergic tendencies Again, such has not been proved for insectivorous animals and many of them, including such diverse forms as bears, skunks, lizards, and robber flies are not deterred by such protective devices.

The idea of orthogenesis, determinate evolution, or predestination as propounded by Eimer is essentially a matter which turns to paleontology for evidence of its validity. The assumption that evolution proceeds by steps that are determined by intrinsic factors without the guidance of natural selection, at least during the incipient stages, is more generally acceptable to paleontologists than to experimental biologists. The former have "seen it happen" in structures as diverse as the teeth

of mammals and the shells of ammonite mollusks, long cited as classical examples. Consistent changes follow the beam and may proceed with undiminished vigor toward a point where they appear, to all intents and purposes, clearly detrimental to their possessors. So far, it seems that the reality of orthogenetic evolution cannot be denied but that its existence is incompatible with any assumption that the individual



Original

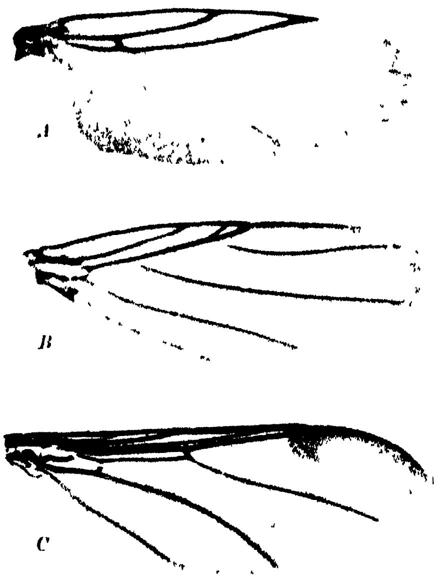
MIMICRY IN SWALLOW-TAIL BUTTERFLIES, *PAPILIO*

FEMALES OF THE MIMETIC *Papilio polytes* OCCUR IN THREE VERY DISTINCT FORMS (SHOWN AT *A*, *C*, AND *E*) THE VARIETY *cyrus* (*A*) CLOSELY RESEMBLES THE MALE. THE VARIETY *polytes* (*C*) APPEARS TO MIMIC A POISONOUS SPECIES, *P. aristolochiae*, SHOWN AT *B* THE VARIETY *romulus* (*E*) APPEARS TO MIMIC *P. hector*, ANOTHER POISONOUS SPECIES, SHOWN AT *D*. THE THREE POLYMORPHIC FEMALE FORMS OF *Papilio polytes* ARE KNOWN TO BE INTERBREEDING MENDELIAN FORMS.

genes reproduce themselves in perpetuity without variation or change other than that due to chance mutations of random nature. However, if gene mutations are either directly or indirectly dependent upon molecular constitution, we must reasonably expect some definite pattern of change to be evident. As we cannot hope to see manifest changes occurring within the period of a few human generations, direct experimental evidence is obviously not to be anticipated, even though we may cheerfully pass the buck to the genes. Painful aspersions have been cast on confessions of ignorance concerning the nature of intrinsic factors responsible for orthogenetic evolution, and it has been far too generally stated that biologists are thus led to embrace the tenets of metaphysical or theological vitalism, both of which are first-class anathemata to modern science. Nevertheless, there is still a good deal of meat in the belief that honest confession is good for the soul, even though the entity named may be nothing more than an impelling figure of speech.

Patently, insects can have played small part in formulating or furthering any orthogenetic doctrine, for a knowledge of paleoentomology is as yet far behind that of many other groups of animals, despite very substantial progress during recent years. Nevertheless, the insects, in connection with the color patterns of butterflies, furnished some of the critical material on which Eimer's original ideas were based.

Other insects of the order Diptera have also offered an interesting side light in connection with the degeneration of the wings, organs which form a complex, morphologically integrated unit. In several families of these flies it is noticed that an extreme simplification of the wing venation occurs whereby some members of certain families (Scatopsidae, Ceratopogonidae, Phoridae, and Hippoboscidae) develop very similar configurations. Although we know that all these have not been developed from a single



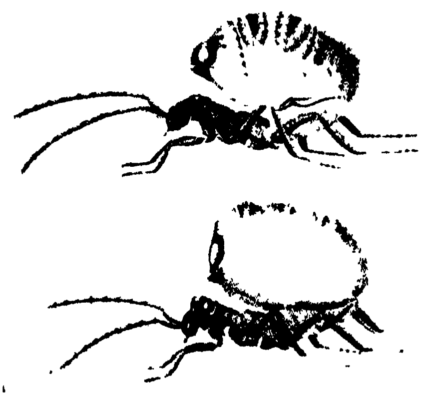
(after *Bequaert*)

WINGS OF FLIES

WINGS OF SEVERAL UNRELATED FAMILIES OF DIPTERA, ILLUSTRATING STRIKING CONVERGENCE IN THE VENATION, COINCIDENT WITH A REDUCTION OF THE WING VEINS. *A*, *Scatopse* (SCATOPSIDAE), *B*, *Phalacrotophora* (PHORIDAE), *C*, *Lynchia* (HIPPOBOSCIDAE)

ancestral type, the loss of veins has proceeded to an almost identical pattern. Here there is no recrudescence of atavistic morphology, as the primitive venational type is the most complex to be found in this order of insects. This convergence in simplification is clearly in the nature of an orthogenetic process, resulting perhaps as a reduction through the casting out of specific genes during evolution. This is exactly what most commonly happens, for example, in apparently random mutations as observed in the most studied insect, Drosophila, also a member of the same order Diptera. It is, however, not haphazard but an orderly process and remains at present inscrutable, nonetheless so when pushed back into the realm of ultramicroscopy.

A similar convergence in degenerative pattern is seen in certain small parasitic



TWO STRANGE BEETLES

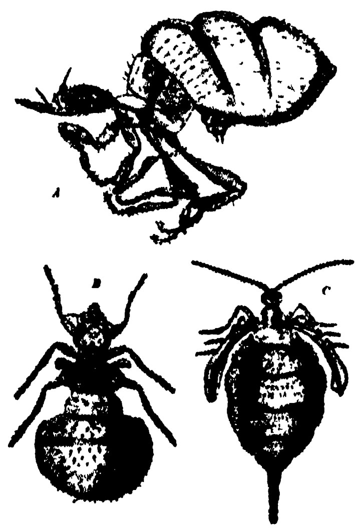
THESE STAPHYLINIDS REPRESENT EXTREME CASES OF PHYSOGASTRY FROM THE ORIGINAL FIGURES OF SCHIODTE, WHO FIRST MADE KNOWN THIS REMARKABLE TYPE OF BEETLE

Hymenoptera belonging to several superfamilies, where the wing venation is reduced to an isomorphic configuration evolved independently from the more complex one characteristic of the generalized members of each group.

A peculiarity known as physogastry is developed by various insects, especially staphylinid beetles and flies of at least two families that live as termitophiles, exclusively in the nests of various tropical termites. The abdomen of these physogastric forms is enormously distended till they become grotesquely deformed, presumably in response to the excess of highly nutritious food with which they are stuffed by their overzealous termite hosts. To some extent this satietal morphology represents a postmetamorphic stretching of the intersegmental membranes like that of the still more severely affected termite queen and as such has no more genetical significance than the goose that supplies *pâté de foie gras*. Here, however, it represents a purely developmental or genotypical conformation. Moreover, it has been shown that two independent lines of physogastric beetles have arisen, one in the Old World and another in the New World, each leading through a separate series of genera to a remarkable similarity in physogastric modifications. This convergence obviously cannot have arisen through a loss of genes, and, further, it must be admitted that the whole process is permeated by a strongly Lamarckian flavor. As there may be hormonal peculiarities involved, these termitophiles present an interesting problem for more precise experimental investigation.

Ever since the doctrine of organic evolution and the unity of life infiltrated biological thought, it has been noted that the rate, or tempo, of the evolutionary process is by no means uniform. This is true when we consider the large groups or phylogenetic units among animals, and similar discrepancies are commonly encountered among the members of much more restricted categories.

The insects present many examples of



After Wasmann and Silvestri

TERMITE-LOVING FLIES

THREE SMALL TERMITOPHILOUS DIPTERA SHOWING SIMILAR PHYSOGASTRIC MODIFICATION IN UNRELATED FAMILIES. *A* AND *B* ARE OLD-WORLD FAMILY TERMITOXENIDAE. *C* IS TERMITOMASTUS, REPRESENTING A TOTALLY UNRELATED NEOTROPICAL[1] FAMILY.

this kind. Thus, the cockroaches, constituting the order Blattodea, have persisted since the Upper Carboniferous and during this long period have undergone only minor changes in comparison with other living orders of insects, most of which did not come into existence till much later. We may attribute such persistence to type over the ages to greater fitness and hence comparative freedom from the action of natural selection. Such indeed was the earlier and readily plausible interpretation. However, it is a far stretch of the imagination to regard these generalized creatures as even appreciably better adapted to their environment than the highly specialized types of insects of much more recent origin. That they are highly versatile must be admitted, but this adaptiveness appears restricted to their behavior, a matter to which we shall return in a moment. Not only these insects, but other arthropods, illustrate this phenomenon beautifully, stripped

Original

A FOSSIL COCKROACH

THIS SPECIMEN FROM THE UPPER CARBONIFEROUS OF MAZON CREEK REPRESENTS A PRIMITIVE ORDER OF INSECTS THAT HAVE UNDERGONE LITTLE EVOLUTIONARY CHANGE OVER A VERY LONG PERIOD OF TIME.

Original

A FOSSIL ANT

THIS SPECIES, *Lasius schiefferdeckeri*, IS ABUNDANTLY PRESERVED IN BALTIC AMBER. LIKE THE FORMICA MENTIONED IN THE TEXT, THIS SPECIES IS NEARLY IDENTICAL WITH THE VERY COMMON LIVING *Lasius niger* OF EUROPE AND NORTH AMERICA. THE PARASITIC MITE WHICH IS TO BE SEEN ATTACHED TO ITS LEG IS MUTE EVIDENCE THAT THE TERTIARY ANTS HAD ALREADY FALLEN PREY TO SUCH PARASITES.

quite completely of any behavioristic connotations. We refer to the scorpions and still abundant king crabs.

The insects have furnished many more specific examples of this variation in the tempo of evolution, which has become a topic of increasing interest to a considerable number of biologists concerned with the problems of speciation and has, in a highly speculative form, already entered the field of Drosophila genetics. So far as our present knowledge goes, the ants appear full-fledged at the dawn of the Tertiary, and there is good reason to believe that they had not been long in existence at that time. Yet, in the Oligocene the most abundant ant (exquisitely preserved in Baltic amber) is really not specifically distinct from a dominant species of Formica that now ranges widely over both Europe and North America. Together with it are extinct genera and species having no close counterparts in the world today, as well as members of other genera where speciation has proceeded at a more moderate though active

Original

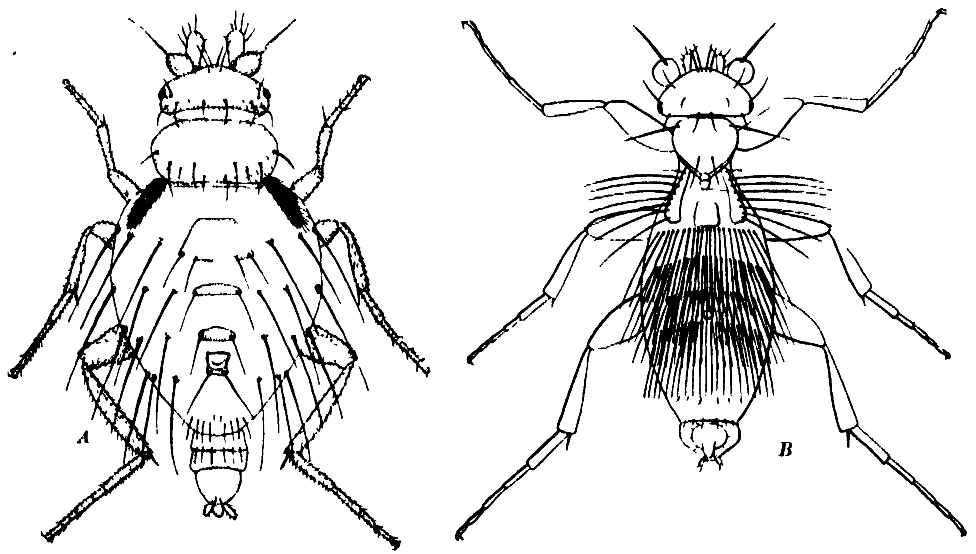
A FOSSIL PARASITE

Pelecinopteron tubuliforme, AN OLIGOCENE PARASITIC HYMENOPTERON IN BALTIC AMBER. A PECULIAR TYPE WHICH IS THE SOLE REPRESENTATIVE OF AN EXTINCT FAMILY

tempo. Similar discrepancies are encountered among other amber insects, and since this extinct fauna is incomparably better preserved than any other of equal age, it presents the clearest picture yet available for a study of this kind.

This particular case of the Oligocene Formica so fully authenticated, falls directly in line with the statistical expectation of slow evolutionary tempo in large and flourishing populations as contrasted to a rapid change predictable for small populations. It would appear that among these ants the complex social organization of the more specialized subfamilies, including the exploitation of Mother Nature's food supplies, has met with the most surprising success. Here in the background appears again the specter of behavioristic adaptation, in this case strikingly like that evinced by the human species, whose dominant position with respect to food has been only mildly shaken by the recent manipulation of food supplies and the extension of our dietary deep into the realm of the ersatz. In spite of this similarity, the human line of ancestry suffered rapid evolution even before the advent of technology. Concerning the behavioristic attributes of the early anthropoids we remain ignorant, but they were obviously not of the fixed nature everywhere evident among the insects. This difference is clearly a factor of paramount importance in regulating the tempo of evolution.

Cases of greatly exaggerated "speed-up" are seen in many myrmecophilous and termitophilous insects whose habitat is re-



After Brues

TWO SMALL, ALMOST WINGLESS PHORID FLIES

THE FEMALES OF THESE AND NUMEROUS RELATED FORMS HAVE DEVELOPED MOST REMARKABLE BRISTLING ON THE BODY AND WINGS. *A*, *Ecitomyia spinosa*; *B*, *Xanionotum hystrix*

stricted to the nests of ants and termites. Notable among these are certain minute flies of the family Phoridae that have partially or completely lost the wings in the female sex but have acquired in recompense most elaborate bristly ornamentations that place them at once among the most bizarre of all insects. Other members of this family, from what we know of them in the Oligocene fossil fauna, are, in contrast, reasonably stable. The same is true of other related living forms that occur in ant or termite nests but exhibit no unusual morphological changes resulting therefrom. Similar exuberance is notable in many treehoppers of the family Membracidae, where remarkable projections, excrescences, and other bric-a-brackery, developed on the prothorax, appear to be utterly useless and to have been called forth by nothing unusual in the environment. These treehoppers are perhaps the most conspicuous examples of this phenomenon, well known elsewhere among animals although commonly associated with sex, as in the plumage of male birds or the contraptions designed partly therefrom by milliners as feminine adornment for the human species. Insects are only one of the groups of animals, or plants, for that matter, that have exhibited such varying rates of evolutionary change during their known history on earth.

Original

A CURIOSITY

A BRAZILIAN TREE-HOPPER, *Stylocentrus concolor* WITH LONG, SPINY PROJECTION ON THE PRONOTUM

Original

A HAWAIIAN BEETLE

Proterhinus validus, ONE OF A LARGE NUMBER OF SPECIES FROM HAWAII, WHERE THIS GENUS HAS UNDERGONE EXCESSIVE SPECIATION

They furnish some of the most outstanding examples yet brought to light, although so far our knowledge is painfully meager when compared with the finely delineated picture which paleontologists are able to draw of mammalian evolution, for example, among the Equidae.

Perhaps the most striking of all cases among insects, involving rapid evolution and very extensive speciation, are several genera of Hawaiian insects. One of these, *Sierola*, is a genus of Bethylid Hymenoptera including more than one hundred and seventy known species in these islands and a few others extending westward to Australia and China. Another, *Proterhinus*, of even greater extent, is the only known genus of a peculiar group, usually segregated as a monotypical family of beetles. The third is *Hyposmocoma*, a genus of moths with well over two hundred species in these islands. All are practically restricted to the Hawaiian Islands where they

Original

LUBBER GRASSHOPPER

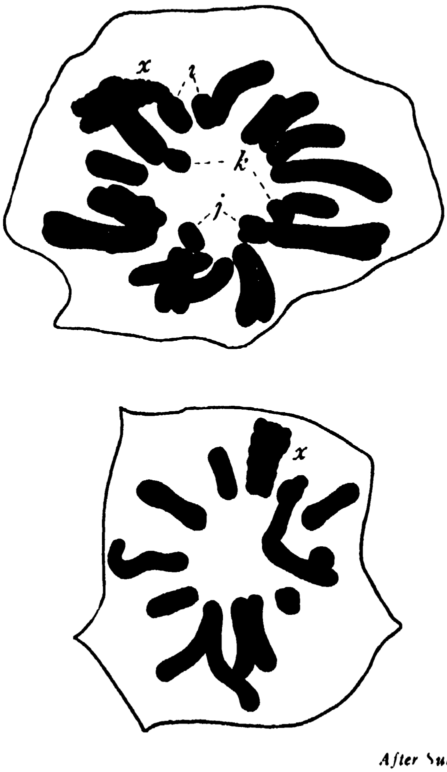
Brachystola magna, A LARGE NORTH AMERICAN GRASSHOPPER ON WHICH EARLY INVESTIGATIONS WERE MADE CONCERNING THE INDIVIDUALITY OF THE CHROMOSOMES

have simply run wild on a prolonged, although geologically very brief, period of active and fertile speciation.

A necessary step in developing the unfolding panorama of genetics during the first decade of the present century was the demonstration of the individuality of the chromosomes. Coming just at the time of the belated rediscovery of Mendel's experimental work on plant hybrids, it made possible a much more ready and detailed study and understanding of the part played by the chromosomes as bearers of hereditary characters.

Insects formed the material for these cytological studies—in this case certain grasshoppers, chosen as particularly suitable for the purpose. The chromosomes of these insects are few in number, rather large, and each is of very characteristic size and configuration, which renders its recognition under the microscope a comparatively easy matter. So distinctive is the assortment of chromosomes in some species of these Orthoptera, that one ardent cytologist, not too well acquainted with the complexity of insect taxonomy, once advocated their use as the basis for a more rational classification of these and other insects. Obviously, if we might peer with sufficient ease and acuity into the ultramicroscopic constitution of the chromosomes, such a plan might conceivably be pushed to a satisfying conclusion. Recent studies of the salivary chromosomes of Drosophila and other Diptera certainly do show that this idea is not so fantastic as it appeared at the time, and, if such structural considerations ever invade the taxonomic field, it will be through the medium of the insects.

The biogenetic law regards the development of the individual animal as an immeasurably accelerated repetition of its evolutionary history, or, to use a catch phrase, it states that the ontogeny of the individual repeats the phylogeny of the race. This is primarily an embryological concept used with telling effect, especially by Haeckel and some of his contemporaries, to gain support for the principle of organic evolution



After Sutton

INSECT CHROMOSOMES

TWO OF THE ORIGINAL DIAGRAMS PUBLISHED IN 1902 BY SUTTON, SHOWING THE INDIVIDUALITY OF THE CHROMOSOMES IN THE MALE GERM CELLS OF THE GRASSHOPPER *Brachystola magna*

On account of the greatly modified manner in which their early development takes place, insects present little that falls in line with the expectations of the biogenetic law. Indeed, had they been chosen as materials, no such idea would ever have seen the light. In their early embryonic development most insects depart widely from the all-pervading pattern so generally characteristic of the majority of other animals Still more conspicuous are the peculiarities of postembryonic growth which have resulted from the highly specialized type of metamorphosis they have developed, quite apart from any phylogenetic memories still lingering in the remains of their aboriginal germ plasm. Thus, the sequence of developmental changes in the holometabolous insects, comprising those with a complete metamorphosis, represents something acquired very recently, since the insects split off from other animals and were already differentiated from related groups of arthropods. The larval stage, which is the real innovation in this mode of development, represents, of course, an adolescent stage and must be regarded primarily as the orderly transition from the embryo to the sexually mature animal However, it has become more than this in that it is really the interpolation of a stage which has undergone specializations that are truly its own, involving not only changed structures but also habits, habitats, and behavior. Under these circumstances the larva has been subjected to varied external influences which have led it to develop adaptations which do not appear to have visibly influenced imaginal structure.

It is quite true that the primary differentiation of larval types may be to some degree understood by the plausible assumption that they represent a sort of developmental fixation of either an earlier or a later embryonic stage, dependent upon the moment at which the embryo is born and becomes a free-living animal. Above and beyond such differences, the abundant and often profound and highly adaptive modifications of innumerable insect larvae obviously represent a phenomenon essentially identical with speciation but manifested only during adolescence. It is also essentially comparable to the sporadic occurrence of more than a single method of development in a single species, as noted particularly in certain Crustacea, to which the term "poecilogony" has been applied. But among animals other than insects this duplicity of evolutionary trends is no more than a freakish contingency There are, however, several analogous phenomena concerned, and we must exercise care that heterogeneous concepts be not confused. For example, the now well-known races of the European malarial mosquito, *Anopheles maculipennis* differ most clearly from one another in the shape and coloring of the eggs But these characters are neither embryonic nor juvenile since the egg shell is a structure produced directly by the adult mosquito The term "epigenotype" has recently been proposed to include the transitory juvenile characteristics in development as exemplified in the growth and differentiation of the wing in Drosophila. Such stages cannot be regarded as primarily adaptive without reference to their end product and likewise do not fall into the category of independent juvenile modifications such as those outlined above

The occurrence of natural variations among the individuals of animals and plants of the same species was one of the original principles on which the theory of natural selection was based. Later, just before the present century, the saltatory variations described by Darwin were recognized as a clearly different phenomenon from the constantly recurrent continuous variations presented by all bisexual animals, with the sole exception of polyembryonic siblings. Under their present designation of mutations they have become the *sine qua non* of

almost every genetical or phylogenetic inquiry dealing with any kind of organisms, plant or animal, and even among the still problematical viruses.

Insect mutations have received a major share of attention in experimental research, so far almost entirely with reference to anatomical structure. More recently, however, it has become evident that speciation among the insects is in many cases manifestly related to their instinctive processes As is well known, the instinctive behavior of the members of this group is far more persistently fixed than that of other animals, particularly with reference to their food habits, and the far-reaching effects of mutations in instinct must be reckoned with as factors influencing speciation. As nearly as may be judged, pronounced mutations in instinct are of rare occurrence and, on the basis of rather fragmentary experimental study, appear to be quite exactly comparable to structural mutations. Quite aside from genetical studies, numerous cases of suddenly altered instincts have been observed where racial types have developed, associated with shifts in the selection of food by species of insects which restrict their diet to particular species of plants. The insects, therefore, have given clear evidence that mutations in habits may initiate changes leading to actual speciation, a supposition made earlier by Lloyd Morgan, but without convincing proof.

Just as has been observed when dealing with structure, both physiological and instinctive variations among insects may be recognized which fall into the category of continuous or recurrent variations. Totally unpremediated experiments of gigantic extent have been performed on some of our common insect pests in connection with the widespread use of arsenicals and other insecticidal poisons to protect horticultural crops from insect damage. Thus, as a result of continuous treatment over large areas, races have been selected having greatly increased resistance to the virulent poisons such as hydrocyanic acid, calcium sulphide (lime sulphur), and arsenicals which are used as sprays From these observations it appears that there are extensive variations in resistance in many wild stocks, not hitherto suspected. Similar wide variations in winter hardiness which have been demonstrated in tropical flowering plants appear also to have their counterparts in insects and to be responsible, at least in part, for the extensions of range which have occurred on occasion among noxious insects, for example, the Mexican bean-bettle has shown itself quite able to withstand the rigorous winters of New England, although no such Spartan qualities are required in its native habitat The importance of such peculiarities to the zoogeographer are obvious

That the progress of modern biology has been greatly furthered through the medium of the insects cannot be questioned, but I do not doubt that others who have been sequestered in the company of worms, bacteria, protozoans, shellfish, or even of man himself may feel that their own charges should be placed in the ringside seats here reserved for the insects

AEROLOGICAL ASPECTS OF THE BIKINI BOMB TEST

By A. A. CUMBERLEDGE
Captain, United States Navy

WITH the bursting of the atomic bombs over the ships at the Bikini Atoll anchorage in July, the gigantic experiment known as Operation Crossroads reached its climax. Designed primarily to test the effect of the atomic bomb on naval vessels, the Bikini tests were not thereby necessarily limited in their conclusions to this single objective. Indeed, it is conceivable that certain other data may prove eventually as significant and valuable in related research as those for which the tests were primarily prepared. The recording of scientific data in all fields of research obtained at Bikini will affect in all probability, through interpretation and use, the life of mankind profoundly.

Mark Twain's classic lament was refuted at Bikini. There everyone talked about weather, and even if they could not control it aerologists were on hand to predict, on the basis of reports garnered from many sources, the prevailing weather for the test days. A world waiting at the radio on these days was assured that the meteorologists had been right—the weather on both occasions was perfect. Admiral Blandy in a recent speech said:

> This weather feature was all important. In fact the more I saw of my Task Force, the more I realized that I had nothing to worry about from them, the only thing that could throw us would be the weather. I felt fairly confident that my aerologists could predict it, but I wasn't sure they could control it. Looking back, I am almost willing to believe they did that.

OPERATION CROSSROADS

Under the direction of the Joint Chiefs of Staff, with the Army, Navy, and Manhattan District and various governmental agencies participating, 2 bombs were exploded at Bikini in July. The first bomb (Able Test) was exploded at a prearranged

Joint Army Navy Task Force One Photo

THE FIRST BIKINI BOMB EXPLODES

height in the air. In the second, or Baker, Test it was planned originally to explode the atomic bomb on the surface of the water, but this specification was changed to permit explosion at a set point beneath the surface of the water.

Target. The target in each instance was a group of naval vessels anchored at specified positions so that accurate data on the effects of the explosion on ships at given distances from the explosion could be recorded. Upon these target vessels platforms were built to accommodate not only instruments to record the effects of the atomic bursts, but also various types of Army and Navy equipment that the services desired to test.

But the gathering of scientific data was not restricted to the surface area affected by the explosions. High above the target points, drones were sent after each explosion through the deadly radioactive cloud which rose like a geyser and mushroomed over the area, to collect radiological data, followed later by conventionally manned planes Airplanes were also used to monitor and detect areas which, it was feared, had been contaminated by radioactive particles.

No glamorous figure of stage and screen ever faced a greater barrage of cameras than Operation Crossroads. Men delving into the epochal possibilities of atomic fission were careful that no photographic angle be omitted, from drones and piloted aircraft, surface operating and target ships, and Bikini itself, cameras recorded this moment in destiny

Split-Second Accuracy. The entire experiment, vast in size and tremendous in implication, from its inception to its climax was necessarily complex The gigantic undertaking of Task Force One commanded by Vice Admiral W. H. P. Blandy required not only the cooperation of 40,000 Army, Navy, and civilian personnel, the use of

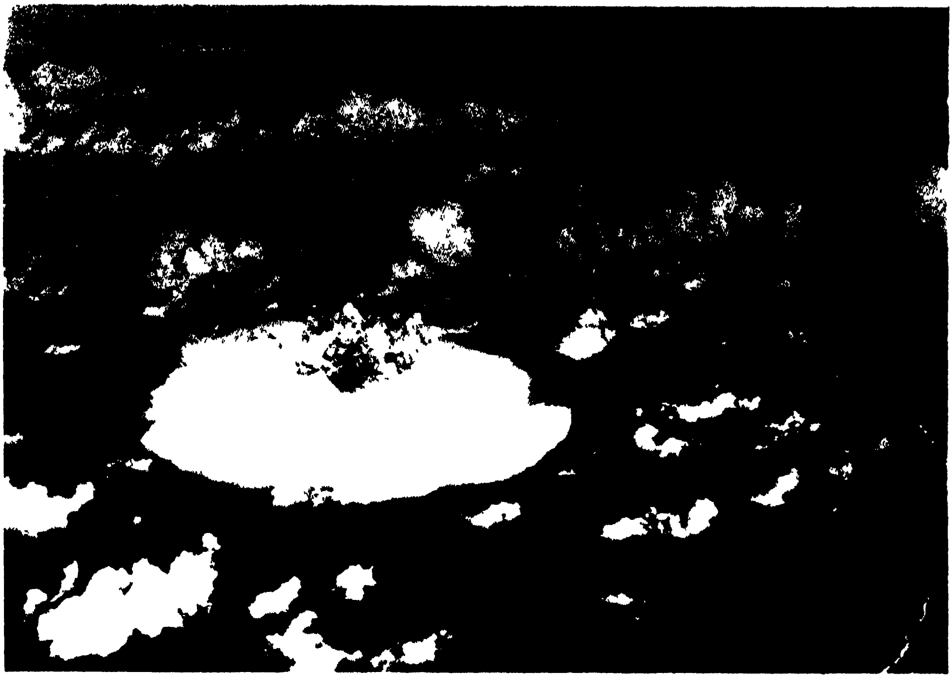
Joint Army-Navy Task Force One Photo

HUGE CLOUD COLUMN

FOLLOWING THE EXPLOSION OF JULY 1, 1946

more than 200 naval vessels and approximately 75 aircraft, but also the cooperation of almost all the military establishments in the Pacific Ocean area. The work of these thousands of men, as well as the expenditure of money and materials, had been so great and the collecting of data in the short time during and after the explosions had to be so swift and accurate that only a schedule in accordance with split-second timing would be effective. It is hard, therefore, to imagine a more dramatic concern for weather than there was in relation to the Bikini bomb tests. Practically every element could be guaranteed but the unpredictable whimsy of nature whereby winds can come and storms brew out of the far reaches of the equatorial Pacific.

The Importance of Weather. When the atomic bomb tests were postponed from May and June to July, public interest as well as the concern of Task Force One focused on the weather. The favorable weather anticipated by the original schedule



Joint Army-Navy Task Force One Photo

THE SECOND BIKINI BOMB EXPLODES

could not be counted upon during midsummer at a point in the equatorial areas where the winds of two hemispheres converge. This weather belt is subjected to extensive cloudiness, fickle winds, and torrential rains. The zone, called the equatorial front, moves north and south but lags behind the movement of the sun across the equator. Thus, generally, toward August the front reaches Bikini. This does not spell cloudy weather every day, but at such a time clear days in the northern Marshalls will be intermittent. The front on any given day may be a considerable distance either north or south of Bikini Atoll, but if all the positions are averaged the mean position lies across Bikini.

Thus it is clear that the change of schedule to July jeopardized to some extent, insofar as suitable weather was concerned, the successful accomplishment of the tests. Since, however, certain days might be ideal for the tests, the aerologists, their task complicated by the oscillations of the equatorial front, had to be prepared to advise the Task Force Commander when suitable weather conditions were expected.

METEOROLOGICAL REQUIREMENTS

Weather was important on several counts First, it was necessary to insure radiological safety of all personnel. It is hard to imagine anything more lethal than the radioactive remains of atomic fission We are dealing with an all too possible nightmare when we consider the equivalent of tons of radium floating loose in the atmosphere in deadly concentrations. To guarantee that the tests would not be suicidal, it was necessary for aerologists to make sure that the winds at all levels up to the base of the stratosphere would be in such a direction as to carry the contaminated atmosphere away from personnel participating in the tests.

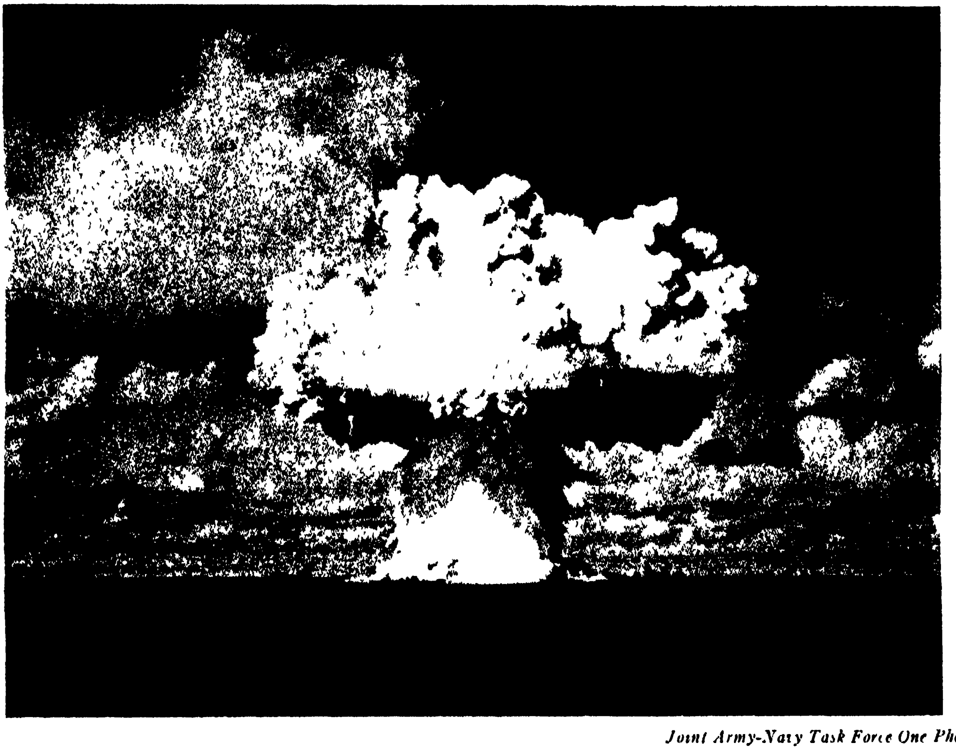
The Task Force Commander, Admiral Blandy, had the following to say about safety:

> If our manned planes were to pass through the cloud, which might happen after it had broken up or if part of the cloud dropped some of those fission products on one of the 150 naval vessels of the Task Force operating outside the lagoon, it might mean serious illness and even death to some of the 40,000 men participating in or observing the tests. So it was all-important that we know exactly what the winds were doing at all levels. You can see that our weather forecast for Able Day was not just a matter of "fair and warmer," it was a matter of life and death.

Although the factor of safety was primary, there were still important considerations related to operational requirements. The weather had to be suitable for flight operations since certain data could only be collected if there were unrestricted aerial observations. All crews on aircraft had to be able to see the array of target ships, and, in the Able Test, from an altitude of 30,000 feet the bombardier of the B-29 carrying the bomb had to have an unrestricted view of the target. Successful photographic coverage, of course, was dependent upon clear weather. Although the critical altitude on Test Baker was lowered to 20,000 feet, an unrestricted view from even greater heights was desirable. In both tests, therefore, the meteorological requirements were equally stringent.

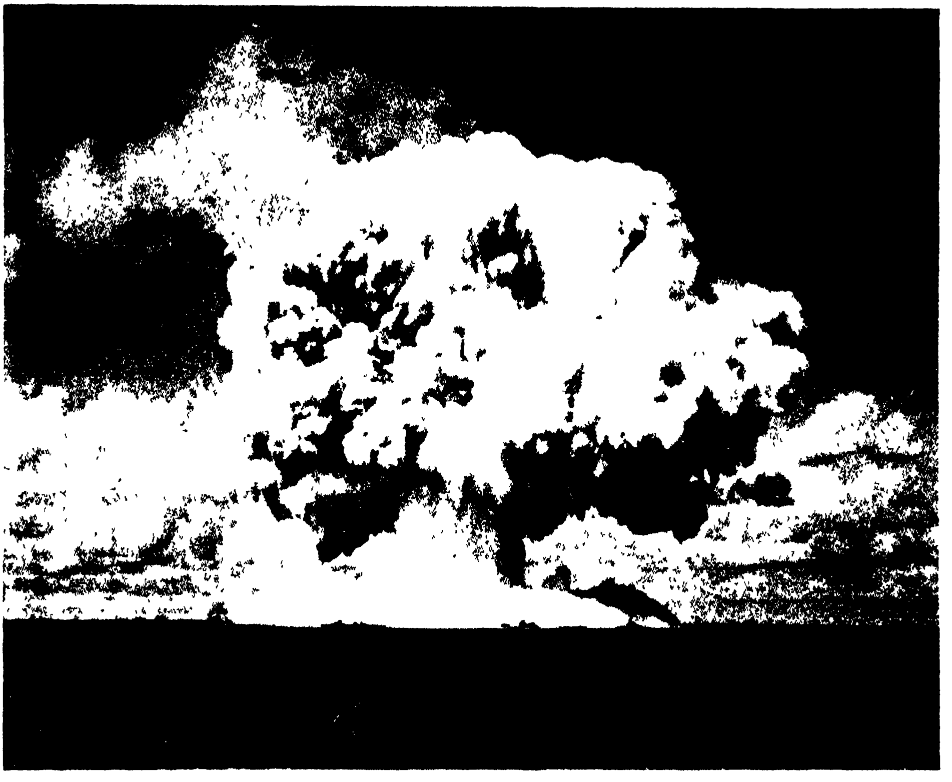
Out of these requirements came the decision that the tests could be run off only under these conditions:

1. All the winds up to 60,000 feet had to be from a safe direction.
2. The total amount of cloud below 30,000 feet on Test Able Day and 20,000 feet on Test Baker Day could not exceed three-tenths. If the cloud conditions were between three- and seven-tenths, it might be possible to accomplish certain tasks under local orientations of



Joint Army-Navy Task Force One Photo

THE LADY OF THE LAGOON



Joint Army-Navy Task Force One Phot

THE COLUMN OF WATER BEGINS TO FALL

cloud, but on the whole that possibility would be in question If the total cloud cover was seven-tenths or over, the operations under such conditions would involve serious loss of data

Furthermore, time was of the essence. If accurate forecasts of the weather could not be predicted 24 to 36 hours in advance, the only alternative open to the Task Force Commander was to prepare to explode the bomb each day The utter impracticality of this procedure is at once evident, for the complexity of Operation Crossroads, with its vast instrumentation, meant that once it had been set in motion it would have to be canceled before midnight of the same day in order not to waste the following day. In other words, the operating personnel required time to set and check instruments and to take personnel away from the target ships. It is obvious that to prepare for a test each day on the chance of ideal weather would mean that after 4 or 5 days the crews would be exhausted, and, if the tests were still delayed at the end of the period, complete rechecking of all instruments would be required.

Upper-wind forecasts had to be prepared at least 24 hours in advance in order to allow time for placing ships and aircraft bearing operational personnel in sectors where they could accomplish their missions and at the same time be free from radioactive contamination If these forecasts were wrong, it would be easy to change the location of fast-moving aircraft, but to execute a similar maneuver for comparatively slow-moving ships would create a problem of such magnitude as to render it physically impossible And, faced with the dreadful possibility of a sudden shift

in wind direction which would rain radioactive particles down on manned ships, aerologists had to formulate their plans and make decisions which would guarantee the Admiral information so that he might protect the fleet from such a danger.

Upper-Wind Studies. One of the first tasks confronting the aerologists assigned to the staff of Joint Task Force One was the preparation of elaborate and detailed studies of all available upper-wind data in the Marshall Islands area. Statistical studies indicated the maximum dispersion of radioactive cloud that could be expected at all levels and the probable frequencies of wind directions. In preparing operational plans for ships and aircraft and arranging for alternatives in event an emergency arose because of changed wind direction, these upper-wind studies were invaluable. The statistical studies were likewise important in determining the plans for insuring safety to personnel by determining the exact nature of the hazards which might be expected. Upper-wind data for the Bikini area were not available. Careful interpolation of wind data obtained at Eniwetok in 1945, together with scanty records from the southern Marshalls, produced information sufficient for preliminary planning. Then, as the operation progressed, it was possible to augment and modify the original studies on the basis of data received on the spot.

The available data revealed that the average distribution of winds in the Bikini area was easterly from the surface to 25,000 feet. Between 25,000 and 35,000 feet, the winds shifted into westerly quadrants and stayed westerly until the tropopause (about 55,000 feet) was reached. Above the tropopause, the wind direction was again easterly. Although this was true throughout the year, there was evidence that 25 percent of the time during July and 50 percent of the time during August winds from easterly quadrants existed at all levels from the surface to the stratosphere. On the basis of these percentages, the requirement was made initially that there be easterly winds from the surface to 60,000 feet, but this stipulation was relaxed as operating experience made it apparent that Task Force One could not afford to pass up a day with suitable cloud conditions because of stringent wind requirements. Moreover, it was found that various upper-wind combinations could be used with safety In determining acceptable wind conditions, each case had to be decided in the light of the radiological hazards to ships, aircraft, and inhabited islands in the area.

Reporting Schedule. In the vicinity of the Marshall Islands, at Marcus, Wake, Eniwetok, Kwajalein and Tarawa, a network of Army and Navy weather stations was established, equipped with radar upper-wind recording instruments. Manned by specially trained personnel, including specialists from the United States Weather Bureau, these stations furnished information four times daily on wind conditions up to approximately 60,000 feet.

The network of island-based stations was augmented by ship stations in which aerological units were established for the purpose of making upper-air observations by radar. Observations from these units were scheduled every 3 hours, and just prior to and during the test periods, every 90 minutes. At first glance, it may seem that an excessive number of upper-air soundings were required, but we could not depend on less frequent reports in view of the fact that the very lives of personnel were dependent upon wind direction.

On the basis of upper-winds reports and the regular weather maps, daily forecasts of expected winds to the level of the stratosphere were prepared for the succeeding 36 hours. It was upon the basis of these forecasts that the sectors in which

ships and aircraft could safely operate were designated.

Weather forecasts were not based only upon reports from the network of island and ship weather stations in the Marshall Islands area. A far-flung network of reporting units covering the North Pacific insured adequate coverage. From China and Siberia to a line midway between Hawaii and the West Coast, and from the equator to the Aleutians, reports were received from the regular Fleet Weather Central broadcasts from Pearl Harbor and Guam.

A Fleet Weather Central is an aerological unit charged with furnishing weather information to the Fleet. The Weather Central not only collects reports, edits, compiles, and broadcasts weather data at specified times, it also analyzes, encodes, and furnishes weather maps to units too small to perform these functions. Let a storm or typhoon start its reeling course at sea, and the Fleet Weather Central immediately notifies all naval vessels, sends out warnings, locates the track of the storm, and estimates its intensity.

These Fleet Weather Centrals, developed just before the beginning of World War II, are an indispensable link in the weather chain. Without the Weather Centrals, it would be impossible to intercept a workable coverage of weather reports in time to prepare a forecast of any value. If weather forecasts are to be reliable, the aerologist must have an ample coverage of accurate weather reports and they must be received promptly; forecasting on the basis of local data is bound to prove unsatisfactory.

In the Marshall Islands area, Army and Navy weather stations were required to make more frequent reports than ordinarily required.

Because of Bikini's isolated position in the Pacific, it was impractical if not impossible to establish a sufficiently dense network of shore-based weather stations within the desired radius. The alternative of establishing ship bases was not wholly satisfactory in view of the time involved. Obviously, a ship can remain on station only a certain length of time and then must be relieved, so that additional ships must be provided to prevent lapses in receiving observations. On the basis of the number of ships involved, the personnel, the logistics, and overhaul problems, such an arrangement can only be justified on a long-time basis.

Weather Reconnaissance Aircraft. It was therefore advisable to depend largely upon weather reconnaissance aircraft—3 Army Air Force B-29's and 4 Navy PB4Y-2's—with trained weather personnel aboard. Equipped to gather weather data, these planes could be dispatched swiftly to any point. Radioing back their reports to base, these planes covered thousands of square miles of ocean areas. In addition to the two scheduled daily flights lasting 12 to 14 hours, these airplanes were called upon when special information was needed.

Illustrative of this special use is the procedure used on test days. On Able Day the weather planes were routed over Bikini Atoll. The first plane arrived about 0130 local time and immediately made contact with the U.S.S. *Mt. McKinley* over voice radio. After making an upper-air sounding over the lagoon and giving these data and a complete weather report to the ship, the plane then proceeded eastward on its assigned flight path. Additional planes arrived at 0330 and 0530 local time, each following the procedure of the first plane. On Baker Day the routine was altered so that all 3 planes arrived in the area at 0330. After an upper-air sounding had been made and reported by one of the planes, they were stationed at 1,500, 8,000, and 15,000 feet, respectively, at positions approximately 40 miles upwind from the target, and from these points aerological officers reported cloud move-

ments toward Bikini lagoon. Once the atomic bomb was exploded, they proceeded on their assigned flight. By using the planes in this way, it was possible to obtain meteorological information directly from the levels above and near the target area.

The technique of aerial weather reconnaissance developed during World War II was an outstanding contribution to the forecasting of weather for operations.

Weather Conferences. Once all information was collected, it had to be placed on maps, and analyses which included both surface and upper-air charts had to be made. Since this service required large numbers of trained personnel, it was necessary to establish a special Crossroads Weather Central at Kwajalein rather than on a ship, where restrictions of space and communications would be a problem. But this separation of the aerological unit from the U.S.S. *Mt. McKinley* did not mean that it was not an integral part of the operation, for the installation of a voice radio conference circuit established between the *Mt. McKinley* and Kwajalein made regular and special weather conferences possible.

Throughout the Task Force there were 75 aerological personnel, of whom over half were qualified aerological officers. This total does not include the personnel at the Marshall Islands weather stations or in the entire Pacific network. Although the figure may seem high, it proved to be the absolute minimum for efficient operation.

The formulation of a final official weather forecast for operation on the following day required the coordinated efforts of both the Staff Aerological Unit and the Crossroads Weather Central. Early in the morning, the unit on the *McKinley* and the Weather Central made weather forecasts for the next day upon the basis of the analyses of the 1,200 Greenwich civil time reports. These forecasts were the subject of the early morning weather conference, during which the Weather Central contributed necessary details of the upper-air analyses as well as information obtained from personal conversation with aerological officers aboard the reconnaissance plane. The Staff Aerological Unit supplied essential cloud data and trends obtained from local indications in the Bikini area. After a full discussion, the Staff Unit dictated the official forecast. This forecast, which was immediately distributed to all Task Group Commands, was used in preparing operational plans.

Each morning at 0830, the official weather forecast was presented in detail to the Task Force Commander and his operational staff. Following this briefing, the Admiral made a complete operational decision and the staff simulated actions to be taken on the actual test days. This procedure was of great value in training all aerological personnel.

In the late afternoon, another weather conference with the Weather Central was held and the morning forecast was modified if observations warranted it. If modifications were made, they were presented to the Task Force Command at 2200 local time and Task Group Commanders were notified.

The elements of each completed forecast included the amount (stated in tenths) of low, middle, and high clouds; base and top altitudes of the low clouds and the altitude of the other cloud layers; precipitation if expected; the wind direction and velocity of 5,000-foot increments from the surface to 60,000 feet; height of the tropopause; visibility, temperature, and relative humidity.

In view of the fact that only once during the entire period covering all the test and rehearsal days did an operation have to be canceled because of inaccurately predicted weather, it is clear that the fore-

casting at Bikini was effective. Further indications of the efficiency of the forecasting system can be gained from a summary of the weather forecasts for Able and Baker Days.

WEATHER FORECAST FOR ABLE DAY

The weather map for 1200 GCT on June 29 (2300 local time, June 29) indicated the presence of a widespread high-pressure cell, the center of which was located 600 miles north-northwest of Midway. This anticyclone was drifting slowly east-northeast toward the Gulf of Alaska, thereby causing a significant weakening of the surface pressure gradients in the Marshalls. An extensive low-pressure system was located 250 miles west of Guam. Although this low did not have the intensity of a severe storm or a typhoon, it was causing overcast skies and showers as far west as the Philippines and as far east as Truk. It was not expected that the low-pressure area would deepen or show much movement in the next 48 hours.

On the twenty-ninth a west-to-east trailing upper-air low-pressure trough passed the Bikini area and caused the development of a high-pressure ridge at altitudes above 20,000 feet. This passage of the upper-air trough, which was traced accurately through Eniwetok and Bikini, intensified and produced severe thunderstorms on the thirtieth in an area 400 to 500 miles directly east of Bikini. The development of the upper-air wedge subsequent to the passage of the trough caused the antitrades to flow from the northwest at altitudes above 20,000 feet. From soundings made by weather reconnaissance aircraft, there was also noted a dying out of the air at levels above 6,000 feet. This was correctly associated with a general subsidence and invasion of drier air caused by the upper-air high-pressure development. This factor eliminated the likelihood of a layer of middle clouds in the Bikini area. The equatorial front in the region of the Marshalls was located just south of Jaluit and north of Kusail and Ponape. The front was *not* expected to influence the weather conditions at Kwajalein or Bikini for the next 36 hours.

Upon the basis of these facts, the following forecast for the Bikini area for July 1, 1946, was prepared and presented to the Task Force Commander at 0830 local time June 30:

> Two- to three-tenths cumulus clouds with bases at 1,500 feet, tops at 5,000 feet. No middle clouds. About six-tenths of high cirrus clouds at altitudes above 30,000 feet Total cloud cover below bombing altitude at target time two- to three-tenths. Winds aloft expected to be easterly 10 to 15 knots up to 15,000 feet, variable at 2 to 8 knots between 15–25,000 feet and northwesterly 25 to 35 knots above 25,000 feet

The Task Force Command thereupon scheduled Able Day operation for July 1; time of bomb drop, 0830 local time.

During the day of June 30, the weather reconnaissance aircraft soundings showed significant increases of moisture at levels below 5,000 feet in the Bikini area, but this factor was expected to influence only the nocturnal convective cloud condition. The cloudiness would tend to reach a maximum near dawn and then rapidly diminish during the early forenoon. At the conference held at 2200 June 30, no change was made in the weather prediction announced in the morning. Between the hours of 0100 and 0600 local time on July 1, weather reconnaissance aircraft made continuous cloud observations and upper-air soundings in the Bikini area. Seven- to eight-tenths cumulus clouds, occasionally swollen to 13,000 feet and accompanied by frequent lightning with showers, were reported in the immediate vicinity of the lagoon. While this nocturnal convective activity had been anticipated, its intensity was significantly greater than expected. At 0500 local time on

July 1, another weather briefing was held, at which time the forecast of two- to three-tenths cloud coverage was reiterated. It was pointed out that the nocturnal cloudiness would tend to reach a maximum at dawn and from that time onward would diminish.

This prediction was substantiated by the command aircraft which was checking on weather conditions at the time, as well as by the Aerological Officer aboard the weather reconnaissance aircraft flying in the immediate vicinity of Bikini. Relying on this additional information, the Task Force Command directed the B-29 which was to drop the atomic bomb to take off from Kwajalein on the revised target time of 0900. After 0700, the cloudiness continued to diminish, and at 0830 the total amount of cloud was estimated at from two- to three-tenths. The forecast had proved accurate in all respects.

WEATHER FORECAST FOR BAKER DAY

The forecast for Baker Day was equally spectacular. Perhaps it was the more remarkable of the two forecasts, for it was made during a period of unsettled weather and heavy tropical rain. In the weather conference on the morning of July 23, it was pointed out that a typhoon was developing in the Marianas area and that a general synoptic situation was occurring similar to that which had produced favorable weather for the Able Day operation. Widespread subsidence was therefore expected east of the typhoon area. This optimism was somewhat premature because a surge of the trade winds from the south sent the equatorial front into the Bikini area during the night. Thunderstorms and heavy showers persisted throughout the Bikini area and surrounding quadrants.

While this deterioration of local weather prior to the morning conference of the twenty-fourth made the members of the Staff Aerological Unit apprehensive, they found no positive reason to modify the original favorable prediction for clearing weather on the twenty-fifth. It was predicted that the equatorial front would move northwest of Bikini and drier air would become dominant.

Thus the following forecast was presented to the Task Force Command:

> Weather forecast for Bikini area for 25 July 0800 to 1600 local time. Three- to four-tenths cumulus clouds, bases at 1,500 feet, tops mainly 3,000 to 8,000 feet, but isolated tops developing to 15,000 feet. Little or no middle clouds. Three- to four-tenths of high clouds near 32,000 feet. Total cloud cover below 20,000 feet three- to four-tenths. Winds aloft easterly 15 to 25 knots at all levels.

This favorable forecast was accompanied by certain reservations. The outlook certainly did not warrant complete confidence. The Task Force Command was advised that weather reports, particularly those relating to winds and the air reconnaissance reports, received during the day would indicate whether or not small amounts of clouds would prevail. Baker operation was thereupon scheduled for the following day, but with a reservation that a postponement might be made after the evening weather conference if the favorable forecast did not hold.

During the day the aircraft weather reconnaissance reports indicated that the frontal wave had definitely passed Bikini. Furthermore, although the conditions eastward were showery and unsettled, there was improvement in the southeast, and cumulonimbus clouds in the Bikini area were being sheared off above 20,000 feet by a northerly subsiding flow. By late afternoon, there was a decrease in convective activity resulting from this subsidence. At the 2200 weather conference, it was stated that the frontal wave would continue northwesterly and the equatorial front would stay north of Bikini.

The forecasted heights of the tops of low clouds were reduced to 8,000 feet. Therefore good weather would prevail.

All late information confirmed the forecast, and the Task Force Command ordered the test to go off on the schedule determined in the morning.

During the evening conference distant lightning played to the north of Bikini in the direction of the equatorial front. Radar observations revealed shower activity 30 miles north and several showers to the east and southeast. Since the winds were southeasterly, it was probable that one of the small showers would pass over the U.S.S. *Mt. McKinley* and that thereafter conditions would improve. A light shower proved this assumption correct.

Weather reconnaissance flights undertaken at 0300 local time showed that conditions were favorable for the operation. The upper-air radiosonde observation revealed an isothermal layer between 7,000 and 8,000 feet, with relatively dry air above which prevented clouds from developing to any extent. This forecast was verified by the fact that during the critical time for photography and flight operations, only two- to three-tenths of small cumulus clouds were present. Thus the forecast made during one of the worst days encountered at Bikini proved valid.

EXPLOSIONS AND EXISTING WEATHER CONDITIONS

Prior to the atomic bomb tests, there was widespread speculation as to the effect of these explosions upon existing weather conditions. Prophecies ranged from the formation of rain showers through violent thunderstorms and, in a few cases, even to the formation of a typhoon.

Basis of Forecasts. Forecasts of violent weather reactions were based primarily upon two major considerations. It was thought that the large amounts of moisture in the tropical air mass at Bikini, plus the large quantities of water expected to be evaporated from the water surface, would be extremely conducive to thunderstorm formation upon the release of the large amount of atomic energy into this mass. Second, it was believed that the high atmospheric concentration of ionized particles resulting from the atomic burst would serve as nuclei for condensation and thus be conducive to the formation of clouds and rainfall.

If the mechanism producing a thunderstorm is examined, it will be found that these storms require convection, or mechanical lifting, for at least several hours over relatively large areas as well as the proper vertical distribution of moisture. Convection, or lifting, of the air mass continues even after the full-fledged storm has developed. The atomic bomb is unable to produce convection of the sustained type. The bomb produces a sudden impact of energy, and convection ceases as quickly as the hot ball of gases cools on ascent and reaches its maximum altitude. The atomic explosion, therefore, consists of a violent, quick action over a relatively small area. A great deal of speculation centered on whether this sudden impulse might not precipitate thunderstorms if the atmosphere were just on the verge of instability. The answer to this question is that the release of energy is so sudden that the atmosphere is unable to rearrange itself to take advantage of the instability in the time allotted.

Any examination of the atmosphere will show that at all times there are suitable nuclei present for water vapor condensation. These nuclei are sufficient for cloud formation in all areas but particularly over ocean areas where very abundant hygroscope salt nuclei are present.

First Tests. In the New Mexico test, a thunderstorm preceded the atomic explosion. Without considering the sequence of events, this observation is not infrequently distorted to imply a genetic relationship. The fact that the storm occurred *before* the explosion proves that there was no connection between these events.

Many references have been made to rainstorms associated with, or resulting from, the atomic bombs dropped over Japan. These have been, in the main, speculatory and are without substantial meteorological basis. If rain showers did occur in Japan, it is conceivable that they were the result of convection caused by the widespread fires which sustained it for a long period of time after the explosion. This phenomenon has been noted over large forest fires and over burning European cities during the war.

Shortly after the atomic air burst on Able Day at Bikini, many small, light rain showers developed throughout the northern Marshalls. The clouds associated with these showers extended from 2,000 to approximately 6,000 feet. In the path of travel of the radioactive cloud, some measurements were made of radioactive rain. The amount of radioactivity was so small that it was of only academic interest. Some attempt has been made to associate this radioactive rain with the formation of the showers. The showers were, however, very widespread and were accounted for on the basis of the existing atmospheric conditions. The radioactive rain was a result of radioactive particles above the tops of the clouds falling into the rain cloud or of particles being present in the area where the cloud formed and, thence, falling out in the rain.

Inspection of ship and shore records in the Bikini area and pictures taken over the lagoon revealed that the only detectable changes which took place in the wind or atmospheric structure were the momentary effects of the blast and heat wave and the violent changes which took place in a rather limited area in the vicinity of the explosion. It is important to note that the cloud pattern over Bikini lagoon was undisturbed except at that point where the doughnut-shaped cloud formed around the explosion.

At the present time there are now two adequately documented cases of the inability of atomic air bursts to cause rainfall, the New Mexico test and the Bikini Test Able. In both instances, the thermodynamic and moisture structure of the atmosphere was properly staged for rainfall production. The mechanism for the release of energy in the form of rain showers or thunderstorms in each case was missing.

Cloud Chamber Effects. One of the most interesting phenomena noted in connection with the two atomic explosions at Bikini was the cloud chamber effects. Each explosion was accompanied by the formation of a dome-shaped cloud which later took the form of a doughnut as the top of the dome was disrupted by the resulting upward movement of the ball of hot gases in the first test and by the water column in the second test. This doughnut-shaped cloud extended from the surface to approximately 1,000 feet. The cloud effect can be attributed to rarefaction in the wake of the blast wave with the accompanying adiabatic cooling. The existing relative humidity was high enough so that the adiabatic cooling caused condensation.

In Test Able, the intense heat and the resultant ball of hot gases developed by the explosion created a large convective cloud similar in appearance to the cauliflower structure of the cloud of a thunderstorm. As the hot gases rose, there was constant cooling from the outside and the excessive moisture evaporated from the water surface began to condense. The result was the formation of a relatively narrow, towering cumulus cloud. The rate of ascent was very rapid, and in about 10 minutes the cloud reached a height of approximately 35,000 feet. As quickly as the cloud reached equilibrium with its surroundings (its maximum altitude), further convection was shut off. All further processes were concerned with the dissipation of the atomic cloud.

The identity of the cloud was maintained for about 50 minutes; then the existing

winds dispersed it into 3 main sections. The portion from the surface to about 15,000 feet moved to the west-northwest; the portion from 15,000 to 25,000 feet was diffused in all directions by the light and variable winds in that region; and the part above 25,000 feet drifted to the south-southwest. When the cloud was no longer visible, its trajectory was computed from the observed wind soundings and was shown ultimately to have taken a path at all levels to the northwest. It recurved in a large arc, flowed to the northeast, and passed over the vicinity of Wake Island 48 to 72 hours after burst time.

As the cloud passed upward through the freezing level at 19,000 feet, a characteristic false cirrus, or scarf, cloud formed and gave the appearance of cascading downward. Within 2 or 3 minutes, this false cirrus was enveloped by the rising warmer convective and turbulent currents and became indistinguishable.

Second Bikini Test. In connection with Test Baker, much interesting speculation was offered on the aerological effects. With the atomic bomb exploding beneath the surface of the water, would there be the same ball of fire as had been observed before? It was believed that a ball of fire, together with tremendous amounts of liquid water and water vapor, could be expected to result in a rain of spray and the formation of a convective cumulus cloud. If there was no ball of fire, the result would merely be the expulsion of of liquid water only into a plume of spray. Since the collision between tiny suspended cloud droplets, if any, and large falling water drops would remove the former, it was considered that no significant cloud could be formed by the simple injection of water into the air.

Actually, Test Baker produced a vigorous ball of hot gases with the attendant liquid water and vapor. Within seconds after the previously described cloud chamber effect, a column of water and spray was observed having a width of approximately 2,100 feet, topped by a water vapor cloud that rose to 8,000 or 9,000 feet within 3 minutes. This cloud subsequently settled down until its top was about 7,500 feet. At the same time clouds formed underneath an isothermal layer present in the atmosphere at about 6,000 to 7,000 feet. This cloud spread out to a diameter of about 6 miles. A rain of water spray continued to fall from the cloud layers for 15 to 20 minutes. The source of moisture for this rain came completely from the water ejected from the lagoon.

Effects of Local Weather. It can be said that no significant meteorological influence other than purely local cloud effects resulted from the Bikini tests Able and Baker. The evidence obtained also sheds light on the reverse of this problem, that is, whether it is possible to destroy certain destructive atmospheric phenomena of nature such as typhoons, hurricanes, and tornadoes by a sudden release of man-controlled energy. At present this possibility seems remote. It is logical to deduce that the limitation of available energy in the atomic bomb which prevents storm formation will also prevent storm destruction. The tremendous energies required over considerable periods of time to cope with natural phenomena are still not attainable in man-made explosions similar to the Bikini detonations. It may thus be concluded that a local rainstorm can neither be started nor dissipated by an atomic explosion, no matter how favorable atmosphere conditions may be. The forces exerted on the atmosphere by the atomic explosion will not appreciably affect the surrounding wind, temperature, or pressure pattern except momentarily and in the very immediate vicinity of the burst. Man has not yet reached the stage where he can compete with the forces of nature.

A CASE REPORT ON A HISTORY OF SCIENTIFIC IDEAS*

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PRESENT-DAY emphasis on the importance of science in civilization and its place in our culture is evident to anyone who reads educational publications like the Harvard Report and books and articles in the more popular categories, such as *Teacher in America*, by Jacques Barzun, or William Laurence's reports on science in the *New York Times*. College faculties are discussing these days how best some unifying, integrating force can be restored to college curricula to achieve a harmony like that in the days when theology was the capstone of learning. Nearly 40 years ago, however, Thorstein Veblen recognized such a harmonizing force when he wrote: "The growth of the scientific point of view begins further back than modern Christendom, and a record of its growth would be a record of human culture."

This paper is a report on the history of the scientific point of view that has actually been taught now for 25 years in a liberal arts college for women. It is an attempt to show the importance and the significance of the subject matter in the opinion and from the experience of the students themselves. And it is also a plea to scientists to welcome such courses in undergraduate colleges despite the fact that they may be taught by historians or philosophers who are not experts in science themselves. The historical method and approach reach many who would otherwise be deterred by their own unfamiliarity with technical terminology. It gives meaning to the sciences they may have had to study as part of their education and it helps to relate science to knowledge as a whole. It also re-enforces from an outside point of view the importance of the scientific work science students have been studying, leading them on to further work in the field or to further reading and study about it.

In 1922–23 the history of the scientific point of view was first given at Goucher College, Baltimore. Since then, with the exception of one year, it has been given every fall, including this past one. It is an elective in the Department of History offered to junior and senior students, all of whom have already had in college at least a year of laboratory science of one or more kinds. These students come from every major department in the college: music as well as biology; Latin, fine arts, philosophy, history, and economics as well as chemistry, physics, physiology, mathematics, and psychology. For the period of the fall term they give one-third of their working time to the demands of the course and in that time they trace the development of science and the sciences through the ages and through the civilizations of western Europe from primitive man to Pasteur.

They are guided in their study by a carefully prepared syllabus and by reading lists supplemented by their own detailed and intensive study of a man or an idea from each of two periods, that of the Greeks and that of the beginnings of modern science. Classroom talks and discussions help them to trace the thread of science through the maze of historical material of all kinds. Emphasis upon certain major documents like the Edwin Smith Surgical Papyrus,

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the Hippocratic Oath, and Francis Bacon's dream of Salomon's House in his *New Atlantis* give them glimpses at firsthand of the long road science has traveled through the ages. Admittedly a rapid survey, avoidance of the curse of superficiality is attempted by insistence throughout the course upon its providing merely a framework or outline for reading, study, and thought for many years to come. The syllabus provides the skeleton of an idea, the development of science; the students' own prior information and all their reading and study contribute to filling out that skeleton into a vital, growing body of knowledge about science and its spirit. And that spirit is of major importance today.

How can such a course be taught? Its originator, after undergraduate courses in laboratory science as well as in the humanities, was trained in history under James Harvey Robinson and greatly stimulated by his course at Columbia University in the "History of the Intellectual Class of Western Europe." She earned her own graduate degree by tracing the gradual acceptance through the centuries of the Copernican theory of the universe and found it was not a long step from an interest in the history of an astronomical theory to a major interest in the progress of science itself: What helped it advance, what hindered it? What sciences first developed, and how far were the men of science great because of, or in spite of, the era in which they did their work? Finally a long year of work in London on science in the seventeenth century under the guidance of Dr. Charles Singer and of Professor Wolf, together with constant revision and further reading—all this has reinforced and strengthened the teaching of the professor in charge. So also have the successive classes who throughout these years have made their contributions to her training by their points of view, their questions, and their suggestions. And it is to these students that the director of this course turned for an answer to the questions: Was the course worth while? And, if so, in what ways? What did it mean to the students themselves as distinct from what the instructor thought it should mean?

During these 25 years 445 young women have completed the course satisfactorily and have since graduated from college. Some 30 more are still in college. To those who have graduated a two-page questionnaire was sent, requesting their help in preparation for this address. Sixteen of the questionnaires were returned for lack of the right address, 2 came back unanswered, but 64 percent of those queried troubled to reply, many of them with amplified statements and accompanying letters. The replies came from members of every class from 1923 to 1946 and from almost every major department, providing a wide cross section of interests and activities. Almost half of those replying, 134, had majored in college in some science; the other group of replies, 139 of them, was divided between the 38 who had majored in the arts, languages, and literature and the 101 who had majored in philosophy and the social studies. All of them were asked what the significance of the course had been to them as part of their undergraduate curriculum and what specific use they had made of bibliographies, facts, and ideas in their later work. Had it consciously affected their intellectual life after college (Table 1)?

To judge by the item most frequently checked (238 times by 273 replying) and also most frequently double-checked (65 times), as the one of most significant value for all groups, this was to develop an interest in the history of ideas. For the older alumnae this, they say, has meant wider horizons, a more tolerant point of view, and greater patience with man's slow progress. That ideas have a history of their own has turned the attention of some alumnae to tracing specific ideas in other fields, not just in science—in fact, one calls

it "potential dynamite"! Another wrote that it made her "more conscious of [her] heritage and of the rise and fall of ideas and hence of the progresses and retrogressions of civilization as a whole."

The next largest total number of checks made by all three groups together emphasized the sense of historical perspective gained through the study of the scientific developments down through the ages together with a widened knowledge of notable people. "It gave me a feeling of the orderly progress of events." To one it gave the "sweep" of history. To another it took science "out of a vacuum" and "related it to its background, social, economic, cultural." Or, as another phrased it: "It took science out of the laboratory and made its story fascinating and real." It aroused interest in "scientists as men and products of a certain age, heritage, and culture." It made another aware of the "interrelationships of science with politics, art, and every phase of life." In short, science became "a part of human life, and of the development of man."

TABLE 1

Analysis of Replies to Questionnaire on Significance of Course in History of Scientific Point of View

Number of Times the Following Statements Were Checked by Alumnae Classified According to Their Undergraduate Majors

Statements Checked by 273 Alumnae	Alumnae Divided According to Undergraduate Majors			Total
	Science 134	Phil & Soc Studies—101	Lang, Lit & the Arts—38	
Developed interest in the history of ideas	120 (35)	84 (25)	34 (5)	238 (65)
Developed historical perspective	96 (21)	79 (17)	30 (10)	205 (48)
Widened knowledge of notable people	99 (6)	76 (6)	29 (1)	204 (13)
Increased appreciation of scientific method	81 (5)	79 (11)	24 (1)	184 (17)
Opened possibilities for later reading and study	88 (17)	66 (6)	29 (9)	183 (32)
Widened range of reading done	79 (4)	68 (4)	23 (1)	170 (9)
Related various fields of the curriculum to each other	63 (6)	58 (12)	28 (9)	149 (27)
Synthesized science courses	68 (10)	23 (1)	4 (1)	95 (12)
Increased understanding of laboratory procedures	11 (1)	9 (0)	0 (0)	20 (1)
Led to election of more science or mathematics courses	4 (0)	2 (0)	5 (0)	11 (0)

(Figures in parentheses indicate the number of times this item was double-checked to indicate that it was considered the most significant result of the course.)

For the small group of majors in the arts, languages, and literature, the development of an interest in the history of ideas or of a historical perspective was hardly more im-

portant than the fact that for most of them it integrated the curriculum, "relating science, literature, the arts, as parts of a whole, not as specially growing fields of endeavor." And for many of the younger alumnae, majors of the social studies, that too was for them as important a result as the development of historical perspective or even the increased appreciation of the scientific method, to both of which their group as a whole gave second place. Their answers blend into each other, though, as shown by their use of the terms "synthesis" and "correlation" of knowledge.

Curiously enough, while more than half of the science and of the arts majors said they had gained in the appreciation of the scientific method, to three-quarters of the students of the social studies that was a major result of the course. "Ways of thinking," "broad point of view," and "liberal ideas" are not of course definitions of the scientific method, but apparently in the minds of these alumnae they are linked with that idea. Specifically, some have tried to apply the scientific method in teaching illiterate soldiers, in working with engineer husbands, in pursuing graduate study. It is a theme running through many of the answers: "It made me *think*." It developed "understanding of the importance of methods of observation and of scientific honesty as applied to social studies also." It was concerned with "challenging mental processes and their logical evolution." "I do not remember facts or details [this from a graduate of 1929] but I know that certain ways of thinking and reasoning and a broad outlook as well as a certain tolerance (to the extent that I possess them) can be traced in large measure to this course."

Increased awareness of the history of ideas and of historical perspective, an appreciation of the scientific method in other fields than just in the laboratory, an integration and correlation of the fields of knowledge beyond the somewhat artificial barriers of college departments—these are not all the values of such a course, though they are of marked significance to these women. The large majority count themselves enriched by acquaintance in their setting with great men whose names they had hardly been aware of before and whose ideas, some of them two thousand years old, startle them by their modernity. A former major in psychology was amazed to discover for herself that Empedocles, for instance, had contributed to her field; and all are impressed by the scientific objectivity of the Surgical Papyrus, the history of which reaches back into the third millennium before Christ. The medieval period came alive to a brilliant English student who wrote that before it had always seemed empty of interest to her. Most of all, they emphasize that it is science as a continuing activity that impresses them. One man drops defeated, but sooner or later another picks up his idea and carries it on.

The influence of the course does not stop when the classes are ended. For a few it has led to the election of more courses in science even in those students' senior year. A graduate student at Yale writes of her enjoyment as an auditor in a seminar on the history of medicine, another that for 15 years now astronomy has been her hobby. For many it has led to the reading of books they otherwise would not have been interested in. Teachers and librarians have made frequent use of the bibliographies and the outlines provided, to judge by their comments. A doctor's secretary reads for her own pleasure the historical material advertisers send her employer, and many speak of their interest in recent historical books and articles about science. As a stimulus to further reading in the field the course has contributed its share.

Incidentally, the course has also had a social value for these young women quite unexpected by the instructor. They write that it has contributed to dinner-table con-

versations, USO gatherings, and even to greater companionship in marriage because they had a more understanding appreciation of the scientific work being discussed than they otherwise would have had. One young woman remarked that she had elected the course because she was engaged to a medical student, that she knew nothing whatever about his field, and she thought that reading about the history of medicine would give her more comprehension of, and a better preparation for, her future life as a doctor's wife. Others have written of going with their husbands to scientific meetings and of hearing one scientist belittle the social studies and another point out the need for historical training for scientific people. One who graduated twenty years ago writes: "From my long acquaintance with scientists, I think historians are more appreciative of scientists than scientists are of history." And husbands are quoted as having wished that they had had such a course in their undergraduate days.

Some young women, fired by the subject, have asked about the possibility of making a career for themselves in the history of science. One, a chemistry major while in college, has done so and is now managing editor of a journal on the history of medicine while at the same time working in the history of American medicine. Others have started graduate work in the history of medicine until marriage carried them away to other activities. For most the answer to the question of the career has had to be: Keep it as your interest, your hobby; you cannot earn your living on graduation by work specifically in this field as yet. Recently, however, a possible opening has appeared through the developing work of the museums of trade, industry, and science and their desire to secure young people who can explain exhibits to school children and who can write about them accurately and directly for publication. This may well prove to be a hopeful opening for more than the one or two who have found their way into it already.

At best, a course like this in the history of the scientific point of view can only be the opening of a door and a glimpse of the long vistas of rooms and galleries stretching on endlessly, waiting to be explored. As an introduction it provides a frame of reference. Logically, it should be followed forthwith by a detailed history of the biological or of the physical sciences, according to the student's interest—a detailed and technical history that would place the emphasis on modern developments in those sciences and would therefore have to be taught by historically-minded scientists, experts in those fields. Modern developments are far too technical as well as too numerous to be crowded into an already overcrowded survey, and they need to be taught by one who can speak with authority in the science itself. Such an arrangement was in operation at the University of London in 1930. Professor Wolf planned for his graduate students in the history of science to meet together for the first half of the year to survey the historical development of science down to 1800 or thereabouts. Then the class divided according to its preference, and during the second half of the year specialists in the two major divisions of science traced modern developments in their respective fields.

In an undergraduate college, as at Goucher, the same plan can be carried out on a small scale. A professor of chemistry has led his senior students in a history of chemistry. A professor of physiology has done the same in her field for her seniors, and for years a professor of biology has been dealing with theories of evolution and an astronomer with descriptive astronomy, set in their respective historical backgrounds.

Science majors in particular, of course, express the desire for more recent and more

technical material, but in the same breath they add that there would not be time for it, and even for them, as one expressed it, the course had made "lab work real."

Should such a survey be taught by a historian or by a scientist? Almost unanimously the science majors and the non-science ones chorus: By a historian. They prefer the different point of view, which they characterize as a broader one, freer from the technicalities of the sciences. A few said it did not matter as long as the person was a good teacher. But most stated they feared the stress a scientist might give to his particular interest as a possible restriction upon the historical perspective and the breadth of view which the historian of ideas can use in relating science as a whole with history, philosophy, literature, and religion. Technical details they can get elsewhere. What they appreciate is seeing the science of their choice correlated with other sciences and brought into relation with the general history of civilization. Parallels between the historical method and the scientific method bring greater appreciation of the distinctive values of both and of their importance in society today.

An older graduate writes that this was

> the only course pertaining to the sciences in my whole college career [and she had had to take a number] that I did not feel was being given in a language completely foreign to me; it gave me sufficient confidence in my understanding of the scientific method that I attained my M.A. from professors who lay stress on sociology being a science, and I can be of some help to my husband, a chemical engineer, because I can follow his line of professional thought, at least in respect to principles and generalizations.

What, in summary, do these comments and others like them indicate? What virtues has a history of the scientific point of view for women whether they are specializing in science or in something else? Only one of the 64 percent who answered said it had been of little or no use to her. For most it has made the era in which they live richer, more understandable, more alive, less provincial, and more dynamic. It has helped to break down artificial barriers and to unify science with other aspects of knowledge. Science for them is less esoteric, more significant, because they know something of the human struggles and failures attending its development, as well as the successes. Science for them is no longer chemistry or physics or mathematics, of concern only to those who have a special taste for laboratory work. It has become a significant part of knowledge as a whole, the influence of which permeates modern life and thought more thoroughly than even the science majors themselves had realized. They have discovered that, while great scientific discoveries have spurred on further scientific advances, they have also affected philosophy, literature, religion, and the course of history itself as well as being in their turn fundamentally affected by these same forces. And in their own thinking they have gained an appreciation of the scientist's methods of work till they no longer smile at seeing as a definition of science, "the process which makes knowledge." They, too, have had a glimpse of the meaning of the passionate search for truth.

The historical approach to science richly rewards its students, whether they are laboratory workers or not. Scientists would do well to welcome into the education of all students, not just of those in their own fields, courses similar to the one reported on in this paper; for the advancement of science even in this scientific age depends in large measure on the popular understanding of its principles and its methods. Popular support can help; popular prejudice can also cripple. Scientists need not fear the lack of expertness in their own particular science by the well-trained historian or

philosopher who attempts such a course. Students are shrewd. They discriminate between technical matters, about which they themselves may perhaps enlighten their instructor, and the breadth of outlook, the different point of view, the fresh presentation of names familiar in science in their historical setting as part of a culture and of an age hitherto not considered of importance to them in the mid-twentieth century. They can and do turn to their science professors for further technical enlightenment, which is as it should be; but those same professors, specialists in their various sciences and absorbed in contemporary developments, oftentimes in that absorption fail to realize how abstruse to the student, how difficult, and how complex their subject is. Yet that subject when viewed historically takes on new interest and reality when one has some idea of its dramatic history of failure and success, its interrelations with the ages in which its growth has been favored or checked, its incorporation into itself of the ideas of men of all ages without regard to their nationality or race. From such a study the student returns to her laboratory with a new appreciation of the complex processes confronting her, for she has had a glimpse of their long evolution. For her they have become *real*.

More than ever, civilization today needs not only the experts but a general public trained to appreciate scientific methods and the scientific approach to truth. People need to apply such methods and thinking to their own living and to value at its true worth the scientist's passion for truth. Teachers, librarians, mothers of families, writers, social workers—all testify to the values these ideas have had for them and to the stimulus to their intellectual life that the history of science provided. Would that all college faculties provided such courses.

The preservation of human liberty is paramount in world society in our time. The struggle to achieve and maintain it, viewed historically through the ages by following one major aspect of it, the development of scientific thought, brings reality and unity to a student's thought and work. For this struggle transcends all boundary lines, whether in the curriculum or on a map, and gives background for the comprehension of some, at least, of the factors comprising that paramount issue, human liberty.

VARIED APPROACHES TO NATURE

By LEWIS G. WESTGATE
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To him who in the love of Nature holds
Communion with her visible forms, she speaks
A various language.

NATURE'S message is an individual one: it is not the same to Darwin, John Muir, Winslow Homer, or Wordsworth. The various approaches to Nature find their justification in meeting essential human needs. They are not to be arranged in a hierarchy of ascending values; each has its own peculiar service to render. The scientist cannot say to the artist, "I have no need of thee;" or, again, the mystic to the scientist, "I have no need of thee." What follows is an attempt to point out the human values in the differing approaches of the scientist, the nature lover, the artist, and the poet, or mystic.

THE approach of the scientist is primarily intellectual. Its motive is curiosity. The child's inquiry, How is it made or how does it go? is still valid for the man of science. Nature is multitudinous, chaotic; the scientist is concerned with the order behind the apparent disorder. He observes, classifies, experiments—and meditates. In the end he has knowledge.

The great growth of science has taken place since 1500. In that time it has given us a new world and a new method and spirit.

This new world is radically different from the tight little world of the Middle Ages. Astronomy today looks out into infinite space, star on star, galaxy on galaxy. The earth is but a minor planet circling about an ordinary star, our sun, which is off-center in one of millions of known nebulae. The geologist looks back on an earth history of some 2,000 million years and counts on a future measured in millions. More important, the biologist views man as an animal among animals, as sprung from Nature and an integral part of Nature. For the first time man sees himself in perspective, gets a sense of his place in the totality of things, and is able to judge his present tendencies and future prospects. He finds himself a part of a great ongoing process, each stage of which is the culmination of all that has gone before and contains the seeds of all that is to come later. The key word is evolution.

In this new world the scientist increasingly finds order behind multiplicity and confusion. Early man peopled his world with spirits, mostly hostile, and lived in constant fear. In the thinking of the scientist there is no room for the intrusion of outside agencies into Nature. Neither devil nor angel has a look-in. Everything has its cause. When that is once understood fear goes, and superstition. If we can learn Nature's ways and are willing to adjust ourselves to them, we find her dependable, our friend.

The great contribution of science to the thought of the present is not so much its new knowledge, important as that is, but its method and spirit. Its method is, in fact, nothing other than that which we use every day in finding a lost cat or what is wrong with a stalled automobile. The procedure is simple: There is a problem, a proposed solution or hypothesis, and verification, which gives us knowledge. The difference lies in degree; in the extreme care which the scientist takes to avoid all error, inexactness, and personal bias. It is the very opposite of wishful thinking, which is wishing, not thinking. This procedure is

the only way we have of getting knowledge, is applicable in all fields where knowledge is possible, and the area in which it is applied is being steadily widened. It is this respect for fact, this spirit of unbiased devotion to truth, that is the great contribution of science to intellectual and moral life.

The world of modern science is quite unlike the world which the common man thinks he sees. It sometimes seems that the scientist's approach to Nature is a departure from Nature. The different sciences get away from daily experience in varying degrees, physics furthest. The so-called observational sciences—botany, zoology, geology, anthropology—still maintain touch wi h the common world. But for the physicist, it seems that the nearer he gets to "reality," the further he gets from actuality. From moving bodies he abstracts mass and motion and with these, from Galileo through Newton to the present day, he has been building up a splendid mechanical system which ties together sun, planet, and falling apple and is found to stretch out through infinite space. But it leaves out those qualities—color, form, etc.—which are of more interest to most of us as the source of immediate enjoyment. Subatomic physics carries us still further from everyday appearance. "The external world of physics has become a world of shadows," says Eddington. "One has to go out and knock on wood or rock to assure himself that there is anything to this solid earth." Let us be thankful that he leaves us the wood and rock to knock on! Shakespeare may have foreseen modern physics when he made Prospero say:

The cloud-capped towers, the gorgeous palaces,
The solemn temples, the great globe itself,
Yea, all which we inherit, shall dissolve,
And, like this insubstantial pageant faded,
Leave not a wrack behind.

Every dog has his day, and today in science is the day of mathematical physics. Yet, after all, the acrobatics of protons and electrons are not what most concern us. To leave them for the ordinary face of Nature is like coming out of the damp darkness of a mine shaft into the warm sunshine. It is likely that we are too much enslaved to the mathematical sciences, that it is the sciences of life which will have most to tell us; that chemistry and physics are only preliminary to biology. They have flourished earlier because their problems are simpler and easier.

THE nature lover's approach is direct; sensuous and emotional rather than severely intellectual. He is concerned with form and color, with touch, sound, and smell. He delights in the sheer breath of an October afternoon, in the odors of the pines and of the rain-soaked forest floor. His ear is attuned to the roar of the surf or to the gentle murmur of the wind through the trees. His eye glories in the blue of the sea or sky or distant mountains and does not ignore the shifting grays of the overcast heaven. It is the beauty of Nature and its influence on the human spirit that draws the nature lover, not mere knowledge; certainly not any use he can make of it in ordinary everyday affairs. Nature's beauty is primary, a free gift to man, not to be explained by any philosophy, but accepted with filial piety.

Byron felt it when he wrote:

There is a pleasure in the pathless woods,
There is a rapture on the lonely shore,
There is society, where none intrudes,
By the deep Sea, and music in its roar.

Wordsworth, writing of his boyhood experience of Nature, has expressed the point of view with finality:

For Nature then
To me was all in all.
The sounding cataract
Haunted me like a passion the tall rock,
The mountain, and the deep and gloomy wood,
Their colors and their forms, were then to me
An appetite; a feeling and a love,

That had no need of a remoter charm,
By thought supplied, nor any interest
Unborrowed from the eye.

No better incarnation of the spirit of the lover of Nature can be found than in the American Muir or the English Hudson. Muir, a Wisconsin farmer, Scotch-born and university-educated, finally arrived in California at the age of thirty. From then on he was Muir of the high Sierras. For weeks at a time he tramped the high mountain country, alone, with the lightest equipment, never armed. Hunting was for him "the murder business," the very opposite of the sympathetic love for, and appreciation of, animal life. His primary interest was in plant life, especially the trees, and in the glaciers and their former courses; but he cared for every aspect of outdoor life: birds, streams, storms, the Indians, everything except civilized man. It would be pleasant to quote extensively from Muir's writings; two passages must suffice. The first is from *The Mountains of California*:

> I chose a camping ground on the brink of one of the lakes where a thicket of Hemlock Spruce sheltered me from the night wind. Then after making a tin-cupful of tea, I sat by my campfire reflecting on the grandeur and significance of the glacial records I had seen. As the night advanced the mighty rock walls of my mountain mansion seemed to come nearer, while the starry sky in glorious brightness stretched across like a ceiling from wall to wall, and fitted closely down into all the spiky irregularities of the summits. Then, after a long fire-side rest and a glance at my note-book, I cut a few leafy branches for a bed, and fell into the clear death-like sleep of the tired mountaineer.

The second is from a letter to a tired and conventionally pious schoolteacher:

> Do not, I pray you, destroy your health. The Lord understands his business and has plenty of tools, and does not require overexertion of any kind.
>
> I wish you could come here and rest a year in the simple unmingled love fountains of God. You would return with fresh truth gathered and absorbed from pines and waters and deep singing winds You say that good men are "nearer to the heart of God than are woods and fields, rocks and waters" Such distinctions and measurements seem strange to me. Rocks and waters, etc., are words of God and so are men. We all flow from one fountain soul

Muir was to a degree a mystic, as I take it all lovers are. A reviewer of the recently published journals of Muir writes: "The philosophical naturalists of today have drifted away from the course John Muir was following. . . . John Muir is out of step with the times, or perhaps the times are out of step with John Muir." To be out of step with the present times may be an asset and not a liability. It may mean only that Muir is closer in step with the Eternal. Those who have stood hushed and alone at sundown will understand.

The nature lover's approach to Nature appeals to more of us than does any other. It requires less specialized and technical preparation than that of the scientist. It is neither necessary nor advisable to move in on all Nature. Many doors open to her domain: the love of birds, flowers, and gardening; of forest, desert, and mountain; of sky and cloud. Some answer one call, some another. None are excluded. But to get on intimate terms with Nature, to get the highest pleasure from that intimacy, is a long, happy, and enduring quest. Like human friendship, it must be won. It means being with Nature and studying her ways through the years. It is an experience open to all; no barrier of race, religion, or age intervenes. The sky, the fields, the mountains, the human face, are there for rich and poor alike.

THE painter is the see-er, he whose trained eye is sensitive to the forms and colors, lights and darks, of Nature. If we limit the discussion to landscape painting, it is for convenience only; what is true of the open country is equally true of man and the

man-shaped aspects of Nature. The play of light and color on city streets, in our homes, on human face and hair, we miss as easily as, perhaps more easily than, the features of the out-of-doors. Just as the ear of the trained musician responds to slight differences that the ear of the untrained man does not distinguish, so the painter through his long training notices form, color, and meaning that are hidden from those who have not attended to such things. It is the function of the artist to catch this and to express it for the rest of us.

Here is a marine painting by Waugh, the Provincetown artist who died in 1941. It has sea, sky, cloud, and rocky shore; wind and light and color. A strong, cold, onshore wind is driving in immense, deep-blue, white-capped waves, which break in a wild whirl of opalescent water and spray on the granite ledges in the foreground. The evening sun reddens cloud, spray, and rock. No human figure, no work of man, shows. Waugh has caught the very life and strength of the ocean. He has caught something more; the spirit of eternity, of the never-ending clash of the elemental forces of Nature. So it has been since God said, "Let the waters under the heavens be gathered unto one place, and let dry land appear." Throughout the earth's history, for some two billion years, with no human eye to see it, the slanting sunlight evening by evening has touched to redness similar scenes along all shores. It may be that after man's course is run and all evidence of his occupancy has disappeared that same scene will be enacted about all the borders of the continents. Ocean—sky—land—these are the signs of infinity. Waugh has caught not only the spirit of the ocean, he has caught its eternity.

Waugh writes of his own work:

> I spend part of each year studying the sea. I both paint it and watch it carefully, and the latter method of studying I am sure is invaluable. In that way I fix certain forms clearly in my memory and learn the why and the how of the grand old ocean . . . If you really love Nature she will love and teach you.

As to the aims of his art, the painter may be allowed to speak for himself:

> The chief mission of the artist is to reveal that portion of nature's riches which he has discovered, to those who would not otherwise have suspected their presence He serves as a translator and interpreter of nature to those who cannot understand her language (Millet).

> If we can give a man a canvas that will take him away from his desk and lead him into the field and make him feel what we feel in the presence of beauty, we have done something good In our art this is what we strive for (Innes)

> The Louvre is a good book to consult, but it is only an intermediary The diversity of the scene of nature is the real prodigious study to be undertaken . . The Louvre is a book where we can learn to read But we should not be content to keep to the formulae of our illustrious predecessors Let us leave them so as to study beautiful Nature and search to express it, according to our personal temperament Time and reflection greatly modify vision, and at last comprehension comes (Cezanne)

To see and, seeing, to feel—for the approach of the artist is primarily emotional and not intellectual—and to express that which in turn will enable others to see and feel is the function of the painter.

It should go without saying, though it does not, that he who as artist pretends to reveal Nature to us through the medium of painting should master his craft. A stammering utterance does not help to get any message across, whether in prose, poetry, or painting. Nor are we in this connection concerned with the need of any cooped-up artist to relieve himself of his boiling emotions or passions, that is self-revelation and not interpretation of Nature. Some of the vagaries of recent art may be of interest as studies in psychology, perhaps abnormal psychology, but are of little help to one who wants a clearer insight into Nature.

THERE is, finally, the mystic's approach to Nature. Nature is alive; we can hold communion with her. Wordsworth's poetry is the best-known expression of this belief, especially the "Prelude" and the "Lines on Tintern Abbey." In the latter he writes:

I have learned
To look on nature, not as in the hour
Of thoughtless youth but hearing oftentimes
The still sad music of humanity
And I have felt
A presence that disturbs me with the joy
Of elevated thoughts, a sense sublime
Of something far more deeply interfused,
Whose dwelling is the light of setting suns,
And the round ocean, and the living air,
And the blue sky, and in the mind of man,
A motion and a spirit that impels
All thinking things, all objects of all thought,
And runs through all things

Others will not have it this way. John C. Van Dyke writes:

> Nor is nature, as some would have us think, a sympathetic friend of mankind, endowed with semi-human emotions Mountains do not "frown," trees do not "weep," nor do skies "smile" Indeed, as far as any sympathy with mankind is concerned, "the last of thy brothers might vanish from the face of the earth, and not a needle of the pine branches would perish" (Van Dyke, John C *Nature for its Own Sake.* Scribners)

Why may not both be right, each possessing a partial insight into the doings of mysterious Nature? The quiet of a summer afternoon may speak peace, yet at that moment on another continent great armies may be at grips and men be slaughtered by the hundred thousand. Apparently Nature does not care, God does not care. Indeed this apparent indifference makes difficult any belief in a God who is concerned with the fall of a sparrow. In the desert one knows that missing the water hole means death. If the traveler on the glacier slips into a crevasse, the ice will close on him remorselessly. Van Dyke seems to be right.

Yet that is not the whole truth. The late Professor Winchester, discussing this very matter of Wordsworth's mysticism, puts it as follows:

> We all know that the common face of nature may sometimes have an effect upon our emotions like a soothing word or a noble deed When Wordsworth says of a distant mountain peak that it sends
>
> Its own deep quiet to restore our hearts,
>
> or bids the river
>
> Glide, fair stream, forever glide,
> Thy quiet soul on all bestowing,
>
> it is not a fancy that he utters, but a fact How it may be, perhaps we cannot tell; but the fact is undeniable And more than that we know In our moments of truest intuition we are sure that the fact of that influence implies some deep affinities of being. It is absurd to say that quartz can generate quietude of soul or that H_2O can calm the mind What is it but spirit that can stir the spirit within us, that can suggest deep meanings to our intelligence or inspire to lofty and tender emotion? This belief may not admit of any very explicit statement in terms, because, like all our deepest convictions, it is half emotion, but it is a belief in which philosophy, poetry and religion are at one And if you question its warrant you will at least find no better evidence for all our most profound convictions (Winchester, C T *Wordsworth, How to Know Him* Bobbs-Merrill).

The validity of the mystical attitude toward Nature is not to be settled by argument. It is a matter of individual experience, where all the evidence is from within. It is partly a matter of temperament. In any one person it varies with differing mood and circumstance. In the quiet of evening, as the sun sinks to rest through golden bars of cloud, a strange and indescribable feeling of confidence in the beauty and goodness of the world comes over one. Is Nature, is God, speaking? Each must decide for himself. He who can may enter by this door the holy of holies of Nature.

The different approaches to Nature are not mutually exclusive; individual men can and do come to Nature by more than one

route. The approach of the scientist is not solely intellectual. He may feel intensely the beauty of the order of swinging planets, of the marvelous adaptations within the bodies of organisms, and of the organisms themselves to their environment; and he is usually, as was Darwin, far ahead of the average cultured man in his appreciation of scenery. Again, the painter and the lover of Nature will see more if they avail themselves of at least the elements of some of the sciences. An artist is expected to base his painting of the human face and figure on an accurate knowledge of anatomy. If he is a landscape painter he cannot appreciate scenery to the full extent without an understanding of the rock formations and of the forces which have carved them into their present shape. Muir did not appreciate the high Sierras less because he was a close student of their forests and glaciers. Nature is a world of color, of light and shade, of light broken, reflected, refracted, echoing endlessly back and forth. One sees more in the cloud-flecked sky of midday or in the banded colors of sunset if he has some knowledge of the physics of light. One does not have access to Nature if he comes with an empty mind. The more one brings, the more he will get.

Each of the approaches we have been considering satisfies definite human interests and needs. They are a free gift to men; and like good literature, friendship, and religion, which are also free gifts, they come only to those who meet certain conditions. They are not to be passively accepted, but actively won.

PROSPECT AND RETROSPECT: A DREAM OF MAN

By Almon Barbour

Back to primeval beginnings
When the earth was a blazing fragment
And the noonday sun was brighter;
Back when the ice moved down from the poles—
I saw myself one with man from the beginning,
Seeing him, knowing all his ways
With animals, plants, stones, stars.

Down the forgotten ages, I saw myself
Tracing his strange and bloody ways:
In the hanging bough in the forest,
Upon the rain-drenched cliffside,
Across the far-lying desert;
Tracing his strange and bloody ways
In every corner, in all the far places,
In all the long ages since the beginning.

One with him down the forgotten years,
Knowing his ways, his songs, his poems,
Thinking his thoughts of God and Evil,
Knowing his dreams of heaven and hell;
One with him, knowing his ancient loves,
Adoring his painted saints, his idols,
Feeling perennial fears and lusts;
Knowing the beat of his heart
Through the generations pulsing
Down all the years from the beginning.

Then I saw myself, an apprentice,
Go with him into the endless seasons;
I saw all man upon his star
Go marching into the endless years.
One with him from the beginning
And now and forever, I heard at last
His voice, trembling with hope and fear,
Interrogate the sphinx:
O ancient earth,
O animal of long hope—do we
With pain of laboring love go marching
Into the hills, or are we lost
Upon this widening, endless plain?
Do we labor now toward the light,
Or do we spin, in vain,
Wild circles in this deepening night?

ON LIFE AS A SEPARATE ENTITY

By THOMSON KING
Baltimore, Maryland

EVER since man has been able to think of matters beyond his purely physical needs and to express and record his thoughts, he has reasoned, speculated, talked, written, and debated with his fellows about the nature of life. There have been many theories and beliefs, but practically all of them can be assigned to the one or the other of two general schools which we will call "Mechanistic" and "Vitalistic." As our knowledge of ourselves and our environment has increased and become more accurate, the outlines of the two great divisions have become more clearly defined and understood. It is doubtful, however, whether we are closer to a generally accepted decision as to which is the true, or even the more probable, answer to one of our most interesting questions.

My object, and I hope justification, for the following elementary discussion of a well-worn subject is a form of approach which may be new, and perhaps interesting, to some readers. This will consist of avoiding religious and metaphysical concepts and confining evidence and argument to a simple but fundamental question, the answer to which may, I believe, be found in our present knowledge and our capacity to interpret that knowledge. The one important question will be presented in its simplest and most general terms, in such manner and form that it can be considered and judged very much as any question about the nature of the physical world is considered and decided.

We can hardly hope to demonstrate a rigorous proof of a broad general question in the same sense as a simple geometrical theorem. Perhaps if we state two theorems, one of which must contain the correct answer to our question, and then examine and weigh the evidence afforded by the present state of our knowledge, the evidence will indicate, and our reason will accept, a clear-cut decision as to which of the two opposed theorems has the greater probability of being true.

The word "probability" is not to be taken lightly. Most of the decisions and beliefs of intelligent people are based upon probability. If I were asked to state something of which I am absolutely certain, I should say, "I am sure that the next ten bridge hands dealt to me will not each contain thirteen spades." Yet I cannot prove it will not happen; my certainty is based on probability. Many of the laws of physics and nearly all of thermodynamics are of this nature; they are statistical, based entirely upon probability; their acceptance implies the assumption that excessively improbable events have not and will not occur. The same ideas and principles apply in biology. Few things can be proved beyond the shadow of a doubt, and indeed it is not necessary that they should be.

The mechanists hold that all phenomena associated with life can be entirely accounted for and explained in terms of matter and energy, and nothing else. They believe that at some time in the distant past, conditions were such that certain chemical and physical processes started, exactly as inorganic reactions are brought about, by combinations of matter under suitable conditions. The continued reaction of matter and energy, and nothing more, has produced, developed, and sustained all the activity and phenomena that are associated with life. Mechanists never admit that anything more is necessary to begin life in a

lifeless world or to maintain it in all its unique and various manifestations when once started.

The vitalists believe that life is something entirely separate, distinct, and different from matter and energy. It is always associated with and observed by means of matter and energy. We cannot observe it except in combination with them, just as we can observe matter only by means of energy, but the vitalists hold that its unique development and phenomena are due to matter and energy plus something else that is neither matter nor energy, nor a combination of the two, but a distinct and separate entity. Some vitalists have identified life with the soul or with consciousness. Certain philosophers have believed the latter to be the only reality—the *cogito ergo sum* of Descartes. All religions have been founded by vitalists, most of them have taught that something about life was immortal. They usually conceded a beginning but denied an end.

We can best reach our objective of the simplest and most general question as to the nature of life and the most probable answer by refraining from further talk of mechanists and vitalists. I will attempt to be scientific in method without being technical in language. Above all, I will endeavor to avoid what Hogben calls "the pitiful failure of introspective philosophy which resides in the finality of its answers." This can best be done by keeping clear of all religious and metaphysical concepts and arguments, which have too often led to vague, barren, and undisciplined discussion. I do not believe they are necessary and I know they are dangerous when it is desired to confine attention to one clear-cut, simple question. We will limit ourselves to the evidence of our senses and our instruments, which are always extensions, refinements, or amplifications of our senses. We will try to interpret that evidence in the light of reason and probability.

Before stating the question, it is necessary to say what I mean by several of the terms I must use. Definitions will be brief, for we are dealing with fundamental concepts and we know that true fundamentals can only be defined in terms of themselves.

By "entity" we mean something having reality in fact, which is separate and unique in itself. It may have many parts, qualities, and aspects, but it is not a part, quality, or aspect of anything else, except for the possibility explained in the next paragraph.

"Matter" and "energy" have their usual scientific meanings. It is to be clearly understood that under certain conditions they are mutually convertible. Einstein has shown that one gram of matter is the equivalent of C^2 ergs of energy, where C is the velocity of light in centimeters per second. We now believe the sum total of the matter and energy in the universe is and, as far as we can know, always has been a constant. Possibly they are two forms of one greater entity, but with the present state of our knowledge it will be simpler and more convenient to regard them as separate entities.

By "universe" we mean the full extent of what we call space and everything contained therein. It is a four-dimensional continuum: it has extension in three spatial directions and in time. From any given point we have an east-west, a north-south, an up-down, and a before-and-after. For the purpose of our discussion, it is immaterial whether the universe is finite or infinite in time and space, for whatever it may be we are going to stay inside.

The expression "in the universe" is meant to exclude from our discussion anything capable of existing outside the universe. A creator obviously must have existed before and outside of anything he created, and any question of creation is irrelevant and unnecessary to the discussion of our question.

I can now state my question in the form of a brief theorem, or hypothesis, and what seems to me to be its only possible alternate:

A. There are two fundamental entities in the universe: *matter* and *energy*. All phenomena are due to these two entities, separately or in combination.

B. There are three fundamental entities in the universe: *matter*, *energy*, and *life*. All phenomena are due to these three entities, separately or in combination.

Before considering the evidence for A and B, we must first be sure that these are the only hypotheses that can be made. Someone may say magnetism is a unique phenomenon of certain states of matter, and gravitation a phenomenon of all matter, and propose them as fourth and fifth entities. I think we now know enough to see that the theory will not hold water or even hold together.

There is a great deal we do not know about gravitation and magnetism, particularly as to how they act at a distance; but we do know essential facts that I think will dispose of the question. All our evidence indicates that gravitation and magnetism are properties that matter has always possessed. They did not appear at a particular era in the earth's history nor, so far as we know, have they changed or developed. Since life appeared upon this planet and began to leave its record in the rocks, its forms and phenomena have changed greatly. Indeed change, growth, and development are among the most important characteristics that have led us to consider whether life is or is not a separate entity. We can account for and explain the phenomena of gravitation and magnetism in terms of matter and energy and nothing else. I think we cannot escape the conclusion that they are properties of the entity matter.

I cannot think of any other phenomenon that can show a better claim to being due to a fourth entity. I must therefore believe the two hypotheses cover the entire field. What A asserts is denied by B. It is obvious they cannot both be true; therefore, one or the other contains the truth about life. It cannot tell us anything of the *whys*, *wheres*, *whens*, or *hows* we should like to know; it does give a general but positive answer to the first great *what*.

The only other objection to our hypotheses would seem to be the contention that matter and energy, and perhaps life, are but aspects or forms of one all-embracing entity. This indeed may be possible, but it is beyond the limits of this discussion and probably beyond the grasp of our present knowledge. By definition we are to consider matter and energy as separate entities, and then to attempt to decide whether life is also a separate and unique entity in the same sense.

I think it is self-evident that the behavior of matter, energy, and living things is governed by natural laws. Whether we believe in hypothesis A or hypothesis B we certainly do not believe in unnatural laws. The word "supernatural" also seems to me to be utterly meaningless except as a measure of our ignorance. All events in the universe are natural, and they are governed by laws. Events are no less natural because we do not understand the laws that govern them.

In weighing the evidence for and against hypotheses A and B, we must proceed exactly as we would in deciding any other question with regard to the nature of the physical world. The study of life by scientific methods is quite young, and there is much that we do not understand. But we may be sure that our best chance of increasing our understanding is by a rigorous application of the scientific method that has given us all our present knowledge of the universe and the laws that govern it: observation, experiment, organization of data, and the application of reason to the data. We seek knowledge, and the field of science

and scientific method is coextensive with the field of knowledge.

We believe a brick has certain qualities because of the direct evidence of our senses of sight and touch. We believe in gravitation because, though we cannot see it, we can see and feel its effects. We believe in electrons and radio waves because of the evidence of our instruments. We are sure X-rays are of the same nature as light but of shorter wave length because the evidence convinces us that this hypothesis is more probable than the alternative statement, that they are not of the same nature. We believe that oxygen and iron are unique elements rather than the alternative hypothesis, that they are not unique but only combinations of other elements, because the evidence of our senses, supplemented by our instruments, when considered by our reason indicates that no combination of other elements can produce the qualities and effects that are associated with oxygen and iron.

If it can be shown that our senses observe and our reason accepts phenomena that cannot be explained by any known property of energy or matter, or by any imaginable combination of the two, we must assume and accept the existence of a third entity in the universe. Hypothesis A falls and B remains. If this cannot be done, A stands and B can be forgotten.

THE EVIDENCE

The properties of matter are inertial, gravitational, electric, magnetic, chemical, and a general group that we shall call physical (shape, hardness, density, etc.). Those of energy are inertial, gravitational, and the capacity for doing work, producing change, vibration, frequency, and movement.

The phenomena of life are growth according to a predetermined complex plan, metabolism, reproduction, evolution of species, a limited time of occupancy in a particular body, inherited habit or instinct, a tendency to variation between generations, and, in the higher forms, consciousness, memory, emotion, reason, and other phenomena of the mind.

It is to the phenomena of living things that we must look for evidence to confirm or refute hypothesis A. Some of the phenomena I have listed are so closely associated that they cannot be discussed separately or in strict sequence. It is best to begin by recognizing and stating one rather obvious but essential fact.

So far as I know, with one possible exception, all phenomena of life are associated with matter. The bodies of all living things are composed of matter, and they live by employing energy. It seems to be characteristic of life to appropriate, use, wear out, and abandon aggregations of matter. Since our senses depend entirely upon matter and energy for receiving sensations and information with regard to the physical world, they can tell us only of phenomena involving matter and energy. If more than matter and energy are necessary to explain any phenomena, our sense cannot directly inform us of the fact; it must be deduced by study and reason from the evidence of our senses or instruments.

The facts just cited have always exerted a profound influence on all discussion of the subject. For many investigators, they have seemed sufficient proof that life must be a rather specialized and peculiar activity of matter and energy and nothing more. Today we can see much further into the problem than a few years ago. In the light of present knowledge, I do not think the fact that the phenomena of life are associated with and observed by means of matter and energy proves anything one way or the other. Until a comparatively short time ago, water was thought to be a single, simple substance. The belief was founded upon lack of knowledge and power of analysis, chemical and mental. For the same reasons men once thought the earth was

flat. With the benefit of more evidence from observation and closer reasoning we now believe it to be an oblate spheroid.

I am aware of and respect the opinion of many eminent biologists, probably a majority of all biologists, that is epitomized in the following quotation:

> There seems to be no escape from the conclusion that the vital properties of organisms are due to the condition in which their material exists for the time, and that they are not the manifestations of the presence and action of a separate entity in the way of a so-called vital principle.

This is a very positive statement of belief in our hypothesis A, but I find it less than convincing. To accept it, I must believe that all the phenomena of life are due to the condition and arrangement of the substance called protoplasm. Protoplasm is indeed a complex and remarkable material made up of a number of the more plentiful elements, but I think the biologists are placing on it more than it can bear. Its condition and arrangement must account for the fact that one tiny cell carries the plan and implements the life cycle of an oak tree, another of a mosquito; it must cause the oyster to build his limestone shell, the bird to inherit the ability to build a nest; it must account for metabolism, which is not found in any other arrangement of matter; finally, it must account for thoughts and all the multitudinous activities of the mind.

The scope of this discussion does not permit any lengthy excursion into biology, but I must cite two more quotations from biologists which seem to me very significant and enlightening. "There is no such thing as dead protoplasm." Do we not have here acknowledgment of something that is not to be found in matter and energy? Biology seems to say there can be no life without protoplasm and no protoplasm without life—the old chicken-and-egg dilemma. But if the activities of protoplasm are due to matter and energy only, what does the statement that there is no such thing as dead protoplasm mean? Why can we not have protoplasm without life? To me, it seems to imply that the origin and activity of living things are due to a combination of matter and energy with, or under the direction of, something else.

The distinction between living and nonliving aggregations of matter is of an entirely different order than that which separates organic and inorganic. So far as I know, all the phenomena of nonliving organic bodies can be accounted for by hypothesis A. We need no special laws or assumptions to explain their behavior with regard to entropy or anything else. So long as I am alive, the laws that are adequate for nonliving bodies fail to predict or explain all my bodily, and to a greater degree my mental, phenomena. As soon as I am dead, although the atoms of my body are the same, no particular or special concepts are necessary—simple organic chemistry will predict and explain all that can happen to that curious aggregation of organic compounds. The laws peculiar to living things are additive, not contradictory, to laws of matter and energy.

Other biologists, in particular the geneticists, are somewhat more specific and specialized in their opinions. They say that it is only necessary to postulate the assembly and combination of material in the primeval sea to produce an autocatalytic substance with a tendency to mutation, to account for the origin and development of life. A typical statement is: "These considerations lead us to regard the gene as the essential basis of life: a gene, or gene substance—i.e., a mutable, specifically autocatalytic substance—contains the potentiality of all the forms and phenomena of life." In this sweeping assertion "gene substance" has replaced the more general protoplasm as the essence and explanation of life. But is the explanation valid? Is it based on facts or on faith? We know that genes are the

carriers and determinants of heredity, that they are found in all living cells. The mechanists are endowing the matter of which they are composed with qualities and potentialities that, so far as I can see, are not indicated by the known properties of nonliving matter. Even the words "mutable" and "autocatalytic" cannot confer upon matter alone the ability to reproduce successive generations of organisms of a predetermined and complex pattern with inherited instincts and abilities.

The eminent physicist and mathematician Erwin Schrödinger says:

> The arrangement of atoms in the most vital parts of an organism and the interplay of these arrangements differ in a fundamental way from all those arrangements of atoms which physicists and chemists have hitherto made the object of their experiments and theoretical research, . . . Whether we find it astonishing or whether we find it plausible that a small but highly organized group of atoms be capable of acting in this manner, the situation is unprecedented It is unknown anywhere else except in living things

He also finds the all-important characteristic of life is its ability to feed upon what he calls "negative entropy." For those who understand the meaning of entropy this is interesting. By these statements Schrödinger does not mean that life processes are not subject to physical laws. What he does mean and states clearly is that they are unique in requiring different and additional laws to interpret them as compared with nonliving matter. Neils Bohr says: "The existence of life is an elementary inexplicable fact, which must be taken as a starting point in biology." Sir J. Arthur Thomson said: "Life is a unique kind of activity, requiring concepts transcending those of mechanism—and mind is independent."

So the chemist, the biologist, and the physicist, each in his own way and according to his point of view, finds an essential difference between living and nonliving matter and accounts for it by some unique arrangement of matter or behavior of energy in an organism which the same matter and energy does not possess before it enters the organism or after its death.

The mechanists have offered no proof or demonstration that a living organism can originate from nonliving matter to operate, develop, and survive through matter and energy, and nothing else. They speculate that it is possible.

We are able to synthesize many products of living things in the laboratory. We know the elements in protoplasm and can make shrewd guesses as to those in genes. We know a great deal about the conditions under which the simpler and more primitive life processes operate. We can keep life processes going by artificial arrangements, under unnatural conditions and environment, when they have been started by living things. We have so far been unable to create a living particle with or without a "tendency to mutation" from nonliving material.

I cannot imagine any combination of substances and conditions existing in the ancient seas that could not be reproduced in a modern laboratory. I find it hard to believe that such a complex organism as protoplasm, or "gene substance," was assembled, combined, and activated by any chance combination occurring in the primeval sea. The probability against its creation, multiplication, and survival, if there was nothing in it but matter and energy, seems to me to be of the same order as that against the ten all-spade bridge hands.

To sum up, we have found no proof that the simplest living organism can be produced without resort to some existing living thing. Furthermore, it seems that even if this were possible, it is very improbable that it could have resulted from chance combinations of matter and energy.

From this brief reference to origin, we must now pass to behavior and continue our

search for evidence. All living things grow. There is also what may be called growth of nonliving things, as in snowflakes or other crystals. Is this nonliving growth the same, or is it essentially different from the growth always associated with life? If life is a separate entity, we should expect the latter to be the case. Failure to find essential differences would support hypothesis A.

Both living and nonliving growth require that suitable material be available in a suitable environment; it is hard to imagine how it could be otherwise. Here the identity ends and the difference begins. Consider snowflakes and infusoria. The former begin to grow without the presence or agency of other snowflakes. Infusoria start only from living infusoria or from very similar living organisms. If I am asked how the first living organism started, I must reply that I do not know and that I cannot see that it has any bearing on the question. I do know that when life appeared a new kind of growth began and has continued.

The growth of crystals and, so far as I know, of all nonliving things involves only the addition of more of the same material to the structure. The growth of living things always involves the assimilation of material, its use to build the structure and to supply energy, and the elimination of used material, an entirely different process. A living organism cannot grow without the elimination of waste products.

As snowflakes grow, there will be many different patterns, depending upon conditions in the medium in which they are growing, but in general all will have a hexagonal symmetry. The infusoria will grow according to predetermined patterns similar to, but not always identical with, the preceding generation. The conditions of the medium may vary within reasonable limits, but the growth of living things sticks to the pattern it inherits from preceding life. If conditions vary too widely, the snowflakes may melt and the infusoria may die. In the first case, the material might, under suitable conditions, be reconstructed into new snowflakes, but the material of the dead infusoria cannot be reconstructed into new infusoria except by the action of life.

A copper crystal may grow in a solution and, if conditions are kept favorable, may become quite large, but it is always a copper crystal. We cannot imagine its growing old and dying; we can, however, imagine its remaining unchanged for millions of years—in fact, for as long as we please. The smallest copper crystal is like the largest, except for size, but the big tree is not like the small seed from which it grew, nor does the moth resemble the caterpillar. In a suitable medium of inorganic materials we can cause the growth of structures that look very much like mushrooms or other living things. We can make a drop of oil or chloroform act like an amoeba: it will perform movements that closely resemble those of the animal and even imitate some of its life processes, such as digestion and extrusion, but none of these mechanisms is alive any more than an adding machine is alive because it can add if properly manipulated.

I think the great difference between nonliving and living growth is that the former is determined and controlled by the materials supplied the medium in which, and the conditions under which, it occurs; the case is quite different for living things.

It is not the matter that enters a living body nor the conditions under which it lives that determine in kind how it shall grow and what it shall be. These follow a plan inherited from preceding life. The arrangement of material is determined and directed by something that is not to be found in lifeless matter. An excellent illustration of this principle impressed me as a boy. There was in our yard a pear tree that had been

grafted upon quince stock. Quince shoots had sprung from below the graft. The sap that came from the roots to build and nourish the central pear tree and the quince branches was identical, the weather and environment were the same; yet one grew as a pear and produced pears, the other as a quince and produced quinces. I have never been able to find an explanation for this and similar phenomena in the behavior of nonliving matter.

Life possesses the power to cause matter to grow according to a predetermined plan, which even in the lower forms is amazingly complex and elaborate. The tiny fertilized egg carries in itself the plans and specifications of an oyster, a mouse, or a man. Conditions and environment can kill them or change their growth in degree but not in kind.

Reproduction and metabolism are so closely associated with growth that it is hard to speak of one without involving the other; the same may be said of evolution. I do not think we can find any phenomenon of nonliving matter that even remotely resembles the phenomena that we observe in reproduction, metabolism, and the evolution of individuals and species; nor can we find in the properties of matter and energy anything that will satisfactorily account for them without the addition or interposition of something that is not found in any known property of matter or energy.

In all living things we have the reproduction of successive generations of similar but not identical units. There seem to be two general methods: by division and sexual. In the first, one makes two; in the second, two make one or more. Chemical processes where life is not present, if carried out with the same elements or compounds under like conditions, produce like products. In the life cycle the ever-present tendency to variation and mutation, with the survival of the fittest and natural selection, produces profound changes in species.

Metabolism is also unique in living things in that it can be described as the purposive assimilation of matter in a living body. It begins and ends with life. There is nothing unique in the chemical and physical processes involved. Many so-called organic substances are being synthesized in the laboratory; sugar can be formed in a suitable solution by the action of sunlight on carbon dioxide. In living things metabolism is obviously designed and directed to effect the survival, reproduction, and multiplication of the plant or animal. Have we ever found, or can we imagine, anything in matter or energy that would be concerned with the survival and reproduction of a particular configuration or arrangement of matter?

The same question might be asked about reproduction, which has given life its power of survival. The duration of life in an individual body is very short—perhaps 3,000 years in a redwood tree is the longest —but its duration in a species may outlast mountain ranges. It has survived on this planet for a billion years and, barring very improbable events, seems destined to continue here for many times that long.

The attributes of life I have so far discussed and what I have had to say about them are such self-evident matters that it may seem superfluous to have referred to them. Many other phenomena and examples deriving from the properties of life in its lower forms will occur to the reader. These could be discussed at great length. My only excuse for even the brief space I have given to stating and suggesting well-known matters is that they must be considered in any review of the evidence. They all have a very direct and important bearing upon the question before us. If neither matter nor energy possesses in itself or in combination the power to produce these phenomena, without the addition of something that is not to be found in matter or energy, hypothesis A cannot be true.

We still have to say something about the evidence that may be derived from the phenomena of the mind, which are exhibited by the higher forms of life in addition to the more universal properties just discussed. There are also several less apparent, more subtle, matters that it may be interesting to refer to very briefly.

Life, whatever it may be, evidently adds to the matter-energy combination the power to observe and remember, to think, to convert thought into action, to communicate observations and thoughts, to have, and to be influenced by, emotions. This enumeration is both overlapping and incomplete but will serve our purpose. These activities must be accounted for, either as being due to the reaction of energy upon certain peculiar arrangements of matter or upon a combination of matter and energy with something that is not in, nor a property of, matter or energy. We can undoubtedly do wonderful things with matter and energy, but our question is: Are matter and energy alone doing these things with us? Personally, I find it very hard to imagine that matter and energy and nothing else are making me write this article. It is even harder, in fact, impossible, for me to imagine that our ability to think, or even the more common phenomena of life, are the result of, and developed from, chance combinations of matter and energy.

It has been said that men and all living things are machines. This is true; they are all more or less complex mechanisms built of matter and using energy, designed to perform the functions associated with life. They all work by chemical and physical processes. When the living machine is wrecked or wears out, so that these activities can no longer be carried on, life disappears and we have nonliving matter. The machine is still there, but it is like an electric motor with the switch open. The motor, however, may be made active again by closing the switch. The matter in the dead animal or plant may be appropriated by other living things and live again in their bodies, but we have so far found no way to bring life back to the original body after it has really left.

We find what may be truly called machines in the nonliving world. Tornadoes and cyclones are heat engines on a vast scale. We can build machines ourselves to do wonderful things: to rule 30,000 lines per inch for a diffraction grating, to add, to multiply, and to solve differential equations; but we know very well that our machines are not alive. If hypothesis A is true, the living machines, including ourselves, consist of nothing but matter and energy. The ability to produce and account for all they do, feel, or think must be found in matter and energy, separately or in combination. All this is simple and obvious, but it leads to some interesting and important conclusions.

It appears that if we believe in hypothesis A we must also believe in some things that to me seem highly improbable. A quotation from *Some Consequences of Materialism*, by J. B. S. Haldane, will provide a good example: "If a super biochemist made a working model of me, atom for atom, this robot would on a materialistic view have all my memories. This may be the case, but if so, I do not see how knowledge is possible."

This robot that Haldane suggests is a very interesting creature. He is absolutely identical with Haldane, so far as matter and energy are concerned. Hypothesis A says he would be alive, that he would possess and could use all Haldane's knowledge, personality, experience, and reasoning power, as well as all other functions of life. B says, No, unless he has something else that is not matter or energy, he is not Haldane and he is not alive.

Even if we apply this idea to a far simpler organism than a distinguished biochemist, say, to the tiny fertilized egg of a fish or insect, the argument seems to me to be equally powerful against those who believe

that all the phenomena of life can be accounted for as due to a rather special arrangement of carbon compounds in a colloidal state and nothing else. To agree with them I must believe that the position and arrangement of the atoms in the egg will cause it to grow through all the phases of the life cycle of the fish or insect. Because of this arrangement of atoms, and nothing more, it will know how and where to seek its food and avoid its enemies. The salmon after four years will return from the wide spaces of the Pacific to the stream where it was hatched. The queen bee will lead a swarm from the crowded hive, and the remaining bees will provide themselves with a new queen. I find all of this very improbable and hard to believe.

It may be asked: If the body is perfect chemically and physically, why should life not enter it and begin to function as it does in an embryo or as it entered the first organism on this earth? We can only answer that, so far as our experience goes, life does not behave that way. It always breaks off from some living organism and begins in the new body in a very humble, small way, whatever greater complexity and size it may attain, it gets by growth. Also, if it is necessary for something that is not matter or energy to enter, to be added to, or to take charge of, the perfect robot mechanism in order for it to produce the phenomena of life, hypothesis A cannot be true.

It also may be pointed out that the phenomena of life often appear as blind and purposeless as those of the inanimate world. A very large proportion of living things live by destroying other living things. The majority of the species produced by life have ended in failure and extinction. Life processes are easily perverted from their apparent objects: witness, cancer or the piece of embryo chicken heart that will apparently go on living and growing as long as it is properly nourished and tended, although there will never be a chicken to use it. All this and much more of the same nature is true, but it has nothing to do with hypotheses A and B. They assert nothing as to the purpose or method of life. They carefully confine the question to something for which I think we have evidence and reason sufficient to give us a satisfactory answer.

I also think there will surely be questions about those phenomena that have been recently observed and studied in connection with viruses. Many believe that they belong to a borderline, or half-life, state, that they are a link between living and nonliving matter. They seem to consist of large protein molecules. One virus, that of tobacco mosaic, has been studied intensively: it can be isolated and crystallized; it may be dissolved and recrystallized a number of times. The crystals are like other nonliving organic compounds; if, however, a living tobacco plant is inoculated with them, the leaves begin to produce more mosaic.

This action to some extent suggests that of a catalyst which brings about or stimulates chemical reaction without itself taking part in it. It is also similar to that of many chemical agents that cause a living body to generate or secrete certain substances. Viruses have no power to cause growth except in living bodies. The interesting point is that they cause the living body to produce more of the virus. As far as I know, there is no evidence that the protein molecules divide, reproduce, or grow in the manner of living cells. It is rather as if a dose of morphine caused a body to manufacture more morphine. The biochemist and the biologist will undoubtedly discover more about these curious substances, but in the present evidence I can find nothing in favor of hypothesis A or unfavorable to B.

The phenomena of the mind do not seem to be limited in time and space as are those arising from matter and energy. The following may be a poor example, and the

reader may think of others that he prefers. Nothing that happens in a distant star can affect matter upon this earth or be observed by our senses or instruments in less time than it takes light to travel from it to us, say, a million years, but knowledge of the state of the star as it was a million years ago may cause a thought about what is happening there today. This thought is a product of life, which differs in no way from a thought about what is happening in the same room as the thinker, and may result in some action by the thinker at the present time.

I have already referred to a possible exception to the rule that all phenomena of life must be perceived by senses that are designed to respond only to matter and energy.

Those who believe in telepathy as a transference or communication of thought between individuals without exchange of matter or energy will probably consider it a valid exception, not to be accounted for by hypothesis A.

A great deal more might be said from the viewpoint of psychology about our question, but I am not a psychologist and prefer to keep evidence and argument on simpler and more obvious lines. I will mention only one other point. An interesting argument might be based upon the statement that the employment or acceptance by the mind of any symbol, a word or figure that stands for something entirely different from itself, proves that something other than matter and energy is present. When matter and energy only are concerned, a word spoken or written can only produce the effects due to a certain combination of sound waves and pressures, or a particular combination of reflected light waves (or particles, if you prefer), or both. The only effects are those due to sound or light waves. When the higher forms of life are concerned, the effects produced are of an entirely different nature. The material effects received by the ear or eye are reported to the brain, where they are translated according to a prearranged code. Immaterial thoughts and emotions are produced which cannot have their origin in the purely physical effects of the reception of the energy signals. Thoughts and emotions in turn produce material action of matter and energy. All this occurs only in living things. If someone answers that pulling the trigger of a gun or dialing a number on a telephone switchboard produces effects that are very different from the energy signal received, the answer is readily apparent. Each action in the chain of dissimilar events produced is the purely physical or chemical consequence of the preceding events. The gun or the switchboard mechanism do not act because they translate a symbol into something entirely different but do what they must because of direct physical or chemical compulsion. To me, this indicates the presence of something that is not matter or energy in the working of the mind.

Perhaps some advocate of hypothesis A will say that the interpretation of the symbol by the brain is all done by purely physical or chemical processes we are beginning to understand. The point is, however, that these processes that take place in a living body—electrical generation and transmission, oxidation, osmosis, the production and transmission of chemical reagents—are correlated, regulated, controlled, and integrated by something that cannot be accounted for by any of the attributes of matter or energy.

Another quotation from Schrodinger may throw light on this aspect of the question:

> My body functions as a pure mechanism according to the laws of nature Yet I know by incontrovertible direct evidence that I am directing its motions, of which I foresee the effects, that may be fateful and all-important, in which case I feel and take full responsibility for them. . . The only possible inference from these is, I think, that I—I in the widest meaning of the word, that is to say, every conscious mind that has ever felt or said I—

am the person, if any, who controls the motions of the atoms according to the laws of nature.

Again, one who prefers hypothesis A may ask: If the possibility of life is not inherent in matter and energy or any combination of the two, how did it begin in a lifeless world? There is of course only one sound answer: We do not know. Nor do we know how gravitation acts through millions of miles of empty space, but this in no way invalidates the fact of gravitation as a physical force and an attribute of matter (or, if you prefer, the result of the property of matter which causes curvature in space—the result is the same). I think we shall learn much more about these forces in the next thousand years. For the present we know a good deal about how they act but little, perhaps nothing, as to why.

We do know that a billion or more years ago life appeared upon this planet and began to reproduce, multiply, and develop. The first living cell was a relatively simple affair, but it contained in itself the potentialities and the plan of all the innumerable and diverse forms that exist today, from amoeba to man. This is not a theory, but an accomplished fact.

Is it possible to imagine that this enormous program was contained in the atoms and energy of the original cell or cells? Or even that the program for the growth and development of a tree or a man is contained in the atoms and energy of the fertilized cell? All we know of atoms and energy seems to me to say this is most improbable.

The alternative that the whole program was the result of chance combinations is far more improbable, more improbable than anything I can imagine.

If we cannot find an explanation for life inherent in the known properties of matter and energy and cannot accept the pure chance theory, we may suggest that it is due to the direction of some supervising power or intelligence within the universe. If this is the case, hypothesis A is untenable and B stands; the directing force is the third entity. (By definition we cannot consider a creator of the universe as "in the universe.")

I think it was Conan Doyle who said, in effect, "When I have eliminated from a problem all impossible solutions, what remains must be accepted as fact."

We set out to determine one important fact about life. Using severe economy in original assumptions we reduced our question to two simple hypotheses, one of which must be true but both of which cannot be true. I have presented such evidence as seemed to me to have a direct bearing upon the question as defined and limited by the two hypotheses.

My own conclusion is that the evidence presently available indicates such a high degree of improbability for hypothesis A that it is practically impossible to accept it except by an act of faith and a denial of reason. I have found nothing in the evidence to indicate any similar degree of improbability for hypothesis B. I must therefore reject A and accept B and believe that there are in the universe three fundamental entities, one of which, because it was a short, simple, and comprehensive word, we have called *life*.

CONSEQUENCES

While by the acceptance of hypothesis B we assert nothing about life except that it is a separate entity, the recognition of the fact should be the first step toward the study of that entity by the scientific method, which has given us all our knowledge of the other two fundamental entities. There are also several rather interesting speculations that follow as natural consequences of the definition.

For instance, it seems reasonable to suppose that some of the laws that apply to matter and energy are special cases of universal laws that apply to all three entities, just as some of Newton's laws were found to be special cases of the more univer-

sal laws developed by Einstein. If this is the case, such laws should be of the most general nature; probably they would not be defined in time or space. The conservation laws and the equality of action and reaction are of this nature. The first simply states that neither matter nor energy is destroyed or completely disappears, it simply changes form. Also we believe that neither matter nor energy is created from nothing. "*Ex nihilo nihil fit.*"

If this idea, which seems reasonable but for which I see very little hope of finding definite proof, is correct, the law of conservation applies to life. It is neither created nor destroyed, it simply changes form. The sum total of life is a constant. Perhaps the total of matter, energy, and life is a constant. It follows that life, whatever it may be, can never become nothing. Death means departure or change of form of that which made the phenomena of life possible. Where it goes or how it changes, we have not at the present time any scientific evidence, but that something becomes nothing cannot be imagined, except by blind faith that accepts a statement without question or examination in the light of present knowledge and past experience.

I think it also follows that if life is a separate entity in the universe, and the conservation law applies to it in the same sense as to matter and energy, we should be very egotistical and foolish to suppose this planet to be the only spot where it exists. There are billions of suns in our galaxy and millions, perhaps billions, of galaxies. How many suns have planets we do not know; that our solar system is unique, or even an extremely rare type, seems highly improbable. Using estimates of one hundred billion suns and one system of planets to each million suns gives us one hundred thousand solar systems in our galaxy. If we assume that each of these has one planet where conditions are such that life can exist, we have a hundred thousand habitable worlds. When we consider the tremendous range of conditions to which life has adapted itself on this world and the diversity of its forms, from bacteria to whales, from algae to men, I do not think the one planet per solar system is an unreasonable estimate. The immensity and the uniformity of that part of the universe we have been able to explore with our telescopes make it seem far more probable that there are millions of worlds containing life rather than only one.

If life came to this planet from somewhere else, and if it cannot be destroyed, it seems unreasonable to think there would be nowhere for it to go if this planet were destroyed or became untenable. If the conservation law is universal, it could not vanish into nothingness any more than the matter and energy. If our sun became a nova, our world and all it contains would probably be gasified. Some of the matter might be converted into energy and radiated into space, but not a grain of it would be destroyed in the ultimate sense. The sum total of matter and energy in the universe would be the same as it is today. Should not the same conclusion apply to life?

HUMANICS: A CRUCIAL NEED

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NATURAL scientists have manifested, all will agree, a greatly increased attention to social and political problems since the advent of the atomic bomb. What will be the net result of this changed attitude? Will it merely mean that they will participate more freely in established sociopolitical activities and as a result will vote more often and more intelligently and be called upon more often to give expert testimony in the fields of their competence?

May it not be that *as natural scientists* they can make special contributions of a more direct and constructive nature contributions which no one else can make? Can it be that these unique contributions will be important, possibly even comparable to the contributions of science to the advancement of technology?

An affirmative view with respect to these latter questions may be based upon the assumption that all social and political problems center in man and upon a confidence that natural science can, if it will, make vast contributions to the better understanding of human beings. The natural-science approach to any subject involves getting to the bottom of things, and in this approach to social problems understanding the units which enter into every society becomes an inescapable necessity. It should be evident that civilization is not endangered by outside forces which we cannot control. If we are destroyed it will be an "inside job." Atomic bombs and biological warfare are of our own making and are not dangerous unless *we* make them so. We human beings are the key to our own troubles.

If natural science is to be given an opportunity to demonstrate its full usefulness in the social field, we must develop an applied or practical science of humanics which will have as its aim *gaining scientific understanding of human beings that will be useful in solving social problems.*

The comprehensive study of man from the purely scientific standpoint is a desirable end, but what we are seeking to describe here is more restricted than this, and has to do with obtaining scientific information that gives promise of being immediately useful. As an applied science it must have its basis in pure sciences but, like other applied sciences, it must involve a number of disciplines and cut across a number of fields.

Wood technology as an applied science deals with everything that has to do with the production and use of wood. It is concerned with wood's microscopic anatomy, biological history, and even its pathology as well as the more immediate problems of sawing, milling, seasoning, and finishing. It is concerned not only with its preservation against bacteriological, mycological, entomological, and other enemies, but also with its modification, pulping, distillation, and the adaptation of it or its products to a multitude of uses. It deals not only with the chemical aspects of wood, but also its behavior as colloidal material as well as its purely physical properties. Any type of science is utilized that will contribute to a better knowledge of wood because it thereby contributes to the more effective use of wood.

It is true not only of forest products but also of other material resources, coal, oil, fisheries, soil, and agricultural products, that maximum development cannot come about until an applied science is developed

with respect to that resource—*until there are scientifically trained individuals in substantial numbers who center their attention on the particular resource in question and seek to develop its full and diversified utilization.*

If concentrated attention is required in the case of the material resources cited, how much more important is it when we deal with the paramount human resources? We need to develop the applied science of humanics, which will cut across all boundaries and devote itself to every aspect of man's existence that appears important in his social relations. It will require for its development the full gamut of the sciences from mathematics, physics, and chemistry to biology and psychology. Nothing that is important in man's life should be left out of the picture. It should be obvious that this applied science may have long-range as well as short-range objectives and is not one that can be developed overnight nor with the expenditure of a few thousand dollars. If it is worth doing, it merits being taken seriously in the same way that we take war seriously and it needs to be supported with something of the same generosity that prevailed in the case of atomic bomb investigations.

The question very naturally arises: What of the science of the past decades? Has not man been studied assiduously and intensively by experts in the various fields of knowledge? Surely scientists have not been asleep.

But the answer is a ready one: Natural science as it is related to man has very often been of a variety which does not concentrate upon those aspects that may be important in society. The idea of studying human beings scientifically and comprehensively because of the importance of the information for social control has been entertained very sparingly and by relatively few people.

From the standpoint of a practical science of humanics, our scientific study of man has had the serious limitation that we have become far more familiar with the pieces which make up the jigsaw puzzle than with how they fit together. Actual complete individuals such as make up the membership of society have not had the full searchlight of science turned upon them. In dealings with human beings we are inclined to depend upon unscientific observations and mere opinions and plausibilities rather than on scientifically gained information. In our everyday application of human science we are in the age of the oxcart.

Our piecemeal study of man has come about because it is inevitable that he should be studied by specialists who become interested in narrower and narrower segments of man's existence. It is only by such specialized study that advance can be made. But an intelligent society will not only develop, encourage, and support purely scientific study of man; it will be alert to the practicalities as well and will be constantly seeking to find and apply knowledge that is socially useful.

The general pattern for studying man scientifically has been about as follows: An anatomist, for example, makes many dissections, takes many measurements, produces many microscopic slides, and as a result learns the intimate details of body structure. Nowadays if he is to discover new information he must specialize on some part of the body, on some organ, or on some specific type of tissue. By the labors of a large number of independent anatomists, it has become possible to amass more and more complete information about the intricacies of structure of every tissue and organ in the body.

Physiologists tend to follow the same plan. Some investigate muscle physiology, some study digestion intensively, some become expert in the physiology of hearing, others investigate some phase of the circulatory system. A large number of individual specialists amass new knowledge

about the functioning of numerous organs and tissues. Biochemists try to push the curtain further back and try to determine what is transpiring, chemically, in each tissue and organ. Psychologists specialize on various phases of mental activity, and by the summation of their efforts there is accumulated a vast fund of knowledge about the diverse aspects of mental processes.

The outcome of this method of attack is that no individual investigator is encouraged to know or care about the whole picture of man or even about a large segment. The active investigators all tend to be absorbed in their respective special fields.

How has society provided for scientifically trained experts whose concern is whole men and the way they fit into society? Who is actively engaged in scientific exploration in this field? One may answer that biologists, biochemists, and psychologists are not always narrowly trained. They are all citizens and have their social responsibilities. In practice, however, what is everybody's business is nobody's business, and the responsibility for the broad scientific study of man falls exactly in that category.

It cannot be denied that a few individual scientists and individual groups have taken seriously the problem of the broad scientific study of man and its import for civilization. The Yale Institute of Human Relations is a notable example of such an effort, and there have been, and are, other such groups associated with various child welfare centers, clinics, and educational projects. These endeavors, while often meriting far more support than they have had, fall short of constituting an answer to the question that has been raised here, because in no case has a substantial amount of attention been devoted to investigation of the physiological, genetic, and biochemical aspects. Each of these is, in my opinion, far too basic and fundamental to receive minor attention or stepmotherly treatment. What is needed is a study of human beings broad enough to encompass all fundamental fields and deep enough to reach the frontiers of knowledge in the various fields.

One of the forceful pleas for the broad study of man was made over two years ago by Professor Lee R. Dice[1] who pointed out "no investigation or group or investigations now in progress is in my opinion sufficiently comprehensive to secure anything like a complete picture of man the animal, as he exists in this constantly changing world." Later in the same article he says: "... The biology of man is certainly of no less importance than the biology of dairy cows or other domesticated animals. . . . Every state should in my opinion maintain and generously support a permanent center for research on man."

Dr. Brozek and Professor Keys[2] have also pointed out the importance of "interdisciplinary research in experimental human biology" and have briefly described the work of the Laboratory of Physiological Hygiene at the University of Minnesota:

> The organization of the laboratory is built up on the conviction that a meaningful attack on the major problems of applied human physiology requires a cooperative approach. Thus physiologists, biochemists and psychologists, together with technical assistants, work as a team.... For many problems it is desirable to have ready consultation with members of other university departments—clinicians, physicists, chemists and engineers.

These scientists, as well as others who may have expressed similar views, have been doing important pioneer thinking and planning, but it should be pointed out that such work has never yet been supported by society in a substantial way. An applied science needs to be fully developed whereby scientifically trained individuals in substantial numbers will center their attention

on human beings and will seek to use all the resources of scientific investigation to further individual and social welfare.

Exactly how the applied science of humanics will develop or what lines of approach may prove most fruitful will be for the future to decide, and no one can forecast in detail the findings that may be made. In my opinion, a line of development that will prove most important concerns attention to human individuals. In the natural sciences, with some notable exceptions in the field of psychology, we have applied ourselves with singular devotion to the biological robot *man-in-the-abstract*, a hypothetical being who plays a highly questionable role in actual human society. We have not considered our scientific findings about man in a sufficiently practical light and have paid far too little attention, it seems to me, to actual human beings.

It is entirely natural and justifiable that we should, in the earlier stages of our social thinking, make the assumption that we can deal with society in a simple statistical manner and consider it to be made up of duplications of the hypothetical "average man." In this case "deviates" are unimportant in connection with an over-all picture.

If we were interested in studying society en masse and without regard to individuals, this assumption would perhaps be adequate, but actually our attitude is very much different from this, as reflected by a quotation from Einstein, a man not unacquainted with statistics, who has said, "All that is valuable in human society depends upon the opportunity for development accorded to the individual."

When we regard the welfare of individuals as of high-ranking importance (and I believe most of us do), the error of thinking in terms of man-in-the-abstract can be inconsequential or very serious, depending upon whether or not men show high variability. If we are safe in concluding that men are substantially all alike and that becoming acquainted with one acquaints us with all, then our error will be small. To the extent that this assumption is untrue, the concept of man-in-the-abstract becomes misleading and we go astray.

It seems obvious that the roots of many conflicts and many social problems lie in the very fact of individuality—our differences in appearance, in opinions, in attitudes, and in behavior—and that scientific study of human beings which has as its aim the improvement in social behavior must take account of these differences, seek out their origins, and finally develop means whereby we can adjust ourselves to them.

There is abundant scientific evidence to indicate that individuality is prominently developed in those organisms high in the evolutionary scale and that each human being possesses distinctive characteristics which appear in every segment of his existence—in his anatomy, in the details of his metabolism, in his sensory and other physiological behavior, including his endocrine system, and finally in his psychological make-up.

In view of this fact it may seem like a hopeless task to deal singly with large numbers of human beings. How can we if they are so variable and if each one matters?

The problem of caring for individual differences within a population is essentially the one that confronts the quartermaster corps which has the task of fitting shoes on the individual men in a large army. There are no two pairs of feet that are precisely alike in size and shape; yet the comfort of every individual's feet is important. For the purpose of calculating the total amount of leather required for the job, it would be necessary to know the average size of the feet to be shod. But of course the shoes cannot all be made of average size;

in this case, very few feet would be comfortable and many would have to go unshod because they would find it physically impossible to put on the average-sized shoes. Fortunately, most people's feet show some flexibility; they can be made comfortable in shoes that are approximately the right size and shape, and providing an army with reasonably satisfactory shoes is by no means impossible. In order to do so, however, the differences in foot sizes and shapes must be recognized and detailed information must be available as to the exact extent and character of the variability.

Society, in order to know how to deal with its individual units, must have at its disposal information with regard to variability—not only with respect to foot sizes and other anatomical features, but also with respect to metabolic, physiological, and psychological characteristics. These latter are fully as variable and fully as important.

Let us cite a concrete example. For instance, most of our thinking with respect to nutrition and vitamin requirements is based upon the assumption that we are dealing with a population composed of average individuals and that all will be well if we care for these. Suppose we consider specifically the status of vitamin A requirements from this standpoint.

Those who are familiar with animal assay methods for vitamin A will recall that for a test about 10 depleted rats must be fed at each level of intake. Six of these must live through the 4-week feeding period and must gain not less than 12 or more than 60 grams in order to have their performance averaged with that of their fellows.

In order for rats to perform according to these specifications, they must not only be highly inbred but must be from mothers whose diet is controlled. An unselected group of rats would show too great variability. That large variability occurs even among highly inbred rats is strongly indicated, for example, by the recent work of Sherman and Campbell,[3] who found that if members of their colony were fed vitamin A at levels two and four times that required for adequate nutrition (for 58 generations), there was a much less variable rate of growth. It appears that some rats required for optimal growth much larger amounts of vitamin A than others.

With respect to the vitamin A requirements of human beings, we should expect an even larger variation than among laboratory rats since human beings do not constitute an inbred strain. Actually, we are very much in the dark with respect to human requirements for vitamin A. In 1932 Mead Johnson and Company offered an award of $15,000 for clinical research on this subject. The terms were as follows:

> The award will be made to the investigator (or group of investigators) who (1) Determines the clinical value of vitamin A (if any) in human medicine *or* (2) Determines the vitamin A requirements of human beings *or* (3) Determines whether vitamin A in amounts more than contained in a well-balanced diet is beneficial in human physiology.

Year after year has passed without any takers for the award, and 4 of the 7 appointed judges have died. Finally after 13 years the judges, with 4 replacements, have advised the donors "that it is their considered opinion that no report or reports have been published which adequately answer *any of the three stated requirements* of the award" (emphasis supplied). They also express the belief "that no adequate answer to the problem as formulated will result from current research" and recommend that the award be revoked.[4]

In view of other pertinent facts at hand, it seems most probable that the failure to find the answers to these perfectly legitimate questions lies to a considerable extent in the high variability of human demands and the difficulty involved in obtaining consistent results. It seems entirely probable that some individuals require for

optimal health five times as much vitamin A as others. (It is notable that particularly in the case of this vitamin different assortments of wholesome foods may yield widely different amounts.)

In the long run it may be as important for an individual to be informed as to his approximate vitamin A requirement and to make use of this knowledge as it is to know his foot size and to utilize this information. With respect to the sizes and shapes of shoes, which involve merely common sense, we recognize and adapt to individual needs; with respect to vitamin A requirements, which can be known to us only through scientific investigation, we think and act in terms of man-in-the-abstract.

Instances in the biochemical field in which the differences between human individuals have received scant attention could be multiplied almost indefinitely, in spite of the potential importance of the phenomena from the standpoint of human welfare.

Investigators in practically every area of learning related to man, whether it be anatomical, biochemical, physiological, or psychological, are at least dimly aware of the fact that in their particular bailiwicks individual human differences are strikingly, and often disconcertingly, prominent. What we have failed to realize fully is how generally significant the sum total of these differences is. We have failed to grasp how these various differences influence everything we do, say, or think—our sensory reactions, our movements, our emotions, our opinions, our social adjustments. An applied science of broad scope dealing with human beings has not existed.

The potentialities residing in the applied science of humanics, which will draw on the resources of all branches of science, are so numerous that the specific examples we might discuss in a short article would hardly be more than the proverbial drops in the bucket. The large problem of education, problems of marriage, the problem of employment and choice of vocations, the problem of health, the problem of a more intelligent (and scientific) selection of leaders in every walk of life, the problem of group bigotry (whose name is legion)—all these fields of application have been discussed at length elsewhere.[5]

In many of these fields it appears to me that we think and act too much in terms of man-in-the abstract, whereas we should be thinking in terms of individual variability and individual needs. Instead of trying to nourish every developing individual with the same educational food, we should find out more about how people differ with respect to the assortment of mental abilities they possess and suit education to the different types. Human variability in the psychological realm is very pronounced, but our knowledge with regard to measurable differences is scanty in comparison with what it might be, and our adaptation of education to individual needs is most often haphazard and unscientific. Technological advance should enable us to devote more time to such problems.

There is strong reason for believing that our terrific crime problem has one of its roots in our poorly adapted education, and that potentially valuable individuals develop into criminals because they possess unusual patterns of mental traits and abilities which do not find suitable nourishment or attention in traditional schools. If we could learn how to train every individual for useful work *along the lines of his or her abilities*, the crime problem would be tremendously ameliorated. This we cannot do by thinking and planning in terms of a society composed of individuals who are substantially alike and who are to be educated alike.

The subject of the scientific study of human beings for the practical purpose of helping solve social problems is so far-reaching and important that it needs to be

encouraged and supported in every quarter. Whether I have exaggerated the importance of the study of individuals remains to be seen and is immaterial from the standpoint of the central theme. It is a fact which can hardly be controverted by scientific readers that the most effective utilization of human energies requires a scientific insight into human beings. Human beings, alike or unlike, need to be studied in a comprehensive way and not alone from the standpoint of narrow segments of their existence. Such study is not encouraged by the present organization of our universities or of our research agencies, because in these organizations an investigator necessarily belongs in one specialized group or in another. The idea that physiologists, geneticists, biochemists, and psychologists should cooperatively study human individuals for the purpose of gaining the kind of information that is essential for solving many social problems is something that is not encouraged or made possible by educational and research organizations as they now exist.

It is not desirable at this time to befog the question of the crucial need for humanics by introducing controversial questions with regard to exactly how such a science can be developed. It seems safe to say, however, that no field of research is more worthy of federal support and that when and if a National Research Foundation is established there should by all means be a provision for humanics because it is absolutely fundamental to the building of a sound social order. Certainly every state-wide or local effort which moves in the direction of understanding human beings better should be fostered; the faster the movement spreads among the nations of the earth, the greater will be the possibilities of maintaining peace.

If scientific opinion can be generated in favor of developing such a science and the public is enlightened with respect to the need, I am fully convinced that natural scientists can make outstanding contributions to the problem of social control. Is not the time ripe?

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Book Reviews

FROM DEMONS TO DRUGS

Miracles from Microbes. Samuel Epstein and Beryl Williams. 155 pp. $2.00. Rutgers Univ. Press. New Brunswick. 1946.

THIS book was reviewed during the hours of 11:00 P.M. to 1:30 A.M. because the reviewer has that habit of so many people —namely, reading in bed—but of more momentary interest is the fact that the book was so interesting that it was read from cover to cover all in one reclining.

A foreword by Major General Norman T. Kirk points out the dynamic nature of science and leaves one with the happy thought that out of a horrible war came some advances in medicine which resulted in at least a lower rate of mortality; and thanks to penicillin many of our boys are alive today who could not have survived without the developments in the fields of antibiotics.

Chapter I, From Devils to Drugs, is a splendid review of medical treatment from medieval times to present-day antibiotics —one sees the panorama of the fifth century B.C. Hippocrates, the establishment of a medical school by Alexander the Great, the anatomical studies of Galen, the era of diagnosis but no cure, the philosophy of nature as the ideal chemist, van Leeuwenhoek's new world with the microscope, Jenner's smallpox vaccine, the Pasteur treatment, Koch's proof of the multiplication of bacteria, and Ehrlich's studies on dyes which were specific for certain bacteria. Thus was the world introduced to chemotherapy as a method of treatment of diseases.

Chapter II, A Problem in Isolation, relates the early trials and tribulations in connection with the phenomenon of antibiosis; pyocanase, pyocyanin, penicillic acid, fumigatim, and other substances were recognized. This was the period of training of men for research in a specialized field.

Chapter III is devoted to tyrothricin and the work of Dubos and others on this antibiotic. Unfortunately, this drug, isolated from *Bacilli brevis,* dissolves red blood cells, thus limiting its potential use. Solutions of tyrothricin injected into udders of cows infected with mastitis resulted in rapid cures.

So much has been written regarding penicillin that this subject is treated rather briefly in Chapter IV. The headaches of plant construction and the development of submerged fermentation technique are passed over in a few sentences, though this was the crux of industrial production. No mention is made of the research sponsored by the Office of Production Research and Development which had so much to do with the modern processes. On page 123 the authors state that "penicillin is still prepared for a grateful world by infinitesimal bacteria enlisted in the battle against their pathogenic brothers." On the previous page penicillin comes from mold.

Chapter V brings one up to date regarding streptomycin, which was discovered by Waksman and Schatz in 1943. This drug, which appears to be effective against many gram-negative organisms, is being tested against tuberculosis, leprosy, typhoid fever, Freidländer infections, tularemia, influenzal meningitis, undulant fever, and other diseases. Production is increasing rapidly, and clinical evaluation will follow.

The authors leave one with the impression that as each new form of plant life developed it must carry with it a mechanism for preventing the lower forms of life from eating it up. If so, the continued search for cures for infantile paralysis and the common cold may be discovered before long.

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THE CASE OF THE MISSING BODY

The Lost Americans. Frank C. Hibben. 196 pp. Illus. $2.50. Crowell. New York. 1946.

THIS is an entertainingly written and, one might as well add, exasperating little book. It will be read and enjoyed by that section of the public which is fond of action and mystery and which believes that the most dramatic explanation of strange events must always be the true one. Much of the action recorded here is true, the drama real. This is the story of the strangest chapter in American prehistory. It is the account of our discovery of the men who saw the last of the giant mammalian world of the American Pleistocene and who hunted its dying fauna to extinction. There are few more fascinating stories in the whole realm of archeology and few who can write about them with greater facility than Dr. Hibben. That is why I said that the book is exasperating—exasperating at least to a hardened old student of the subject like myself. It is too facile and a little too nimble here and there with its manipulation of fact.

At the very beginning *The Lost Americans* is marred by an unfortunately worded acknowledgment, the effect of which is a somewhat rude dismissal of a number of workers, by no means obscure or insignificant, who have contributed to the solution of human antiquity on this continent. Dr. Hibben says:

> The author wishes to acknowledge the help and inspiration of the *two other scientists* in the United States who have concerned themselves with the problems of the earliest Americans—the late Edgar B Howard, of the University Museum, University of Pennsylvania, and Dr Frank H H Roberts, Jr, of the Bureau of American Ethnology [italics mine]

It is entirely appropriate that the significant contributions of the late Edgar Howard and of F. H. H. Roberts, Jr., should be specifically noted, but in the three words I have italicized there is an insinuation that Hibben and the two scholars mentioned have been the only ones to concern themselves with this subject. In later pages, Hibben himself fails to be consistent on this matter, since he mentions Bryan the geologist and certain other scientists. Yet the ill-chosen words at the opening of the book leave an unpleasant impression which is many pages in being dissipated.

One may note, also, occasional statements which must be challenged in the interest of the general reader. We have, for example, no slightest idea of the life habits of *Castoroides ohioensis* which would make it possible to assert that "giant beaver built great dams across long-forgotten rivers" (p. 161). In Hibben's discussion of the extinction of the terminal Pleistocene fauna, he refers to the great bone deposits of Nebraska "where we find literally thousands of these remains together. . . whole herds overcome by some common power" (p. 170). Nebraska possesses probably the finest Tertiary fossil beds in the world, and many of these yield numerous remains. If, however, we pass to the terminal Pleistocene, which Hibben is actually talking about, I am afraid the statement above is, to say the least, exaggerated and I say it with due recognition of the fact that bison occasionally turn up in some number, as at the Scottsbluff quarry. In fact, Hibben himself is not consistent on this point because back on page 90 he

says: "In the Plains area.... where fossil bones have been found, they usually turn up in small quantities and in fragile condition."

To go on with this unfortunate catalogue, neither are the bones of mastodon to be found in Alaska in carload lots. The fact is that in this area their occurrence is extremely rare. As for Hibben's rash assertion that *Bison taylori* can be recognized and differentiated from a modern bison on the basis of a single bone (p. 116), I know of no reputable paleontologist who would dismiss the problems of bison taxonomy with quite such carefree abandon.

Nor does Hibben's assertion that the Eden Valley (Wyo.) site establishes Yuma remains as being associated with *Bison bison* entirely clarify the position of this "culture" in time. Yuma remains found at Scottsbluff were associated with long-horned bison. It is possible, of course, that Yuma is transitional to a modern fauna. But Linton Satterthwaite and Edgar Howard were unable certainly to identify the fragmentary bison remains at Eden Valley. Thus Hibben is either in possession of unpublished information of which the original excavators at Eden are unaware, or he has again ignored scientific caution for the sake of his story.

I think these quotations are enough to reveal that Hibben, in his popular writing, has a penchant for the sweeping statement and the realm of the spectacular which occasionally gets a little out of hand. Perhaps he dwells too enthusiastically on the possibilities of sudden destruction in an area where that placid old Pleistocene bovid, the musk ox, is still lingering toward his eventual disappearance.

Having said all the worst I have to say about the book, I mean no palliative when I reiterate that, though it does not (and this is perhaps wise) attempt to invade the deeper intricacies of chronology, it is a most readable introduction to the whole story of early man's intrusion into the American Ice Age world, what he saw there, and how he lived. It remains unfortunate, nevertheless, that this sketch of Folsom man, his quaint weapons, his missing body, and his weird animal associates, is presented to the interested reader through the hands of a professional archeologist who can write but for whom the plain unvarnished fact is sometimes not good enough. Dr. Hibben has it in him to become one of our ablest interpreters of archeology to the general public. His talents wait only upon a little more humility and a little more care.

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SOCIAL MEDICINE

Medical Education and the Changing Order. Raymond B. Allen. xviii + 142 pp. $1.50. Commonwealth Fund. New York. 1946.

THIS is the seventh of the series of monographs initiated last year by the Committee on Medicine and the Changing Order of the New York Academy of Medicine "as a contribution to contemporary thought on important questions in the general medical and health field." Several of the preceding studies have already provoked much discussion on the part of health workers. The first three were reviewed in these columns* under the heading "Prolegomena to an Inquiry into the Problems of Medical Care," amply informing readers of the composition, history, and purposes of the Committee under whose auspices these works are being presented. The author of the present volume, with degrees in both medicine and philosophy, is executive dean of the Colleges of Medicine, Dentistry and

*THE SCIENTIFIC MONTHLY, **60**: 319–320 (April 1945); **62**: 471–473 (May 1946); and **63**: 146–148 (August 1946).

Pharmacy of the University of Illinois and president-elect of the University of Washington.

This is much more than a book on medical education. It is an exposition of the author's basic philosophy, which he would probably say is scientific humanism, and which, applied to the problems of health and disease, has come to be known in Great Britain as social medicine. With this orientation he reviews the history of medicine. Using it, he analyzes such phenomena of the contemporary medical scene as licensure and certification, cultism, medical ethics, group practice of medicine, the increasing trend toward specialization, and the inadequate distribution of medical care. And in its light he examines the fundamental educational process and medical education in particular. That all these matters should be discussed in a book on medical education is eminently proper, for, as Dean Hinsey, of Cornell University Medical College, says in the preface, "it [medical education] is the keystone of the whole structure of medical practice and medical science and its quality determines their effectiveness in the service of society."

The author observes that man is at once a biological and a social organism, interacting constantly and simultaneously with his physical environment and with other units (other men and also institutions) of the social system to which he belongs. Health is present when there exists a harmonious balance both within the individual and between him and his external world. Disharmony and imbalance are disease, evidence that the body is unable to cope with untoward physical or social environmental influences. It is the same thesis which was so admirably stated by Surgeon General Parran in his speech "Charter for World Health" and which also appears in the preamble to the constitution of the new World Health Organization: "We are convinced that health is not merely the absence of disease or infirmity but a state of complete physical, mental and social well-being. . . ."

It is considered logical, therefore, by the author, that the modern approach to medicine should be not only the cure or even the prevention of illness, but rather the creation of a total situation which contributes most to man's well-being. These are the lines along which the medical curriculum must be improved.

Dean Allen believes that health is a fundamental human right. In this he also stands in firm agreement with Dr. Parran and the World Health Organization: ". . . the enjoyment of which we declare to be a fundamental right of every human being without distinction of race, religion, political belief, economic or social condition." It seems strange that this view should still need defending even after the conclusion of a victorious war for the preservation of democracy and human rights. Nevertheless, there has recently been a revival of the proposition that health is not a right but a "privilege." This point of view, however well intentioned it might be, and despite the fact that it calls itself "constructive medicine," inevitably provides grist for the mill of those who defend the *status quo* and resist changes necessary for the improvement of the nation's health. It is significant that those who consider health to be a privilege decry all attempts at "government interference," while accusing those with whom they disagree of being generally inadequate, politicians, or idealistic dreamers. Dr. Allen, who belongs in none of these categories, nevertheless believes that

> a government which attempts to do something about social, economic and industrial deficiencies and abuses in order to protect the well-being of the individual citizen is an ally of the physician in his struggle against those untoward environmental influences which make for occupational disease, maladjustment and degenerative changes in the individual.

The inadequate distribution of medical care, the reasons for which the author traces, are nothing more, he states, than a phase of the general inadequacy of our entire social and economic system of distribution of goods and services. He believes that there will be a gradual extension of both voluntary and governmental efforts, until an equilibrium between public and private services is reached. A serious responsibility of the medical school is to educate physicians in the socioeconomic problems of medical care.

The author's approach to the problem of specialization is of interest in view of the current debates on the question of "Will the Family Doctor Survive?" and the creation of a section for general practitioners at the last convention of the American Medical Association. He believes that specialists develop in response to "unsolved medical problems and the requirements of highly specialized techniques calling for great skill." But as rapidly as they in turn develop improved methods and simpler techniques, these are capable of being learned by general practitioners, and "the specialists take another self-propelled step toward oblivion." In other words, "a clinical specialty remains secure only so long as its techniques of diagnosis and treatment are so complicated and difficult that prolonged training is necessary to master them."

The statements on cultism are likewise sensible. "As the medical profession has had more of real relief from disease and suffering to offer patients, it has had less difficulty in curbing the activities of charlatans both inside and outside its ranks." The implication is obvious. It behooves the profession to attack this problem not so much by reviling the "irregulars" as by concentrating on becoming more proficient in the management of renegade patients.

Proper tribute is paid to the high quality of medical care rendered in our teaching hospitals by salaried physicians practicing as a group. "A well-organized staff exercises constant surveillance over itself." Incompetence cannot be hidden from one's colleagues under these conditions. The author neglects to point out, however, that young physicians fresh from medical school and internship, where they have received excellent training in the group practice of medicine, have in the main, under the present system of distribution of medical services, nothing to look forward to but individual practice. Many will thus have limited or no hospital affiliations, no surveillance, and no more of the intellectual stimulation which comes from contact with one's colleagues than can be found at an occasional medical society meeting. This is one of the basic paradoxes of present-day medical education. The view is expressed that

> comprehensive programs of postgraduate education for general practioners which aim to stimulate the normal processes of self-education and which give recognition of achievement by certification offer the best possibility for raising the standards of medical practice.

But no explanation is offered of how the average practitioner, surrounded as he is by competitors, may find the opportunity to leave his livelihood for significant periods of time.

On the subject of the cost of medical education and its corollary, the selection of applicants for admission to medical schools, there are statements like "economic barriers to higher education are being lowered" and "there are not enough superior and above-average students to fill the first-year classes of medical schools." This treatment will probably not receive full acceptance by realistic observers. The belief that "superior doctors are born, not made" is difficult to reconcile with the philosophical viewpoint maintained by the author.

In general, this scholarly work is such a welcome addition to the literature on the subject that it will no doubt long be remembered and quoted. It belongs in the library of every medical educator, physician, and health worker as a promise that there are more fruitful days ahead.

LEE JANIS

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SUPERSCIENCE

The Best of Science Fiction. Groff Conklin, Ed. xxvii + 785 pp. $3.00. Crown Publishers. New York. 1946.

THE editor, after a canvass of some 6,000 stories in this field of fiction, presents 40 stories which he considers "an adequate cross section of the whole field, historically as well as contextually."

He has chosen to classify the stories under the captions: The Atom, Wonders of Earth, Superscience of Man, Dangerous Inventions, Adventures in Dimension, and From Outer Space.

The authors of these tales include such well-known names as Frank R. Stockton, Edgar Allen Poe, Arthur Conan Doyle, H. G. Wells, and Julian Huxley. In an introduction, John W. Campbell, editor of *Astounding Fiction*, informs the reader that there are two research chemists, an engineer, a medical doctor, and a top-ranking mathematician in the list of authors. He further comments, "In recent years, the professional scientists have, more and more, taken over the pages of [these] magazines [of Science Fiction]."

The compiler's grouping, as listed above, suggests something of the diversity of the contents of the anthology. A perusal of the titles under a given group emphasizes that to an even greater extent. The themes of some of the seven listings under Part One, The Atom, are: International control of atomic power, superdestruction from atomic warfare, physiological and psychological effects of radioactive rays, disaster from runaway chain reactions, supermonopolies possible from atomic power. Biological, astronomical, engineering, and mathematical fantasies share in the entertainment provided in this collection along with tales from extrapolations from chemistry and physics.

The compiler emphasizes the purpose of such writings as wholly entertainment. The reviewer sees in them an added value: help for the busy scientist toward a more vivid awareness of the imaginary offspring of his prosaic generalizations. Since the achievement of atomic fission and its use for destruction scientists have become increasingly vocal regarding the uses to which their brain children are to be put, especially the energy of fission. If some find the going slow when they seek to project their thinking into the social, economic, or political worlds without their laboratory walls, perhaps science fiction may serve as a mental catalyst in initiating and facilitating that endeavor.

While there is always the possibility that the nontechnical public may take such stories too seriously, there certainly should be little hazard in their influence upon the thinking of a trained scientist. Fortunately for his purposes, most of them are reasonably short so that they can be conveniently used as a means of mental relaxation between periods of technical labor.

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Comments and Criticisms

THE CAROLINA BAYS

It has occurred to me that it may be worth while to put into the record some facts which concern a controversy which has been touched on in the SM and in other publications over the past several years. I refer to the elliptical inland "bays" of the Carolinas

About 1930 Fairchild Aerial Surveys, Inc, of New York City, undertook to make an aerial map of Myrtle Beach, S C, for J D Lacey, Timber Factors I was Fairchild's engineer at the time, Mr. E R. Polley being general manager During this general period, Professor Frank Melton, of Oklahoma University, made sporadic visits to Fairchild Aerial Surveys, Inc, to look over the files for aerial views and map photographs suitable for his use in physiography and geology, as I remember The indicia on the photographs of Myrtle Beach had interested me greatly, and when Professor Melton next appeared I brought these photographs to his attention, remarking that they gave the appearance of the result of impact of a swarm of meteors, particularly the overlay ellipses, which cut clean segments out of the rims of other ellipses Professor Melton was sufficiently interested to go into the matter, with the result that he and Professor Schreiber (?) made an exploratory trip to the area What they found they wrote up and published, a number of articles followed in sequence, by various authors, popular and otherwise

At about the time this matter came to the front, I learned from a Mr Birney, of Fairchild Aerial Camera Corporation, and a native of the discussed Carolina area, of the existence of two or more low hills in the coastal plane, at about the border of the Carolinas and not far from the coast Mr Birney stated to me that these hills were unique in two respects first, that they were hills, and, second, that they seemed to be composed of lumps of rock and iron rust Later, he brought a sample north with him, and it appeared to me to be as stated

I should never think of entering the controversy, as I am not qualified to pass on the evidence, being merely an engineer I should not even hazard a guess as to whether or not the odd hills abovementioned have any bearing on the subject, but I feel justified in hoping that what they indicate may be added to the evidence, if the indications are pertinent

In closing, I wish to remind you of the article by C Wythe Cooke, "Neptune's Racetracks" [April 1945] I apologize for appearing to comment on a dissertation by a scientist—I being merely an engineer—but I will say that if Cooke's solution is correct, then American industry is overlooking a source of cheap power that dwarfs the possibilities of nuclear fission However, I was particularly impressed by the classically lyric treatment of the whole subject by Cooke, his dainty brush-off of opposing views, and the Douglasian grandeur with which he drew his cloak about him in referring to younger scientists One would not be at all surprised if old Triton, if he could read and had read the article, blew a tattoo of derisive hoots on his wreathed horn. Perhaps, though, Cooke is right; possibly those hills of black and rusty rocks are the sole remains of Neptune's stables —EDWIN HOWARD CORLETT

ON THE MATHEMATICS OF COMMITTEES, BOARDS, AND PANELS

As a member of the AAAS I take the liberty of offering your editor a suggestion In your August 1946 issue you published an article by Bruce S Old Is there any chance that you could be persuaded to run some reprints of that article? It will not have quite the circulation of "Message to Garcia," but it ought to have! I can think of a dozen places where I could use a copy —EDWIN HOWARD CORLETT.

TOWARD A WORLD STATE

We are a group of young people who are trying to do something about stopping the next war. One of the most important is the teams of speakers we have on the road, distributing information and starting action groups like ours on other campuses. We badly need more and more literature like "Toward a World State," by Frederick L. Schuman.

We also publish a weekly newsletter called *The Planet*. Could we have permission to republish the very fine poem "Atomic Power," by Thomson King, which appeared in the October 1946 issue?—RICHARD ANDERSON, Students for Federal World Government

The Brownstone Tower

THE popularly accepted age at which one begins to grow old is forty years. From then onward we should be aware of, but not too much concerned about, senescence, the gerontologists' word for aging. It is an unfortunate word because it suggests senility, but it simply means that we are not as young as we used to be. If we are wise, we will progressively adjust our activities to fit our declining physical capacities and will be on the lookout, through our physicians, for signs of diseases that are common in the period of senescence.

Being interested in the facts about senescence and their implications, I thought the majority of the readers of the SM would be interested also, for I suspected that most of them are past forty. To make sure, I took a random sample of one thousand readers from information cards that were submitted this year for the new Proceedings and Directory of the A.A.A.S. The results of my investigation apply only to male readers because I soon found that most women did not record the year of their birth. Here, then, is the age distribution of my sample in ten-year groups expressed in percent: Age 20–29, *6.3;* 30–39, *24.7;* 40–49, *25.9;* 50–59, *22.1;* 60–69, *14.7;* 70–79, *5.3;* 80–89, *1.0.*

The figures show that about two-thirds of our readers are past forty—senescent. The peak comes between forty and forty-four, but there is little change in distribution between thirty and sixty. It is an interesting fact that readers past seventy are as numerous as those in their twenties. High-school students seem to be entirely out of the picture, since the youngest reader of my sample was twenty. The oldest was eighty-nine.

I have on my desk a new book of felicitous title, *The Second Forty Years.* It implies that the span of useful life should not end at the biblical three score and ten but perhaps at four score. It is appropriate to mention this book here because it was written by our former contributing editor, Dr. Edward J. Stieglitz, is approved in a foreword by past-president A. J. Carlson, and is one of a series of popular books of broad significance sponsored by the A.A.A.S. To members who wish to purchase the book the sponsorship of the Association means that orders for it may be sent to the office of the Administrative Secretary for transmittal to the publisher, J. B. Lippincott Company.

The Second Forty Years (x + 317 pages. $2.95) is not a Pollyanna book on aging. Dr. Steiglitz, a well-known gerontologist, presents the facts about the biology of aging and describes the changes that are to be expected as we grow older. He stresses normal aging but gives adequate consideration to the hazards of senescence—the insidious diseases that may overtake us, such as hypertension, heart disease, cancer, and diabetes. Obesity, a hazard if not a disease, is thoroughly discussed. Throughout the book he points out the bearing of the facts on the life of older people. As I was enlightened by this book, I recommend it to the aged majority of our readers.

Although Dr. Stieglitz does not neglect the psychological aspects of aging in his book, he stresses the biological and medical side. Another recent book, *Aging Successfully*, by George Lawton, is predominantly psychological. A chapter from this book, "Old Age: Minus and Plus," was published in the January issue of the SM. With these two books now available it is our own fault if, through ignorance, we fail to make the most of our second forty years.

F. L. CAMPBELL

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HARLOW SHAPLEY

PRESIDENT OF THE A A A.S. FOR 1947

By CECILIA PAYNE-GAPOSCHKIN
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IN ELECTING Harlow Shapley to its Presidency, the American Association for the Advancement of Science adorns the ranks of its leaders not with one man but with many—man of science, administrator, educator, internationalist. Even as a scientist he is a multiple personality - astronomer first, but biologist too. Even those who have worked beside him for several decades have not ceased to wonder at the secret spring of the energy that flows so effectively into so many channels.

Dr. Shapley was born in Nashville, Mo., in 1885. He graduated in 1910 from the University of Missouri. It was a significant moment for astronomy, in transition from the old concepts and methods to the vistas and techniques that were to alter the aspect of the universe.

At the University of Missouri Shapley worked with F. H. Seares (later of Mount Wilson Observatory), the man most responsible for the system of photographic photometry; and at Princeton (where he went for graduate work) he came in contact with H. N. Russell, exponent and interpreter of the new direct idea of the stars as physical entities. From the years at Princeton came a formulation of the methods of interpreting the light curves of eclipsing binaries that has become classical, and the first systematic discussion of the physical properties of eclipsing stars--work in which the contributions of master and pupil were intimately fused.

With his appointment at Mount Wilson Observatory in 1914, the second phase of Shapley's astronomical work began. The opportunity was unique: the world's most powerful astronomical equipment, the new techniques of photographic photometry, and the almost equally new knowledge of variable stars--typified by the period-luminosity curve and the cluster variables. It was the imagination and industry that Shapley brought to add to these ingredients that transformed the concept of the sidereal universe. The galaxy took on its modern perspective as a result of his studies of the globular clusters and the related investigations on the nature of the intrinsic variables. The geocentric universe was gone, and its place was taken by a galaxy—formless at first, but gradually taking its shape and its place as a stellar system among many.

On his appointment in 1921 as Director of the Harvard Observatory, Shapley was still a young man, yet with the equivalent

Photograph by Science Service

HARLOW SHAPLEY

of a lifetime of research already behind him. The third and fourth phases of his work opened before him. As an astronomer, he saw his field gradually broadening outside the galaxy to embrace the distant systems that are the equals of our own in their distribution and their diversity. His study of the groupings that are supergalaxies and the gradations of the metagalaxy has led the way in the modern picture of the expanding universe, whose measured extent is beyond the power of imagination.

As successor to Pickering, it fell to Shapley to transform the Harvard Observatory, heir to a classical tradition, into an institution in tune with the transformation of astronomy. The measure of his success in what is perhaps his greatest scientific task is found in the fruits of his directorate: the diversified output of a collaborating group of astronomers and astrophysicists; the modernization and leadership in equipment and technique; the scope and number of publications, and (by no means least) the creation of a school of astronomy, with its output of the young men and women who will be the astronomers of the future. The ability to combine creative work with the many-sided duties of administration has not deprived the world of the scientist by absorbing the director, and the third and fourth phases of Shapley's scientific career have proceeded concurrently during the twenty-five years of his directorate.

To a young student who asked the secret of his success, Shapley replied that it had lain in his power of synthesis: of recognizing, and giving effect to, the interrelationships of neighboring fields. It is this power that has led him to the fifth phase of his career—his work as an internationalist. Early a moving spirit of the International Astronomical Union, he has broadened his activities until they extend far beyond the strict boundaries of the sciences and concern themselves with the graver problems of understanding and cooperation among the world's peoples.

An account of so rich a career can scarcely be enhanced by the enumeration of some hundreds of papers, articles, books, and addresses which are its outward sign. Honors are familiar to him: eleven honorary degrees, the Draper Medal, the Janssen Prize, the Rumford Medal, the Gold Medal of the Royal Astronomical Society, the Bruce Medal, the Pope Pius XI Prize, the Crux de Honor, State of Puebla, the Aztec Eagle from Mexico, and world-wide membership in academies are the index of the recognition he has earned. Nor will presidency be strange to him; he is past President of the American Academy of Arts and Sciences, of the American Astronomical Society, and of Sigma Xi.

But the enumeration of academic honors, as of scientific papers, is a superficial index of the fitness of a man to be chosen as a leader by his contemporaries. They choose him because he is the outstanding exponent of the interrelation of science and society, and because he epitomizes the qualities that they, as men of science, would wish to possess—the alert, direct, realistic response to the universe as it is and the rare power to realize and to describe it.

THE WEALTH OF THE OCEAN

By WILBERT McLEOD CHAPMAN
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IN MAY of last year the Navy reported that in the course of underwater experiments with sonar instruments it had detected a layer of water off the California coast at a depth of 1,000 to 1,500 feet which showed the sound-characteristics of a semi-solid mass.

In this location it was known that there was no bottom for more than 2,000 feet below this level, and the picture on the sonar screen was not one of solid substance in any case. The matter was reported briefly in the local press, gave rise to some editorial comment about the mystery of the seas, and then passed from the public eye without its important connotations to the national economy having been appreciated.

During the spring sardine season Vince Cardinelli was headed into Monterey with a disappointingly small catch. As his vessel ran he switched on the sonic depth-finder, and, to his surprise, the ping showed that he was over shallow water. He knew from the landmarks that this was not true and, acting on a hunch, he set his purse seine where no fish showed. When the net was pursed it had so many fish in it that he could not haul them all in his big seiner and had to radio to other vessels to come to his aid.

A shark fisherman came to my laboratory some time ago with two strange fish that he had captured in his net off the Golden Gate. The net had accidentally slipped into a thousand feet of water, and, while he had not caught any sharks, he had caught two albacore (which surprised him a great deal) and another species that he had never seen before.

They were beautiful hard-fleshed fish about 3 feet long and looked so much like tuna that the fisherman thought they must be a cross between albacore and some other inhabitant of the depths. They were louvar and are so rarely taken that it was little wonder that he did not know about them; few scientists, even, have ever seen one alive. The California Academy of Sciences had none in their large collections of preserved fishes, and the number in the museums of this country can be counted on one's fingers. The louvar are tuna-like fishes of the great depths, and that is why

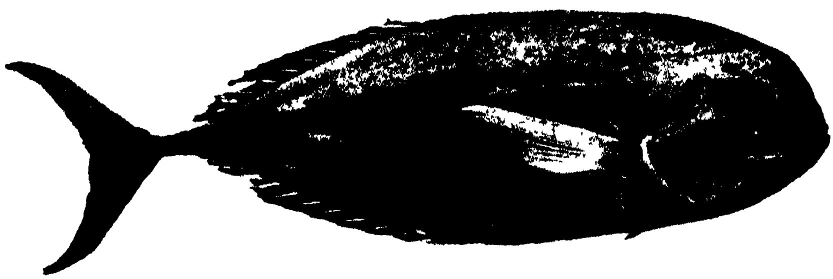


Photo by J. B. Phillips

LOUVAR (*LOUVARIS IMPERIALIS*)

A 5¾-LB. SPECIMEN TAKEN IN THE SURF NEAR MONTEREY, CALIF., NOVEMBER 29, 1939

From the Pacific Fisherman

A CAPACITY LOAD OF SARDINES

El Padre CARRIES ABOUT 170 TONS OF FISH IN THE HOLD AND 20 TONS ON DECK

they are so rarely seen, although occasional specimens are known from all over the Pacific, Atlantic, and Indian oceans.

I was interested not so much in the fish themselves, however, as in their tails. The tunas usually swim in dense schools, at least when they are feeding. The fish caught out of these schools have their tails frayed and worn at the tips as if the fish had been so close together that they had worn off the tips of their tails by whipping them against the other fish. When we were fishing tuna in the South Pacific during the war, we quickly learned that when we caught tuna with frayed-out tails they had come out of a school and that if we kept fishing in that spot we would get more fish. If the tails were shiny, the fish were only strays and there was no use in fishing longer in that vicinity.

The louvar brought to me had their tails worn off at the tips just like school tuna. I could not help wondering if these fish did not occur in large schools like their more common relatives that support our second most valuable fishery. This was not plausible if you believed that the waters beyond the continental shelf were barren wastes, for it takes a tremendous quantity of food to feed a school of large fish; tuna are such rapid swimmers that they burn up vast quantities of fuel.

THERE is no doubt that the layer of water discovered by the Navy off the California coast was a school of some living thing, either fish or fish food. As the evidence begins to pile up one can no longer accept unquestioningly the barrenness, from a commercial standpoint, of the far reaches and the great depths of the ocean.

One of the pieces of evidence hardest to reconcile with a belief in the barrenness of the great ocean depths is that furnished by the economy of the herd of fur seals which now lives on our Pribilof Islands in the Bering Sea. At the last census this herd amounted to something over 3,300,000 ani-

mals. We know that their diet is composed of fish, squid, and similar large inhabitants of the sea. We also know from our experience with seals and sea lions in zoos and aquariums that in order to keep a herd of seal of this size alive it would take, at a minimum, 3.5 billion pounds of fish or like food a year.

This is considerably more than the combined total weight of fish landed each year by our tremendous fisheries in the Pacific Ocean along the entire coast of North and South America from the Galapagos Islands to Bering Strait, including the Canadian catches as well as our own.

If there were any considerable competition between the fur seals and ourselves for fish, this would result rather disastrously for our fisheries, for the balance between the catch of the fisheries and the abilities of the fish stocks to replace themselves is a delicate one.

The much smaller herds of sea lions along the Pacific coast have deadly enemies in the commercial fishermen because the sea lions eat the same kind of fish that our fishermen catch. Our fishermen do not complain about the fur seal because the latter stay out on the high seas beyond where our fishermen work, and they eat fish from the deep seas which our fisherman do not catch.

But the truth is inescapably there. The fur seals are catching more food than we are out of the same ocean with us and are not in serious competition with us. This immediately leads us to ask why we do not try to compete with them and tap this vast reservoir of food for ourselves.

The reason we do not do this is our ignorance of the deep seas and of the seas more than 100 miles or so from our coast. The reason for this ignorance has been an almost complete lack of research by the Americans on the high seas of our side of the Pacific.

This is in strict contrast to the policy that the Japanese have followed. In the past 30 years they have conducted fisheries research from the Bering Sea to Antarctica, in the Indian Ocean and in the South Atlantic, seeking information which could be applied to a greater exploitation of the ocean's wealth—from the floor of the ocean to its surface.

Before the war they had at least 50 seagoing research vessels working on fisheries problems in the Pacific, and we have ac-

Photo by Victor B. Scheffer

FUR SEALS ON ROOKERY AT ST. PAUL ISLAND, PRIBILOFS

counts of 2 such vessels working in our side of the Pacific. Each of the Marine Prefectures had at least one fisheries research vessel, and some had several. In addition, there were fisheries scientists on all the large factory ships that worked off Mexico, in the Antarctic, off Kamchatka, and in the Bering Sea. There were laboratories in the home islands seeking fishing information on a scale of which we have not even dreamed.

The results were reflected in the amount of food taken from the sea by the Japanese. In the years preceding the war their catches amounted to about 16 billion pounds per year (a third of all the fish taken in the world). We were the second largest producers of fish and we took about 4 billion pounds per year.

Do not assume that the Japanese were catching some strange creatures that Americans would not eat. They were fishing for precisely the same kinds of fish that are the backbone of our own fisheries. A full quarter of the fish landed in the United States are sardines; the Japanese caught twice as many sardines as we did. The second most valuable fishery in the United States is for tuna; the Japanese landed three times as many tuna as we did. One of our largest fisheries in the North Pacific is for herring; the Japanese took four times as many herring from the North Pacific as we did. They had almost a complete monopoly of the canned-crab industry in this country as well as in their own.

This information generally goes unheeded, and we say that the Japanese are great fisheaters. But this is not the entire answer: A large part of their catch was sold in the United States, the United Kingdom, and elsewhere in Europe; their exports alone were greater than the total yield of our sea fisheries.

The reason this was so was not that their fishermen were any smarter or more hardy than ours, nor was it that they had better equipment, for no other nation can boast of

Photo by Victor B. Scheffer
A BULL FUR SEAL

as fine fishing gear and vessels as ours. It was because their fishermen knew when and where to go to catch fish on the high seas and in the depths as a result of the information furnished by their research men. They knew the best and most economical methods of catching, processing, and marketing their products—fields of research in which we have hardly made a beginning. One example is sufficient to show the difference between their methods and ours.

They had an albacore fishery of considerable size along the shores of the home islands in which the fishermen ventured as far as 100 miles from shore. We had a similar fishery for albacore along our west coast, which was similarly conducted close to shore. But the Japanese sent research vessels out into the Pacific, and by the time the war came along their fishery for albacore extended 2,000 miles straight out into the Pacific to the longitude of Midway Island. This high-seas fishery had come to overshadow the shore fishery in the bulk of its production. They were also catching albacore regularly in 1,200 feet of water.

We are still fishing albacore within less

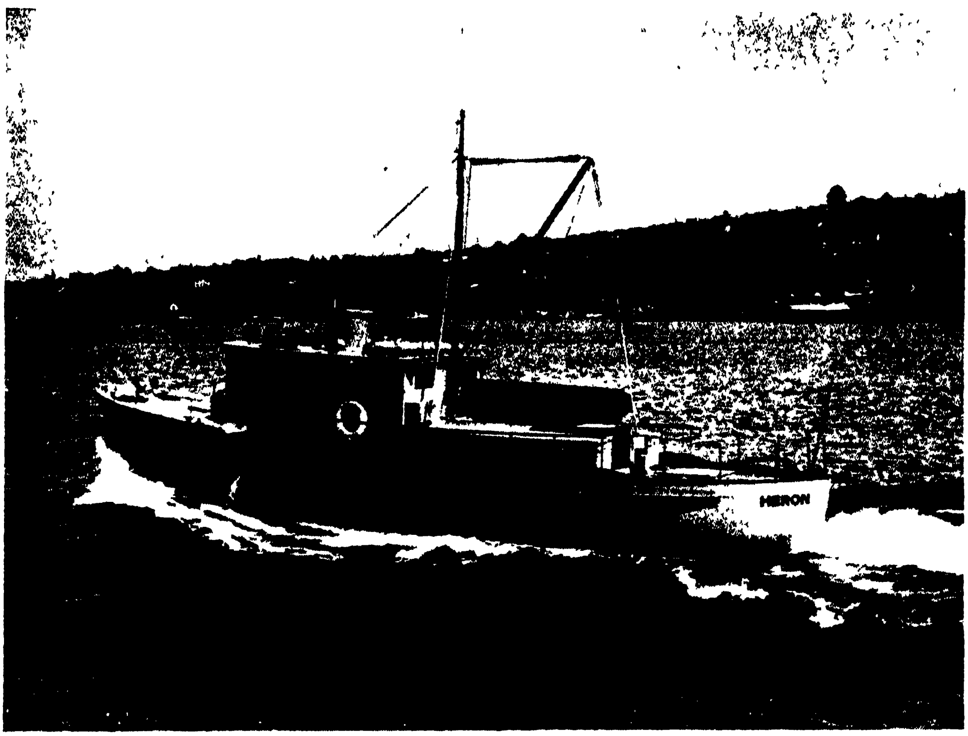


Photo by Ray Krantz. From the Pacific Fisherman

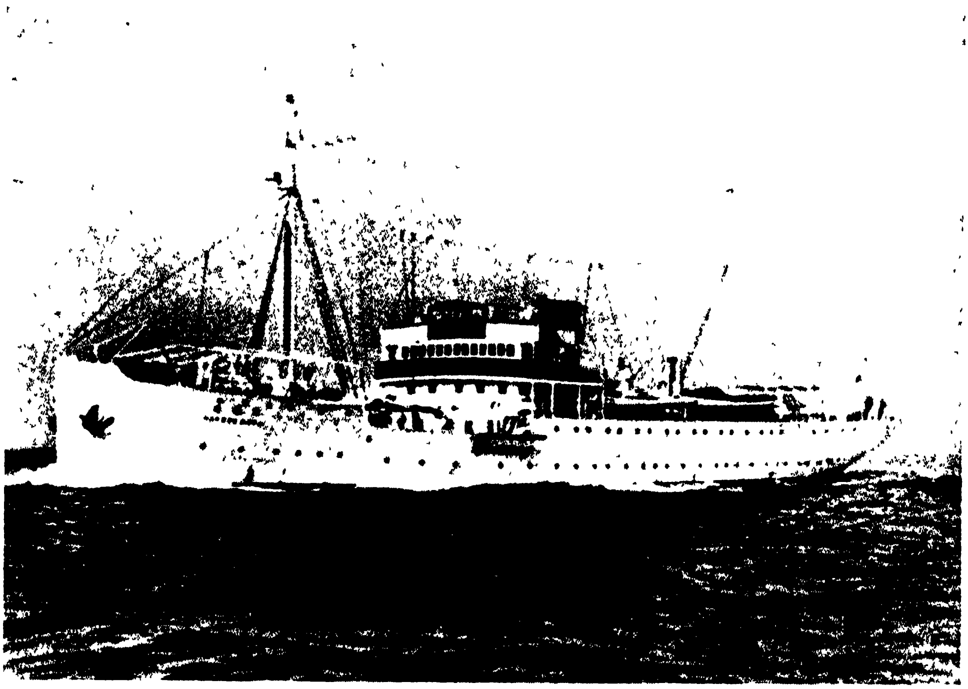
U S FISH AND WILDLIFE SERVICE VESSEL *HERON*

than 100 miles of our coast, and no attempt has been made to explore the high seas or the depths off our coast for the schools of albacore that the Japanese found off their shores.

We are still held in the grip of the theory that there can be no major quantities of harvestable produce in the middle of the ocean or in the depths because there is not enough food there to support large schools of fish. The Japanese, apparently not knowing about this lack of food, went right out and started harvesting.

What has been done by the Japanese can be done by us, but we shall have to put the same emphasis on high-seas research that they did. During this 30-year period in which the Japanese production of food from the sea has seen such a blossoming and in which they have so tremendously expanded their scientific research effort to find out about the possible productiveness of the sea, the United States Government has not had a single fisheries research vessel in the Pacific, save for one small boat in Alaska which was not large enough to get out from the shelter of the inland passage.

Our competent oceanic expeditions have been limited to *one*, which investigated the crab-productive capacity of the Bering Sea (after similar Russian and Japanese expeditions had covered the same ground). This lasted for only 2 years, but it brought back enough information about other fisheries besides crab to far more than pay the costs of the work. The only reason it was undertaken was that the Japanese had found methods to capture crab in our own waters, process them in packages attractive to the American housewife, and sell them in this country at a lower price than our own producers could meet.



From Dr J. L. Kask

PREWAR JAPANESE FISHERIES INSTITUTE VESSEL *HAKUYŌ MARU*

We can learn the secrets of the production of the sea and turn them to our own benefit. The land area of the world is small, and one day there will come a time when it will not produce the food that its people require. The Pacific Ocean occupies about 10 percent more area than all the land in the world put together, and, while the land will produce only in its upper inches, the sea will yield produce in great variety to a depth of more than 1,000 feet.

In the tropical Pacific we have won an empire of tremendous size. It is an empire of great riches, where the land is as nothing and the sea is everything—an empire in which the native people are small in numbers and restricted to small points in its vastnesses; an empire which no other nation save the Japanese covets and which no other nation save theirs and ours can cultivate and make produce.

Off our own shores lie riches in as great quantity. Once our countrymen lived from the sea; then we turned inward to cultivate the land. Now the land is rapidly being filled, and it is time that we again turn our faces to the sea, where there is space for our future development.

LONGEVITY AND CASUALTIES AMONG NATURALISTS IN TROPICAL AMERICA*

By CARLOS E. CHARDON
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THE proverbial insalubrity of tropical climates, a subject about which volumes have been written and which the average American takes for granted, does not appear to be borne out by a study of the lives of the naturalists who have wandered through the jungles of the Amazon and the Río Negro, the great llanos of Venezuela, or the *pantanals* of Paraguay in search of natural-history specimens. Many of these devoted soldiers of the great army of science have, in spite of years of hardships and vicissitudes, reached old age, and examples of octogenarians are not uncommon. Others, by far a small minority, have paid dearly for the adventure and have passed down in history as the martyrs of a noble cause.

The longevity of four of the early chroniclers whose writings included descriptions of the flora and fauna of America is most striking. First among them was Gonzalo Fernández de Oviedo (1478–1557), a Spanish soldier and a friend of the sons of Christopher Columbus. He came to America in 1514 and made 4 subsequent trips, covering Hispaniola, the Darien, and the Spanish Main. He was the author of the *Historia General y Natural de las Indias*, the first portion of which was published in Seville in 1535 and reprinted in Madrid (3 vols., 1852). The book is remarkable for the observations of Oviedo on the flora and fauna of the New World. He was evidently "a born naturalist," as Paul C. Standley calls him. He died in Valladolid, Spain, at the age of seventy-nine.

* This study was partly made possible through a grant from the John Simon Guggenheim Memorial Foundation (1945–1946).

Next is the priest Bartolomé de las Casas (1474–1566), called the Apostle of the Indians and Archbishop of Chiapas. He crossed the ocean 14 times, lived in Hispaniola, Cuba, Mexico, and other colonies. It is remarkable that he started writing his famous *Historia General de Indias* (not published until 1875–1876) when he was seventy-eight years old and finished it at eighty-seven. He died in Spain at the age of ninety-two.

The Jesuit José Acosta (1534–1600) came to America in 1571 and remained until 1587. During this time he covered most of what was then known in the New World. He was the author of the *Historia Natural y Moral de Indias* (Seville, 1590), which was translated into all the learned languages. He had, above all, the right sense of proportion, and his grasp of the universe as an integral unit led Alexander Humboldt to consider him as "the first world cosmographer." He traveled also in Italy and died in Spain at the age of sixty-six, the youngest of the four.

Garcilaso de la Vega (1539–1617) was born in Cuzco, Peru, the son of a captain in Pizarro's army and an Indian woman of noble birth. That is why he is called "Garcilaso, the Inca." He led an interesting life: he spent his youth in Cuzco, later went to Spain to become a soldier, and finally entered the church. He gathered a wealth of information about the Inca Empire from his mother and other relatives and was author of the celebrated *Comentarios Reales* (Lisbon, 1609), which contains valuable information on natural history. His last years were spent in worship, and he died a priest at seventy-nine years of age.

The average life of these four is roughly 77

years and 6 months, and it is interesting to note that none of the subsequent groups did as well as these old-timers.

HEALTH conditions in America during the seventeenth and eighteenth centuries continued to be about the same as in the preceding century. Nevertheless, the life span of naturalists continued far above expectations. The story of a dozen of them, all high-ranking, follows:

The first, George Marcgrave (1610–1648), is the exception to the rule. He was born in Saxony and received a thorough scientific training in German universities. He went to Holland and sailed for Brazil in 1638 as part of the expedition of the Count of Nassau-Siegen. He was the author of *Historia Naturalis Brasiliae* (Amsterdam, 1640), which marks the beginning of scientific work in Brazil. He was later sent to Africa and died of fever in the colony of Angola at the age of thirty-eight.

The priest Charles Plumier (1646–1704) was the first French naturalist to visit this side of the Atlantic. He made 3 trips to the West Indies and traveled extensively in Hispaniola and most of the Lesser Antilles at a time when health conditions were frightful, according to Labat. He was the author of *Description des Plantes de l'Amérique* (1693) and several other books on botany, all very remarkable for the time. At fifty-eight he died of pleurisy in Cadiz, Spain, just as he was getting ready to sail on a fourth expedition to Peru to study Cinchona trees.

Sir Hans Sloane (1660–1753) came to Jamaica as the physician of the Duke of Albemarle. He collected many specimens of plants and animals and wrote *A Voyage to the Islands of Madeira, Barbados, Nieves, S. Christophers and Jamaica, etc.* (2 vols., 1707–1725). Sloane went back to England and became a great scientist, succeeding Newton as President of the Royal Society. He died at the age of ninety-three.

THE "INCA" (1539–1617)
FIRST AMERICAN BORN NATURALIST, AUTHOR OF *Comentarios Reales* (LISBON, 1609)

Another French priest, Louis Feuillée (1660–1732), was a mathematician, physicist, and naturalist, a combination which cannot be duplicated in our modern times. He visited Chile and Peru during 1707–1713 and was the author of *Journal des Observations physiques, mathématiques et botaniques* (2 vols, Paris, 1714). He died in France at seventy-two years of age

Charles Marie de la Condamine (1701–1774) was a mathematician and naturalist sent by the French Academy to measure an arc of the meridian in the region of Quito, Ecuador, to determine the correct shape of the earth. He spent 10 years doing this and then returned to Europe by way of the Amazon River. He published *Relation abregée d'une voyage fait dans l'interieur de l'Amérique meridionale* (Paris, 1745). He died in Paris, as a result of a surgical operation, at seventy-three.

NICHOLAS JOSEPH DE JACQUIN
BORN 1727, DIED 1817 GREAT BOTANICAL EXPLORER OF THE WEST INDIES AND THE SPANISH MAIN

De la Condamine was accompanied by Joseph de Jussieu (1704-1779) as botanist of the expedition. He remained in South America for 35 years, traveling in the difficult Andean regions of Ecuador, Peru, and Bolivia, during which time he built an enormous herbarium. But fate was very cruel to de Jussieu: just as he was sailing for Europe his entire collection was stolen. Poor Joseph, brother of Antoine and Bernard Jussieu, arrived in France brokenhearted and insane, but still he managed to live until 1779; age, seventy-five years.

Jean Baptiste Christophe Aublet (1720–1778) was the French botanist and explorer who first visited the Far East. He later came to Cayenne to collect plants in 1762 and spent several years in the interior, where conditions even at the present time are most trying. He was the author of *Histoire des Plantes de la Guiane Françoise* (4 vols., Paris, 1775), a botanical classic of the eighteenth century. He died in Paris at the age of fifty-eight.

Patrick Brown (1720–1790) was born in Woodstock, Ireland. Succeeding Sloane in Jamaica, he spent several years in the colony as a physician and visited almost every parish. He was author of *The Civil and Natural History of Jamaica* (London, 1756). At seventy he died in England.

Baron Nicholas Joseph de Jacquin (1727-1817) was a native of Leiden and also a great botanical explorer. During a period of 7 years he explored most of the Lesser Antilles, Hispaniola, Cuba, and a portion of Venezuela. He was the author of *Selectarum systematica stirpium americanarum historia* (Vienna, 1763). Jacquin died a professor in the University of Vienna at the age of ninety.

José Celestino Mutis (1732–1808), a Spanish priest and the director of the Expedición Botánica established by Charles III of Spain, resided 48 years in Colombia, many of which were spent in Mariquita, a mosquito-infected region on the upper Magdalena River. Mutis was a friend of Alexander Humboldt He prepared an enormous work called *La Flora de Bogotá*, with thousands of colored plates, which unfortunately was never published. He died at the age of seventy-six.

Louis Claude Marie Richard (1754-1821), French botanist and explorer who visited the Lesser Antilles, Cayenne, and a portion of Brazil, died in France when he was sixty-seven years old.

Félix de Azara (1743–1821), a Spanish military engineer and one of the pioneers in the study of zoology in South America, spent 17 years in the interior of Paraguay, living almost alone, except for a few Indians, in the midst of the jungle. He established the boundary line between the Spanish and Portuguese colonies. Don Félix was not a trained naturalist, but he was an exceptionally good observer. He was the author of *Apuntamientos para la historia natural de los cuadrupedos del Paraguay y del Rio de la Plata* (1802) and a

similar volume on birds (1805). He must have been a great character. He died at the ripe old age of seventy-nine in his native land, Spain.

The average age of the twelve naturalists is roughly seventy years and nine months. All of them, save one, José Celestino Mutis, went back to Europe.

THE eventful trip of Alexander Humboldt and Aimé Bonpland to America (1799 1804) marks a distinct epoch in the history of scientific exploration. Humboldt's *Voyage aux regions equinoxiales* had a profound influence on Darwin and scores of others who later came to America to explore its immense natural treasures.

Alexander Humboldt (1769 1859) was born in Berlin and spent his youth in intensive study and travel in Europe, getting ready for a great overseas voyage. In his early youth he was greatly influenced by his close and intimate friend George Foster (1754–1794), who accompanied Captain Cook on his second voyage of circumnavigation of the globe and was the author of a book called *A Voyage Around the World* (London, 1777). Humboldt was a great admirer of Foster's sister, but she married another, and he remained a bachelor, though there is some indication that he had other romances, judging from a slip he made in the introduction to *Aspects of Nature* (1808). Apparently his failures in love never depressed him; on the contrary, they seemed to act as a great stimulus to his other activities. He was described, from Caracas to Mexico City, as being very jovial and resourceful—traits of Latin temperament which made him immensely popular in social circles.

He and his friend Bonpland sailed from Corunna, Spain, and landed in Cumaná, Venezuela, on July 16, 1799. Their itinerary covered vast territories of little-explored lands, some of them the worst on the continent from the standpoint of health conditions. They covered the region of Cumaná and Caracas, visited Valencia Lake and Maracay, then turned south toward the Apure through the llanos of Calabozo, later they went up the Orinoco and explored the Casiquiare, proving that this river connected the Orinoco with the Río Negro. They sailed down the great river as far as Angostura and crossed again to Nueva Barcelona; from there they took a sailboat that brought them to Cuba. After a few months in Cuba, they sailed south, landing in Cartagena, and went up the Magdalena River to visit Bogotá and see Mutis. Then they went south again as far as Quito, Cajamarca, Trujillo, and Lima. From Callao, Humboldt sailed to Guayaquil and from there to Acapulco, Mexico. He visited Mexico City, its mines, its volcanoes, and Indian antiquities, sailed from Veracruz to the United States, and from there to Bordeaux, France.

DON FÉLIX DE AZARA (1743-1821)
SPANISH MILITARY ENGINEER AND GREAT NATURALIST
OF PARAGUAY AND THE RIVER PLATE

Humboldt's and Bonpland's trip to America lasted five years. I have followed

their trail along the llanos and the Apure River and have often wondered how they ever got through. Yet they managed to do it with but two incidents: Bonpland had an attack of fever (probably malaria) on the upper Orinoco and later suffered from the same malady while going up the Magdalena River in Colombia.

The rest of Humboldt's life in Europe is well known. He became one of the world's celebrities and died a very old man in Berlin.

Aimé Bonpland (1773–1858), whose correct name was Aimé Jacques Alexandre Gougaud, was born in La Rochelle. His early love for plants so captivated his mind that his father used to call him *Bon-plant*, and he has passed on to posterity as Bonpland. He was a physician by profession but could not resist becoming a botanist. After his return to Europe from America, he was appointed superintendent of the Jardin des Plantes and became a close friend of the Empress Josephine. After she divorced Napoleon I, Bonpland was one of the few who remained loyal to her, and he was present at her deathbed. Josephine's death was a severe blow to Bonpland, and he decided to return to America. After a short stay in Argentina, he selected Paraguay, the most remote country of South America, for his future studies on plant life. All the efforts of Humboldt and the kings of France and Prussia to induce him to return to Europe failed to impress him. He spent 10 years in jail under dictator Francia, of Paraguay. Later he bought a small *rancho*, married, and had several children.

FELIPE POEY (1799–1891)

BORN IN HAVANA AND TRAINED IN FRANCE, HE WAS THE GREATEST OF THE CUBAN NATURALISTS.

Little is known about Bonpland's life in Paraguay, except that he came once to Montevideo and presided at a banquet to commemorate the fall of Sebastopol in the Crimean War. He died an old man, in a miserable hut, abandoned by his *mestizo* wife, far away from his friends and civilization.

NINETEENTH- and twentieth-century naturalists have been divided into geographical groups, and the average longevity for each group has been separately determined, as shown below:

The West Indies, including Trinidad Total, 36; average age, 65 years
Mexico and Central America Total, 29; average age, 65 years
Venezuela, the Guianas, and Colombia: Total, 20, average age, 72 years
Ecuador, Peru, and Bolivia Total, 16, average age, 68.5 years.
Brazil and Paraguay Total, 34; average age, 70 years
Grand total, 135; general average, 67.7 years

Of these naturalists, the outstanding examples of those living 80 years or more are as follows:

John Donnell Smith (1829–1928) was born in Baltimore, Md. He became an artillery captain in the Confederate Army, graduated in law from Yale, and acquired relative wealth in business. He retired in

1880 to become a botanical explorer, a profession he had long cherished. He collected plants in many states of the Union but later devoted himself to the study of the flora of Central America. He made many expeditions to Guatemala and Costa Rica, gathered an immense herbarium of 100,000 specimens, and published many papers describing hundreds of new species of plants. Smith had the poise of a distinguished Southern gentleman. Just six months before reaching the century mark he died of old age.

Felipe Poey (1799–1891) was born in Havana and trained in Paris. The founder of natural-history studies in Cuba, he became a distinguished zoologist, especially in the fields of icthyology and entomology. He was the author of *Memorias de la Historia Natural de la Isla de Cuba* (2 vols., 1851–1861) and a dozen other works. While young he contracted a disease in France which paralyzed one of his limbs. Poey lived the rest of his life in Cuba, which at that time was no paradise for a white man; yet he lived to be ninety-two.

Hermann Karsten (1817–1908) was born at Stralsund, in the Baltic. He started his training under an apothecary in his home town but later went to Berlin, where he studied medicine and natural science. In 1844 he went to Puerto Cabello, Venezuela, where yellow fever was endemic, and explored north-central Venezuela until 1847, at which time he returned to Europe. Karsten came back to America the following year, collected extensively in the Andean region, and later went to Unagua, Barcelona, Cumaná, the Gulf of Cariaco, and Cumanacoa, a region still heavily infected with malaria. Later he visited Maracaibo and Colombia and published *Florae Columbiae* (2 vols., Berlin, 1858–1869), splendidly illustrated. He died at the age of ninety-one in Germany.

Alfred Russell Wallace (1823–1913) was born in Monmouthshire, England, and was a great friend of Charles Darwin. He traveled extensively in the Amazon and Río Negro country during the period from 1848 to 1853. His brother Herbert came to America to help him but died in Pará of yellow fever. On Wallace's return trip to England, his ship caught fire. Just as the water and provisions had been exhausted the small boat in which he and some other survivors escaped was rescued near the Bahamas by the *Jordesen*, which was sailing from Havana to England. He later explored the Malay Archipelago and while stricken with fever conceived, independently of Darwin, the theory of evolution. He died of old age in England at ninety.

Theodor Peckolt (1822–1912) was born in Pechern, Silesia. He was a physician and pharmacist who came to Brazil in 1848. Peckolt traveled extensively on horseback, curing the sick and refusing remuneration. He welcomed natural-history specimens, however, and information on medicinal plants. After 20 years of exhausting work (traveling always on horseback) he settled in Rio de Janeiro and established a drugstore, but his interest in medical botany continued. He was the author of a monumental work called *Historia das plantas medicinaes e uteis do Brasil* (8 vols., Rio de Janeiro, 1888–1914). After three-quarters of a century of living in the tropics, he died of old age in Rio at ninety.

Hermann von Ihering (1850–1930) was born in Giessen, Silesia. He specialized on the Mollusca in Erlangen. In 1880 he married and came to Brazil on his honeymoon. During the next 12 years he published 77 scientific papers on various phases of zoology and anthropology. Ihering became the Director of the Museu do São Paulo, which is now called the Museu Paulista. He was also the founder of the Archelenis theory, which conceives the early continent of South America to have been made up of two separate units, "Archiguiana" and "Archiplata," separated by a

HERMANN VON IHERING

BORN 1850, DIED 1930 HE WAS A DISTINGUISHED GERMAN ZOOLOGIST AND FOR MANY YEARS WAS DIRECTOR OF THE MUSEUM OF SÃO PAULO, BRAZIL.

vast ocean now occupied by the Amazon Valley, with the Andes coming later in the Tertiary, connecting both and constituting the continent's present backbone. During the first World War he had to leave Brazil and died in Germany at the age of eighty.

William H Edwards (1822–1909), a descendant of Jonathan Edwards, was born in a small village in the Catskill Mountains. He studied law at Williams College and visited the Amazon in 1842, from Pará to Manaos. His book *A Voyage up the River Amazon* (London, 1847) had great influence in inducing Wallace, Bates, and Spruce to come to America. He later became a successful businessman in the United States: built railroads, exploited coal mines, etc. Yet he always found time to study Lepidoptera and published *Butterflies of North America* (First edition, 18 parts, 1868–1896). He lived to be eighty-seven.

Eugène L. Simon (1838–1924) was born in Paris and became one of the greatest arachnologists of all time. He traveled extensively in the following countries: Sicily, Spain, Corsica, Morocco, Algeria and Tunisia, Suez and Aden, the Philippines, Ceylon, and South Africa. He visited Venezuela during the years 1887–1888 and collected spiders, insects, and hummingbirds. He was the author of *Histoire Naturelle des Araignées* (8 fasc., Paris, 1892–1903). Simon had a wonderful memory and remembered the names of thousands of spiders, the places he collected them, and the exact literature concerning each species. He died in Paris at the age of eighty-six.

Jean Baptiste Boussingault (1802 1887) was born in Paris and became one of the founders of agricultural chemistry. Boussingault went to South America at the request of the liberator Simón Bolivar to organize a scientific institute in Santafé de Bogotá. Landing in La Guaira when he was twenty, he traveled extensively in Venezuela, Colombia, and Ecuador, under the most trying circumstances, during the years 1822 1832. Boussingault served in Bolivar's army as colonel of the staff. His *Memoires*, limited to 300 copies, was published 5 years after his death. His account of his visit to the out-of-the-way Chocó region is very exciting, for he said that in order to evade the attack of the ugly *carate* disease he sucked the milk of a young negro mother. He was later President of the French Academy and died peacefully at the age of eighty-five.

Henry Hurd Rusby (1855–1940) was Dean of the College of Pharmacy of Columbia University and a great botanical explorer of Bolivia. His book *Jungle Memories* (New York, 1933) is one of the best on travel ever published. His first visit to Bolivia was in 1887–1888, when he crossed the tableland and came down the rivers Beni, Madeira, and Amazon to Pará. His second visit was very amusing and was known as the Mulford Biological Expedi-

tion, 1921. It was a most carefully planned expedition: proper food, drugs, and medicines and vaccines against every possible ailment were provided, and every member of the expedition received detailed instructions as to what to do at the proper time. The *Entomological News* for that year published periodical releases on the progress of the expedition, but something unexpected happened. Just as he was in the midst of the jungle, Rusby developed a very severe toothache caused by an infected tooth. This triviality had not been provided for, and the director had to return to New York for treatment and operation. His companions carried on in a most gallant way: They were William M. Mann, entomologist, who is now the Director of the National Zoological Park, and Orlando E. White, botanist, now Director of the Experiment Station at Boyce, Va. Rusby soon recovered, continued his botanical work, which was both fruitful and useful, and died peacefully at eighty-five.

The other octogenarians are: William Schaus, eighty-four; Antoine Duss, eighty-four; Charles Waterton, of Guiana fame, eighty-three; Manuel Villada (Mexican), eighty-three; John Burchell, eighty-one; Frank M. Chapman, eighty-one; Everard im Thurn, eighty; Edward Palmer, eighty; Nicholas Funck, eighty; and Richard Schomburgk, eighty.

In a study of longevity among the naturalists in the American tropics it will be interesting now to make reference to the number of casualties. For the sake of convenience these may be divided into 3 categories: (a) those due to disease; (b) accidental death; and (c) suicide. Out of 135 naturalists studied, 4 belong in the first category, 3 in the second, and 2 in the third. A paragraph about each of these naturalists may be of interest.

James Orton (1830–1877) was born in Seneca Falls, N. Y., and later became Professor of Natural Science at Rochester University. He was sent by the Smithsonian Institution on an expedition through the Andes of Ecuador in 1867 and came back by way of the Amazon River. In 1869 he was appointed professor at Vassar College and published a book, *The Andes and the Amazon* (London, 1870), which was very popular In 1873 he made another expedition to South America, reversing his earlier route. A third expedition was started in 1876 to explore the Beni River, a tributary of the Madeira, in northern Bolivia. The escort sent by the Bolivian government abandoned him and his companions, and they had to make a terrible journey of 600 miles with little food until they reached La Paz. Orton was suffering from exhaustion and overexposure but continued his job in a vain effort to reach the coast. When he was crossing Lake Titicaca he died in a totora boat and was buried on one of the islands of the lake.

William Ashbrook Kellerman (1850–1908), a mycologist, was born in Ashville, Ohio. He studied at Cornell and later visited Europe and received his doctorate in Zurich. After teaching in the agricultural colleges of Wisconsin and Kansas, he went to Ohio State University, where he built a large herbarium and started editing the *Journal of Mycology* (the first periodical of its kind in America) He made 3 trips to Guatemala for the purpose of collecting fungi, especially plant rusts. On a fourth trip he was accompanied by 3 of his students. At the end of this trip, after the ascent of a high mountain, Los Amates, he complained of feeling very weak, was attacked by fever, and died during the night of March 8, 1908.

Of A. H. Fassl-Teplitz, a German explorer, little is known except that he was a first-class collector of butterflies. He made collections in Colombia during 1907 1911. In 1912 he made another trip to South America, landed in Buenos Aires, and

climbed the high Andes of Bolivia, publishing an account of this trip in Darmstadt (1920). His collections were enormous, prepared with great care, and were of exquisite beauty. He returned to the jungle and died in Manaos, in 1922, of malaria.

Erick Leonard Ekman (1885–1930) was a Swedish botanist and explorer. He came to Cuba in 1914, on a Regnell Scholarship, to collect plants for Professor Urban's *Symbolae Antillanae* He explored the island extensively from seacoast to mountaintop. Later he explored Haiti and the Dominican Republic, where he did a very thorough job. He was an eccentric man, living on almost nothing in the way of clothing and food. He always slept in the open in all kinds of weather. Under these conditions he could not live long in any climate and he died in Santiago de los Caballeros, Dominican Republic, of a high fever, which easily overcame his already weakened body.

Carlos Bertero (1789 1831) was an Italian botanical explorer. He visited Guadeloupe, St. Thomas, Puerto Rico, Haiti, Santo Domingo, and part of New Granada (Colombia). He sent large collections of plants to Europe; in 1830 he continued his botanical work in Chile and explored the Juan Fernández Islands (supposed to be *Robinson Crusoe's* setting). A year later he decided to visit Tahiti and other islands of the South Sea, but his vessel was sunk with all men on board.

Fr. Sellow (1789–1831) was a German collector who accompanied the Prince of Wied to Brazil in 1815. When the prince returned to Europe, Sellow continued his work in Brazil and made large collections of plants, especially in the San Francisco River Valley and the interior of Minas Geraes. He was drowned while bathing *à la nude* in the Río Duce ("Sweet River").

Ferdinand Nevermann (1881-1938) was born in Hamburg, Germany, and was a general biologist. Nevermann went to Costa Rica in 1909 to engage in farming but devoted himself, beginning in 1921, to the study of Coleoptera and insects in general. He sent large collections of insects to Europe and published several papers in Costa Rica. His was the toughest luck of all. On the night of June 30, 1938, while he was collecting ants in the dark, a hunter mistook his silhouette for that of a puma and shot him.

William John Burchell (1782-1863) was born in Foulsham, England. Burchell was sent in 1805 by the West India Company to the far-off island of St. Helena, where he stayed 5 years as a botanist. He had left his sweetheart in England and continued corresponding with her until he persuaded her to come and marry him. The trip was a long one, and the captain of the vessel was so intrigued by her beauty that he married her. This unexpected breach of promise greatly upset Burchell, who decided to leave for the Cape of Good Hope, where he made large zoological and botanical collections. He went to Brazil in 1825 to continue his explorations and returned to England in 1830. He lived in Foulsham for 3 decades in the midst of his collections and notes, not daring to show them to anyone for fear they might be stolen. In 1863 he committed suicide at the age of eighty-one.

Karl Ferdinand Appun (1820–1872) was a German ornithological collector who traveled extensively in Venezuela. He published a book of travel entitled *Unter den Tropen* (2 vols., Jena, 1871), which is very readable and well illustrated. He was an eccentric man, traveled very seldom in company, and had a persecution complex, so he carried always a little bottle of concentrated sulfuric acid in his pocket. One day in the midst of a Guiana forest he committed suicide by pouring the acid over his face.

It is difficult to explain such a low mortality through disease—less than 3 percent—and such longevity—an average of sixty-eight years and four months—among the

naturalists in the tropics of America. An attempt, however, may be made, considering only one of the factors involved, the neuropsychic. MacCartney's work on tropical neuropsychiatry, as reviewed by Ellis H. Hudson ("Tropical Medicine: Its Scope and Present Status." SM, January 1944, p. 47), throws some light on this dilemma:

> Strange environment, foreign language and customs, unaccustomed food, intense light and heat, tropical rains, ubiquitous and exasperating insects, all are factors which, combined with the monotony of existence, the lack of amusements, and restrictions of companionship, lead to neuroses and depressions. Least successful are the egocentric and rigid personalities lacking poise or given to idiosyncrasies and complexes. Most successful are the sanguine, the adaptable and resourceful, and the intellectually curious.

Some statements in the above paragraph are interesting and offer a possible clue. For the naturalist in the jungle, there can be no "monotony of existence, lack of amusements, or restrictions of companionship, which lead to neuroses and depressions." Burchell, the brokenhearted, and Appun, the psychopathic, are the exceptions, and both committed suicide. But no one can imagine the great Martius, or the celebrated Humboldt, or the indefatigable Natterer, or Frank Chapman, or Ducke, or a hundred others ever suffering from psychoneurosis or homesickness. Several years ago, I attended in Caracas a luncheon given by Mr. William H. Phelps to a group of scientists and I shall never forget what Dr.

CARLOS BERTERO (1789–1831)
AN ITALIAN BOTANICAL EXPLORER OF THE WEST INDIES AND CHILE. HE WAS LOST AT SEA.

Alexander Wetmore said in referring to his vicissitudes in the field: "I would not change my field experience for all the treasures of India."

Wetmore's reaction was the sincere expression of a true scientist and a great explorer. He was indeed right. To the naturalists in the field there are no worries as the rest of us think of them. The naturalists, as a group, are all "adaptable, resourceful, and intellectually curious." No wonder, in their minds, there is no room for hysteria.

A 1947 ECLIPSE EXPEDITION

SPONSORED BY THE NATIONAL GEOGRAPHIC SOCIETY AND THE U. S. ARMY AIR FORCES

By LYMAN J BRIGGS

Research Committee, National Geographic Society

ON MAY 20, 1947, the total eclipse of the sun will be visible in east-central Brazil between 9:30 A.M. and 10:00 A.M. At that time the altitude of the sun will be in excess of 40°, affording an excellent angle for observation

A group of scientists of the United States, under the sponsorship of the National Geographic Society and the U. S. Army Air Forces, has for the past year been planning an expedition to some place in the path of totality in South America to conduct a full program of studies of eclipse phenomena. Recently a reconnaissance party flew to Brazil, established cooperative relations with government officials, and selected a site near the small town of Bocayuva in the state of Minas Geraes. It has been located by preliminary observations at Latitude 17° 15′ S., Longitude 43° 42′ W., and is about 400 miles north of Rio de Janeiro on a dry plateau at an elevation of 2,300 feet. On the level area chosen, the Army Air Forces will set up a base camp for housing and feeding the civilian scientists and their own officers and men who will take part in the expedition. The important instruments and much of the equipment will be flown to the camp.

In addition to the representatives of the National Geographic Society and the Army Air Forces, the party of observers will be made up of physicists, astronomers, radio engineers, and other specialists from the National Bureau of Standards, Georgetown Observatory, the Naval Research Laboratory, Yerkes Observatory, and Lick Observatory.

Dr. C. C. Kiess, of the National Bureau of Standards, assisted by Dr. Harold Weaver, of Lick Observatory, will attack special problems in astronomy and spectroscopy. Using two high-power spectrographs provided with two of the best concave gratings ever ruled, they will make high-dispersion spectrograms of the flash spectrum of the sun and of the solar corona. The gratings are mounted so that they can cover the entire photographable range of the solar spectrum from 3,000 angstrom units in the ultraviolet to 10,000 in the infrared. When nine-tenths of the sun's disk is covered Dr. Kiess will photograph in rapid succession the diminishing crescent. These exposures will provide data for evaluating the absorption coefficient of the sun's atmosphere. Such information is important in arriving at a better understanding of the radiative processes that go on in the sun's atmosphere and in radio broadcasting on the earth. In recording solar images Dr. Kiess and Dr. Weaver will make a special effort to obtain the fainter lines in the corona.

Dr I. C. Gardner and Mr. W. L. Scott, of the Bureau of Standards, using an astrographic camera of nine-inch aperture, will photograph the corona both in black and white and in color. They will also study the polarization of coronal light with two cameras of 61-inch focal length. All three of these cameras were designed by Dr. Gardner. Because of the great range of brightness of the corona, variable density filters will be used in the large camera to black out the center part of the picture in order that an evenly exposed image of the faint portion of the corona can be made.

One of the smaller cameras will be used to determine how far out an image of the corona can be obtained. The second small camera will be operated with a polaroid filter so that successive pictures will show the state of the polarization of the corona.

Engineers from the Radio Division of the Bureau of Standards will measure the changes which take place in the ionized layers of the earth's atmosphere as the shadow approaches and passes on. Since the Division operates a series of stations throughout the world these stations will make observations at the same time that studies are being made at the eclipse camp. The observations will be used to obtain the distribution and variation of the ion density and also to provide data for computing the recombination coefficient. Information about the lower layer of the ionosphere is already fairly complete, but much less is known about the upper layer. The work, therefore, will be particularly concentrated on the latter level A special effort will be made also to find whether there are noticeable ionization effects when particular parts of the corona are eclipsed.

Professor George Van Biesbroeck, of Yerkes Observatory, will undertake accurate measurements of the Einstein shift in the apparent positions of stars whose rays pass close to the sun in reaching the earth and for that purpose is now constructing a special telescope with a focal length of about 20 feet. Since the coronal light contains a considerable amount of green, it is desirable to absorb these and shorter wave lengths by means of a filter because this coronal radiation tends to mask the images of faint stars. By using only the red end of the spectrum, it is possible to record the images of stars close to the sun's limb where the Einstein shift is the greatest. The recent development of photographic emulsions which are highly sensitive to red light makes this practicable. But in order to utilize this advantage to the fullest extent, the telescopic objective itself must be so designed as to give its sharpest definition with red light, in contrast with the general practice of constructing telescopic objectives to focus most sharply with blue light. Accordingly, a new 5-inch objective has been designed and constructed for this work at the Yerkes Observatory, corrected to give its sharpest definition with red rays. The optical glass used in this objective was made in the glass plant at the National Bureau of Standards.

Professor Van Biesbroeck is also equipping his telescope so as to superimpose upon the star field coming from around the sun a second star field that will come in at 90° to the axis of the telescope and from a direction at the same altitude as the eclipse field. It is planned to leave the instrument in place for about five months, the time necessary to bring into the evening sky the star field which will be behind the sun during the eclipse. In order to get as much information as possible about the shifts as they are affected by the refraction of the atmosphere, it is proposed to make an extended survey of temperature conditions in the atmosphere from the ground up to 20,000 feet or more at the time of the eclipse and on preceding days. This work will be done by the Army Air Forces by means of instruments carried aloft in airplanes and sent up by sounding balloons. The Army Air Forces will also provide equipment for receiving the accurate time signals needed by the civilian scientists.

Dr. Paul A. McNally and Dr. Francis J. Heyden, of Georgetown Observatory, will make a large number of photographs for the purpose of measuring with precision the time at which the moon makes its four "contacts" with the sun's disk to provide additional experimental data relating to the theory of lunar motion. They will use a special film of such fine granulation that it will give needle-sharp images, which facilitate measurement. To supplement

an existing set of very fine photographs of the northern Milky Way, Dr. McNally plans to spend several weeks before and after the eclipse seeking to obtain equally good photographs of the southern Milky Way.

Dr. E. O. Hulbert, of the U S. Naval Research Laboratory, will measure the brightness of the zenith sky at various altitudes as the shadow of the moon moves over the observation point. As a part of this work he desires to determine the molecular density and temperature of the air as high as possible. If all conditions are favorable, he should be able to get measurements of the temperature up to 60–70 kilometers.

World-wide interest is being taken in the forthcoming eclipse. Scientists from Brazil, Argentina, Australia, New Zealand, Russia, Sweden, Finland, and Denmark have announced their intention of making observations at various points in Brazil. Of particular interest are the plans of the group from Sweden, Finland, and Denmark, who propose to utilize the eclipse in a new determination of the figure of the earth. The proposal originated with Professor Bonsdorff, of Helsinki. Briefly, it consists in measuring with great precision the times at which the second and third contacts occur as observed at two stations widely separated along the center line of the path of totality. For the forthcoming eclipse it is proposed to locate one station in Brazil and the other in West Africa. From these times of contact the length of the chord of the great circle passing through the stations can be computed. If the mean error in timing the contacts does not exceed ±0.04 second, it is believed that the distance between the two stations may be determined with a mean error of ±60 meters The American expedition has volunteered to make available to this group the accurate time signals received from the Naval Observatory, the Bureau of Standards, and Greenwich.

Inasmuch as the time of contact actually extends over a period of about 6 seconds, owing to the irregularities of the surface of the moon, a question arises as to how a precision of ±0.04 second may be secured. Two methods will probably be used. The first consists of a series of photographs made at very short intervals, the exact time being known at which each exposure was made. By comparing the photographs from the two stations, some event in common, such as the disappearance of a particular Baily bead, may be recognized. The second method consists of a similar series of spectrographic records, in which the spectral lines will be crossed by lines and bands produced by the mountains of the moon. Here particular events may again be recognized on the spectrograms of the two stations, especially with the aid of densitometer measurements.

ALL OF A KIND, CAPER TO CANTO

By W. J. HUMPHREYS
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EACH of the three great rhythmic arts—the Dance, Music, Poetry—merits large space in a treatise on physiology, for each is a manifestation of that interaction between nerve and muscle which, when rhythmically repeated, induces the pleasurable feeling of mental ease and physical well-being. The most primitive of these arts and the first any of us ever practices is the dance—rhythmic movements of the whole body or any portion or portions of it: the entire body in the whirl and glide of the waltz; legs and feet in the clog; hands and arms only in certain symbolic oriental dances, and even the abdomen alone, in that most difficult, perhaps, of all dances, the *danse de ventre*. But whatever the dance, simple or intricate, solitary or with one or more partners, its characteristic feature is rhythm. If the movements are irregular and chaotic, they are not a dance.

But why the dance? Nowadays some, like the ballet girl, dance for pay; some for show; some because it is a social custom; some because of the exhilaration it affords them; some to work up and display battle fury; some to express worship; and others for this or that whatnot else. All these dances, however, are according to forms set by rules or fixed by custom; not one is truly impulsive and spontaneous, such as the dance of a child before its father's knees on his return from a long absence or the capering and cavorting of his shepherd dog, both of which joyous displays and many others like them appear to be caused by strong emotion, for surely emotion does beget motion.

And in turn the motions of the dancer beget emotions in the minds of the observers, emotions that often cause them to feel the same movements of their own muscles and even induce them to join the dance. Just how these results are effected the biochemist and the laboratory psychologist each can tell us a little in terms that make sense, the armchair psychologist can tell us all about it in mystic words that leave our confusion as badly confounded as his own.

Nor to induce motion is it necessary that the emotion be ecstatic, or even joyous, for motion is a universal and spontaneous sign language of comprehensive range, from the friendly wink of the eye to the angry shake of the fist.

Why our movements tend automatically to be rhythmic, and therefore a dance of a sort, is not entirely clear, but partly, at least, it is because such motions produce less fatigue than does an equal rate of physical exertion irregularly expended. This is true because rhythmic movements are so automatic—so habit-like—that they require practically no mental attention, just muscular exertion, whereas irregular motions demand not only muscular effort but also such close attention as to produce mental fatigue as well, a sort of weariness that is worse than just being tired. Hence, physical work of any sort is least tiresome when done rhythmically. With even strides one can walk longer and farther than he can by going hippety-hop. Hence, too, the even pulls of the oarsman alone or in time with the rest of a crew; the regular strokes of the rock driller's sledge, the even swings of the farmer's scythe; the to-and-fro flip of the weaver's shuttle, and hundreds of other kinds of work, scarcely more than pleasantly tiring when

done rhythmically, are fatiguing beyond endurance when the necessary movements are so irregular as to demand constant close attention. This is one reason why the beginner at any one of many sorts of manual labor tires so quickly—he doesn't have the "swing" of it.

Similarly a series of sounds rhythmically repeated is far more pleasing to the ear than a succession of notes that go on and on without any semblance of order or repetition. We catch the swing of the thing and feel the mental ease it affords. At each note we sense what is coming next and experience no mental strain about it—even feeling, perhaps, simultaneous muscular reactions, however still we outwardly may seem to be. This is music, a succession of sounds rhythmically ordered. Whether any given piece of such music is liked by a particular individual depends upon his personal taste, which in turn depends largely upon what he has been accustomed to hearing. Chinese singing, which may put an American prima donna's teeth on edge, is loved by the millions who grew up with it; hers, which would give a mandarin the jimjams, we pay much to hear. The skirling bagpipe, however much disliked by most people, thrills the Scotchman beyond measure. The whining musette is far from pleasing to every ear, but Shriners proudly follow its lead as they go marching on; and so it is even with the tom-tom, whose insistent beat, annoying at first, finally will get you if you don't watch out.

In all these widely different sorts of music there is one quality, the least common denominator of all that is recognized by anyone as music, namely, rhythm. Of course there is much beside rhythm in most music, but rhythm, at least, it must have.

How, then, did music get started? That is another story and one of which we have neither history nor even tradition. Surely it first was vocal and possibly incident to rhythmic labor like, say, the swinging of a sledge hammer or the pulling of an oar. Here at the end of each heavy blow, or strong pull, the pent-up breath causes, as it suddenly escapes, an involuntary grunt that may be, and often is, in the form of a short explosive word or, when the work is not too strenuous, two or three words, the first being the start and the last the explosion end of the grunt, just as we often hear them today under such circumstances. Eventually this one- or two-word grunt was lengthened, as it now frequently is, especially by a slow worker, until it covered the whole interval from start to end of each strenuous effort, over and over again—vocal sounds of whatever easy sort set in rhythmic order. Vocal music this is, easy to imitate, with endless variations, even without the stimulus of regularly repeated physical exertion, though most likely and best sung, perhaps, when there is a recollection of such work in the singer's mind.

With this vocal start, instrumental music, however varied and elaborate, is but a sequel, and through it all the basic element is rhythm. And the same element, rhythm, pertains also to all real poetry, whatever the language. True, in much printed material that looks like poems it does not exist at all, but these are poems without poetry.

Caper to canto, then, the Dance, Music, Poetry, all is restful rhythm, producing a minimum of nervous fatigue for a given amount of muscular exertion, actual or fancied—actual in the dance, fancied in poetry. Each concerns the interplay between nerve and muscle and therefore is a subject of prime importance to the physiologist. The dance, music, and poetry all please us, but it is for the physiologist to tell us why they do.

A BIOPHYSICS SYMPOSIUM*

THE PARTICLE PHYSICS APPROACH TO BIOLOGY

By RAYMOND E. ZIRKLE

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SINCE I know nothing of philosophy and very little of formal logic, my only service in a symposium of this sort is that of a specimen or exhibit. My role, as I see it, is to provide for discussion an exhibition of the typical naive biologist who thinks about the structures and functions of living things in terms of particles and tries to interpret these structures and functions in terms of the physical properties of the particles.

Particle physics contributes to biology in two ways. First, it contributes points of view. The biologist who uses the approach of particle physics thinks of the ultimate structures and functions of living things in terms of the physical particles, their aggregates, and their interactions with each other. We try to explain all the activities of organisms in terms of the activities of the ultimate particles. Second, particle physics provides powerful techniques for the investigation of biological structure and function. This, perhaps, is inevitable if one bases his biological concepts on the particle point of view.

Let us consider the manner in which our naive biologist regards the structure and activities of an organism in terms of the properties of the ultimate physical particles.

The ultimate particles currently well recognized in physics are the positron, the electron, the proton, and the neutron. So far as we now know, the positron is inclined to evanescence and rarely figures in biological phenomena; we shall therefore ignore it in this discussion. The remaining three particles, however, are directly or indirectly of extreme importance. The electron has extremely small mass and a negative electric charge. The proton has a positive electric charge equal in magnitude to that of the electron, and its mass is some 1,700 times as great as that of the electron. The neutron is electrically neutral and has a mass very close to that of the proton. All these particles have magnetic properties, and the electron and the proton, being electrically charged, give rise to electric fields even when at rest.

According to current atomic models, the nuclei of atoms consist of protons and neutrons and therefore have net positive charges. The relatively vast extranuclear volume of an atom is occupied by a cloud of electrons, each of which occupies an "orbit" which is mathematically describable. In an electrically neutral atom, the number of orbital electrons is equal to the number of protons in the nucleus; this number is known as the atomic number and identifies the chemical element to which the atom belongs. The number of neutrons in the nucleus, for a given number of protons, is somewhat variable but not indefinitely so, because too great variation leads to instability of the nucleus. This means that atoms of the same element may have different masses; the various kinds of atom of the same element are called isotopes. We shall return to these later.

Since the orbital electrons, and particu-

* This article and the two following ones by Drs. Burr and Margenau are based on papers presented at a symposium on The Philosophy of Biophysics at the annual meeting of the A.A.A.S., St. Louis, April 1946, in a joint session of Section L and the American Philosophical Association.

larly those in the outermost orbit, are the portions of the atom which are closest to neighboring atoms, these electrons are the particles whose intrinsic properties and arrangement chiefly determine the behavior of that atom toward its neighbors. This behavior is collectively what we know as chemistry, and if we are studying the behavior of an atom in an organism, we are studying biochemistry. Since the configuration of the orbital electrons is chiefly determined by their number and by the properties of the electrons themselves, and since the number of electrons in an atom is dependent upon the number of protons in the nucleus, we can see that chemistry, including biochemistry, is dependent upon the ultimate particles of which atoms are built.

Because of the configuration of the orbital electrons, each species of atom has a characteristic behavior toward other atoms, and in particular it may enter into atomic aggregates which we mostly call molecules. The number of species of possible aggregates is large, as might be expected, and in biochemistry we encounter an extremely large number of certain of these molecular species. These atomic aggregates, or molecules, have certain means of reacting with others, and these potentialities we consider to be determined by the properties of the constituent atoms, which in turn are determined by the properties of the ultimate subatomic particles.

Our naive biologist will go even further and visualize a microscopic biological cell as consisting of molecules, ions, and colloidal micelles (aggregates of atoms or molecules), all arranged, both spatially and temporally, in ways determined by the properties of the ultimate physical particles in the cell and in its environment. And he will go even further and regard a complex organism essentially as an orderly aggregate of cells and their products. This complex organism may have certain macroscopic properties, such as gross anatomy, symmetry, and gradients of various sorts, which can be studied very profitably without reference to particle physics, but the particle biologist regards all these properties as derivatives of particle properties.

We have just seen that the concepts of particle physics enable the biologist to think about biological phenomena in potentially very simple terms. This is one of the great advantages of taking the particle viewpoint. However, this advantage in viewpoint is at least matched, and possibly overshadowed, by the wealth of techniques which particle physics furnishes for attack on biological problems. For instance, consider the various devices made possible by taking advantage of the properties of the electron: the vacuum tube, the X-ray outfit, the electron microscope, and the electron diffraction apparatus. One could extend the list of particle techniques almost indefinitely, but, further to exemplify the naive biologist, I shall explain in somewhat greater detail some of the particle techniques with which I happen to be most familiar.

First, let us consider the application of ionizing particles in biology. These ionizing particles are either electrons or atomic nuclei of various sorts. These fast charged particles may be primary radiations, or they may be set in motion by means of other radiations such as X-rays, gamma-rays, and neutrons. Under certain conditions, very substantial beams, either of electrons or of light atomic nuclei, may be produced artificially by means of the so-called atom-smashers, such as cyclotrons, Van de Graaff generators, etc.

These ionizing particles have been used in basic and applied biology in various ways. For instance, for two decades, they have been used in genetics to produce mutations. Here the fast ionizing particle presumably produces some alteration in the

particle make-up of the gene or of some grosser structure such as a chromosome or chromatid. The geneticist usually is not very curious about the exact nature of the physical action of the fast particle upon the genetic make-up of the cell, but he has a very intense interest in the end result—namely, the genetic effect. Essentially, he modifies the intimate mechanism of heredity by bombardment with ionizing particles; analysis of the modifications yields information of basic interest to genetics. We have here an interesting parallel to some of the methods of particle physicists, who have been notably successful in getting information about particles and particle aggregates by drastic bombardments of various sorts.

Further, there has been a wide application of ionizing particles in medicine, where usually they are produced at considerable depths in a large organism by making use of some primary radiation, such as X- or gamma-rays, which ejects fast electrons from atoms in the irradiated body. Long experience has taught radiobiologists and radiotherapists that certain types of cells and tissues can be inhibited in certain of their functions by smaller amounts of these ionizing particles than are other cells and tissues; and, in cases where the cells or tissues which one wishes to destroy happen to be more radiosensitive than those which must not be destroyed, the medical use of the ionizing particles is termed successful.

In practically all biological applications of ionizing particles to date, including the two examples just cited, it has been technically expedient to irradiate at least a large portion of the organism and all the cells within that gross portion. The primary X- or gamma-rays eject the fast electrons at random as they traverse the irradiated body. And the ejected electrons then travel tortuous paths more or less at random through the adjacent tissue. Much more could be gained in the way of fundamental knowledge of the action of the ionizing particles, and much more effective applications in basic biology could be attained, if we had some device for limiting the paths of the ionizing particles to some relatively small fraction of the cell under study, preferably a known fraction such as the end of a chromosome whose genetic make-up is reasonably well known. To date, studies of this type have been rare, crude, and largely dependent upon structural peculiarities of the cells investigated. The reasons for this lack of exploitation have been technical; it has been difficult to localize the tracks of the ionizing particles. However, the advent of the cyclotron and other sources of fast charged atomic nuclei, such as protons, deuterons, and artificial alpha particles, now make it possible, if one wishes to go to sufficient expense and pains, to limit a parallel beam of particles to a relatively small fraction of a cell of average size. The analytical advantages of such a procedure are obvious.

A SECOND and relatively new application of particle physics is the technique of isotope tracing. This is currently much discussed, and justifiably so, because of its power and its wide applicability in biology.

The principle of the technique has its basis in the ultimate particulate constitution of the atomic nucleus. Disregarding one or two exceptional cases, all the various isotopes of any given element are practically identical in their chemical behavior because their orbital electron configurations are similar. The various isotopes of any element, however, contain different numbers of neutrons in the nuclei. This difference in neutron number makes possible various methods of analysis for the different isotopes. Some of the methods are based simply on the differences in

nuclear mass due to the differences in number of neutrons. The mass spectrograph is perhaps the commonest instrument which works on this general principle. Other methods are based on the fact that some isotopes, because of extreme ratios of nuclear neutrons to nuclear protons, are unstable and therefore exhibit radioactivity, a property which is of course quite distinctive and measurable in absolutely unweighable amounts of material.

Let me briefly illustrate the simplicity, power, and importance of isotope tracing by consideration of a specific example: the physiological function of carbon dioxide in animal metabolism. Prior to the availability of tracer techniques involving the isotopes of carbon, carbon dioxide was universally regarded merely as a final breakdown product of animal biochemistry. This point of view was due to the observation that carbon dioxide was one of the net products of the chemistry of animal cells. In view of this fact and of the further fact that carbon dioxide is universally present in mammalian cells, it was impossible by conventional methods to detect any other role of carbon dioxide, because if a new sample of carbon dioxide were presented to the cells, there was no way of distinguishing a molecule of the new sample from the molecules already present. However, with the advent of artificial radioactivity, a new and radioactive isotope of carbon (C^{11} in distinction to the natural isotopes C^{12} and C^{13}) was made available. A sample of carbon dioxide, some of whose carbon atoms were C^{11}, could then be presented to animal tissues. Since no C^{11} was already present in the tissue, this procedure amounted to "tagging" the carbon atoms in the sample. The result, surprisingly enough, was that some of the radioactive tagged atoms were later found in some of the organic substances isolated from the cells. This showed very straightforwardly that carbon dioxide is not only the end product of some of the reactions of animal chemistry, but that it is actually one of the raw materials. The complexity of the animal metabolism of carbon dioxide was thus demonstrated to be far greater than anyone had realized, and, as a matter of fact, it is only now being unraveled in a substantial way. All this unraveling, needless to say, involves the use of isotopic tracing of carbon in conjunction with the more conventional techniques of biochemistry.

I have chosen the foregoing example not only because of its biological importance, but because of its simplicity. The uses of the isotopic tracers naturally are considerably diversified and have important advantages additional to those brought out in this example. However, I believe this case gives a glimpse of the types of analysis that are made possible by the isotopes and that are impossible without them.

In addition to the radioactive isotopes, certain rare natural isotopes can be used as tracers if samples enriched in these isotopes are made available. The techniques of analysis are, of course, different from those used with radioactive isotopes, but the essential principles and advantages involved in their use are the same. Some of these stable isotopes, such as N^{15} and H^{2}, have already been used with great success in investigations of the biochemistry of these elements.

For purposes of this discussion, I should like to emphasize that tracer techniques, in both their mental and their instrumental aspects, would be hard to imagine without a foundation of particle physics.

In brief conclusion, I should like to affirm my own attitude toward the particle physics approach to biology as follows. First, it is by no means the only profitable or interesting approach to problems of life. Second, however, it provides concepts of

biological phenomena—and techniques for investigating them—which are of great power and which have demonstrated their value in scientific and practical achievement. Third, it provides fascinating and ever-changing fields in which to spend a lifetime, especially in these days of dynamic expansion of nuclear physics.

One of the best publicized despisers of boredom was Ulysses, into whose mouth Lord Tennyson put the following words:

> All experience is an arch wherethro'
> Gleams that untravell'd world, whose margin fades
> For ever and for ever when I move.

I feel sure that particle physics in these times would provide the ever-changing scene craved even by Ulysses.

FIELD THEORY IN BIOLOGY

By H. S. BURR
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THE study of the development of the central nervous system, using the method of experimental embryology, has done little but confirm the well-known fact that in the brain and spinal cord, with their peripheral connections, is an extraordinarily complex organ system. This very complexity has made the study of the nervous system a fascinating exercise. Much has been learned of the elements which go to make up the intricate arrangements of the parts of the nervous system, but our knowledge is still incomplete. The experimentalists have examined the processes by which development has proceeded from relatively simple primordia to highly organized components. Specific potencies of particular cells have been recognized, and processes by means of which the potencies are realized have been described as organizators, inductors, and the like.

Out of this welter of experimental facts, there has emerged a reaffirmation of the fact, known to all neurologists, that the nervous system is the result of an extraordinarily meticulous and precise design. No matter what part of the nervous system is examined, the preciseness of the design is everywhere evident. Masses of neurone cell bodies are seen to be located in specific regions of the nervous system with great exactness. The nerve fiber processes of these cell bodies connect these nuclear masses over rigidly determined pathways. Variations in these structural relations are virtually unknown, save possibly in some of the minute relationships.

The modern neurologist, standing on the shoulders of many generations of careful investigators, has tended to assume that the design of the nervous system is so well known that, except for minor additions, there is little more to be learned. On the other hand, while the final design seems to be fairly well recognized, there is little information in the literature as to the nature of the forces by means of which this design is realized. A few students have been aware that the design of the nervous system is but a special case of design in the living organism. Many have felt that the solution of the problem of organization in all living things would eventually supply an

answer to the origin of design in the nervous system. The history of biology shows that the few attempts made to solve the problem have been unsuccessful. Scientists have been loath to accept the "final cause" of Aristotle, the entelechy of Driesch, or the *élan vital* of Bergson, for these represent qualitative descriptions of events and not objectively measured correlates of them. They have been dismissed as unscientific. Somewhat better were the embryonic fields of the German experimentalists, notably Spemann, or the physiological gradients of Child. These latter also are unsatisfactory, for the nature of the forces involved is not explicit. Many modern experimentalists tend to rely on tropisms, either mechanical or chemical. It is not surprising, therefore, that many years of study of the nervous system led to the conclusion that the problem must be attacked in a new way. The design of the nervous system is extraordinarily precise. Therefore, the forces which make for that precision must be exceedingly powerful. Moreover, the forces themselves must show interrelationships which control not only local events but also the unfolding of the whole pattern. If this problem is faced squarely the question arises, What kind of forces can be imagined that are capable of bringing about these obvious results? The traditional forces are manifestly inadequate. It would seem, therefore, worth while to turn to a different approach. Design is not unique to living systems but is to be found everywhere in the universe. It is not unreasonable to suppose, therefore, that the same forces which impart design to the material universe might also perform the same function in the living organism.

At this point there appear two different approaches. Many investigators believe that the design of the whole system can be deduced from the complete description of all the entities of which the system is composed. This is the attitude of most biologists who believe that, once the living system is analyzed into its constituent chemicals, the design of the living organism will follow as a consequence. The second point of view, however, maintains that there are relationships between entities not completely derivable from the nature of the entities themselves. It is these relational forces which, in the last analysis, control the directions in which activities move and which, therefore, impart pattern to the arrangement of the entities.

Both of these points of view are implicit in much biological research. The first has, however, dominated the picture to the almost complete exclusion of the second. This is probably due in large part to the fact that the second approach has led largely to the development of unprovable theories. It is at this point that biologists can probably turn to the physicists, for physics tells us that there are in the universe three sets of forces—gravitational, electromagnetic, and nuclear.

Of these, gravitation, although it has its influence on growing things, is not strong enough to establish pattern, and nuclear forces, probably the most powerful known, operate over such exceedingly short distances as to be largely irrelevant to the problem of organization. There remain, then, electromagnetic forces as the determiners of design in the universe. In general, all three sets of forces can be conceived of as definable and measurable aspects of a primary field property of the universe. As such, they lend themselves to experimental analysis and verification.

ELECTROMAGNETIC forces have been known since the investigations of Faraday demonstrated their existence. The mathematical expansion of the field aspects of these experiments, through the analyses of Maxwell, Larmor, and Lorentz, has led to the identification of matter with electricity. The electromagnetic field, in other words,

possesses a vector property which not only establishes the position of all charges within its range, but also gives direction to the movements of the charges. This field, then, has the attributes necessary to establish pattern or design in matter. The importance of the concept has been amply attested by the technological developments of the past hundred years.

Physics, then, has provided a clue to the nature of the forces which impose design on the material universe. But living organisms are also a part of the universe. Unless they are conceived of as special creations outside of the laws governing the rest of the cosmos, subject to the controls of an entelechy, or an *élan vital*, they also must be controlled by electromagnetic fields. Moreover, living beings are made up of the same atoms or charges as are found in nonliving matter. There are no reasons to believe that entities common to both worlds are acted on by different forces. The chemistry of living organisms, for example, differs in no essential way from the chemistry of inert matter, save possibly that the problem is more complex. Moreover, the forces involved in chemical reactions are fundamentally electrical, basically the expression of electromagnetic fields.

It is clear, then, that the heart of the problem of the biologist, the forces which impose design on the living organism in general and the nervous system in particular, is identical with that of the physicist who is faced with the origin of design in the material universe. The solution provided by physics, therefore, should be applicable in biology.

If the above position is accepted, a number of conditions must be met. In the first place, a one-to-one correspondence between the pattern of the electromagnetic fields and the organic form of the living organism should exist. For example, one of the characteristics of living things is symmetry, either radial or bilateral. To be sure, this property is not universal. There are forms of life without it. Nevertheless, it is widely found. The proposal made above would demand a significant, measurable relationship between field and symmetry. There should be a correspondence between an axis of the field and that longitudinal axis which is characteristic of the nervous system of the vertebrate.

In the second place, some method of measuring the electromagnetic fields must be available. In the physical world one property of such fields may be determined by establishing points between which there are steady-state potential differences. Such information does not, of course, identify the sources and sinks of the field, but it does provide an important measure of the relationships between the charges. Knowing the sources and sinks, Maxwell's equations make it possible to define the field and its potential gradients. Knowing the potential gradients, the field can be described without giving specific knowledge of the location of the sources and sinks.

Finally, there must be available a technique by means of which true potential differences can be determined. This condition must be attained through the design of a measuring device that is relatively independent of changing resistance and therefore of changing current flow. Only thus can separation of the dependent variables of Ohm's law be achieved. Only thus can an important measure of electromagnetic fields be made accurately.

But can these conditions be met in the living world? Fortunately, they can, for it has been known for more than 100 years that living systems possess many electrical attributes. That these electrical phenomena are important has been adequately attested by the development of the electroencephalograph, the electrocardiograph, and many electrometric techniques applied to biological problems. Furthermore, there

have been many scattered evidences of the importance of these electrical manifestations in other aspects of the living system. Witness particularly the brilliant pioneer studies of Lund on some of the vital processes of such simple organisms as Obelia and such complex mechanisms as the Douglas fir.

So many signs of electrical activity have been found and utilized that it ought to be possible to relate them to each other under a single assumption. This can be accomplished, first, through adequate theory; second, through logical consequences of that theory; and, third, through techniques for testing the predictions in the laboratory.

Let us assume that electrical phenomena are constant concomitants of the living organism. It should then be possible to determine the existence of potential differences. By definition, such potential differences constitute a field. Can it then be shown that the pattern of these potential differences is correlated with the organic pattern of the living system?

What does this mean? It means, as Northrop and I have suggested, that four questions must be put to nature:

1. Do steady-state potential differences exist in living things?
2. Are they chaotic or do they exhibit a pattern?
3. Does the pattern of the electromagnetic field so measured correlate with the pattern of organic form?
4. And, finally, does the field so determined control the design of the living system in the same way that such fields control design in the material world?

It is clear from the above that the following assumption can be made. Electromagnetic fields determine the pattern of organization in biological systems. The logical consequences of such an assumption demand that these fields be measured with certainty, that they exhibit patterns which correlate with patterns of organic form, and that they maintain a certain constancy throughout the complex flux of chemical reactions accompanying growth and the living process. Local short-time changes in the field might be expected to accompany biological activity of various kinds and therefore yield valuable data about specific biological events. It seems to us, therefore, on the basis of all the evidence obtainable, that the application of the field theory of physics to the basic biological problem might yield interesting and significant results.

The past 15 years have been spent in investigations of the possibilities above-stated. At the outset the development of an improved and simplified technique was necessary. With the help of Dr. Cecil Lane, of the Physics Department of Yale University, and of Dr. L. F. Nims, a physical chemist, in the Department of Physiology, a procedure was evolved which has made it possible for us to answer many questions. Contact with a living system is made through nonpolarizing reversible silver-silver chloride electrodes in salt solution in approximate ionic equilibrium with the solutions of the system being measured. The difference in potential between any two points with which the electrodes were in contact was measured by a vacuum tube bridge, the input impedance of which was 10 megohms. The high input impedance made the measurement of voltage difference independent of resistance change. With this instrument it has been possible to measure potential differences in the microvolt range. The instrument is sufficiently free from drift so that continuous measurements with suitable apparatus can be made over a long period of time. Finally, the procedure has made it possible to study a living system without interfering with its integrity. A very large part of experimental biological research has involved cutting living systems into ever smaller pieces. This destroys at the outset one of the unique properties of living

organisms, their unity. It would take too long to present here all the evidence which has accumulated, but a few of the more important aspects may be interesting.

At the outset, it was obvious that there were in general two types of experiments that could be performed. Since the technique made it possible to measure biological systems from a somewhat new standpoint, a great many of the observations were exploratory. The selection of the particular experiment to be performed was largely determined by circumstances. This resulted in studies of many living systems, ranging from bacteria to man. Interesting and important as these seem to be, the more important objective was the relationship between field theory as measured by standing potentials and fundamental biological problems, particularly the problem of biological organization. However, every attempt was made to plan the experiments so that they might give an answer to the four basic questions mentioned above.

The first of these asks if these standing potentials are to be found everywhere in living systems. The answer is, unequivocally, Yes. In every experiment in which measurements were made on living systems in both plants and animals, reproducible standing potentials were found. They were not invariant, but the variation was nearly always within narrow limits. This is not surprising since a living system is a dynamic mechanism in constant activity, and, hence, some degree of variability might be expected. Nevertheless, in general, the measurements were astonishingly constant.

With regard to the second question—Is there pattern in the potential differences?—the evidence was no less clear. Characteristic patterns of potential differences were found in most of the laboratory animals and in man. In the latter instance, the right side of the body was almost uniformly positive to the left side by several millivolts. In something less than 10 percent of the instances this polarity was reversed. Moreover, in any one individual, potentials measured from day to day, over many weeks and months, showed a definite tendency to remain within very restricted limits. Since the technique makes it possible to study the individual without interfering with normal functioning, many artifacts which might be expected to accompany fragmentation of the living system were avoided. Incidentally, the positive polarity of the right side of the body bears no discernible relation to handedness.

Differences between the sexes were also noted. Early in the study it was observed that there were marked increases in the potential difference between the right and left side of the body in women, recurring at approximately monthly intervals. This observation led to a more precise analysis of these potentials, and evidence was collected on both animals and women pointing to the probability that this significant change in standing potential was associated with the phenomenon of ovulation. This was an interesting and unexpected consequence of the theory which still requires additional investigation.

Turning now to more fundamental aspects of the consequences of the theory, a characteristic of living systems is their capacity for growth. It was necessary, therefore, to determine whether or not these standing potentials change during the growing process. Corn seed, Obelia, salamanders, and mice were explored, and everywhere it was found that during the process of growth and development the standing potentials steadily increased in magnitude to an asymptote. The pattern of these potentials, however, showed only minor variations. In the salamander egg, for example, potential differences between

any two parts appear, but with low magnitudes. During the early stages of cell division and the formation of the blastula and gastrula, there is a steady rise in potential, accelerating with time. This continues with increasing rapidity until the free-swimming larval stage is reached, when potential differences along the long axis of the larva reach a value of 6 or 7 millivolts. The studies on mice reveal that a similar rise in potential occurred in the first third of the animal's life, leveled off during the middle third, and tended to decrease during the last third. During the phases of early differentiation in corn roots and in the developing salamander egg, considerable variability in the stability of the potentials was noted. This suggests that the process of differentiation by itself involves a good deal of biological activity as reflected in potential measurements. It was during these studies of growth that the first real evidence of the existence of field properties was noted.

In the experiments on aquatic animals the technique was so arranged that, once the electrodes were in contact with the animal, the stage on which the animal was placed could be lowered away from the electrodes. The animal was then reversed end for end and raised in contact with the electrodes. This gave a check on the validity of the measurement. Quite promptly it was noted that instead of the potential difference dropping to zero the moment the animals lost contact with the electrodes, a voltage difference could still be read, even though 1.5 to 2 millimeters marked the space between the electrodes and the animal. This means, of course, that there must have been voltage differences in the surrounding solution. Such potential differences are not present when the animal is removed from its aquatic environment. The rate of decline of the potential difference, as the distance increases, is an exponential one. The presence of this phenomenon could be due only to the existence in the living system of a field. At first glance it is somewhat extraordinary that an animal in fresh water or in salt solution can maintain this potential without exhaustion. In spite of the presence of an external shunt, the voltage is not only maintained but increases with time.

At one stage in the development of the salamander embryo, actively beating cilia keep the animal in constant rotation within the gelatinous capsule. If electrodes are forced through the capsule, so that they approach the surface of the contained embryo, regular and rhythmic reversals of potential difference can be recognized as the embryo revolves. Because the potentials at this stage are low, an experiment was performed and photographed in which an older animal was mechanically revolved beneath two electrodes. The potential swings were recorded on an ink-writing galvanometer. Under these conditions the revolving salamander produced an electrical output of alternating character very similar to the output of an ordinary generator, save only in magnitude and frequency. Again this phenomenon could not have been recorded if there had not been in the animal a characteristic field.

The third question was, Is there a correspondence between the pattern of potential differences and organic form? The answer to this problem was sought through a study of the electrical characteristics of embryos of both animals and plants. Again the results indicated a close relationship between the measured potential pattern and the developing organism.

The single-cell egg of the frog and the salamander were explored electrically. Structurally these eggs are radially symmetrical systems, but electrically they are not. Measurements made between the upper pole of the egg and four equally placed points on the equator showed that invariably there was one point which

showed a greater voltage drop than any of the others. This point could be marked and the subsequent development of the egg watched. In every instance, the head end of the embryo developed at the point of greatest potential difference. This electrical asymmetry was present before the egg was fertilized and, so far as the present studies go, showed no change following fertilization. Here, then, is evidence of the existence of an electrical pattern, present before the appearance of any formed structures, carrying on through fertilization and cell division up to the formation of a definite nervous system. The axis of the embryo coincides with the axis of the electrical field. There is, in other words, an electrical pattern, present through the early phases of development in spite of innumerable chemical reactions and definitely related to the pattern of organization of the nervous system. The constancy of this field pattern raises innumerable problems yet to be solved. Since the electrical pattern is present before and after fertilization, there is indicated a positive relationship of the field to the design of the living system and to its genetic constitution.

BECAUSE the problem of form in plants is somewhat simpler than it is in animals, a study was made of the correlation between electrical patterns and form in plant embryos. Cucurbits were chosen because the important studies of Sinnott have provided a structural basis for an examination of any such relationships. Three inbred lines of *Cucurbita pepo* were used. One developed elongate fruits, one round, and one flat. Measurements of the potential differences were made along the axial and equatorial diameters of young ovaries and developing fruits of these three races differing markedly in shape. It was found that the magnitude of the potential differences bears little relationship to the absolute size of the embryo, but there was a high positive correlation between the ratios of the electrical differences and the ratio of the dimensions. Here, then, is another evidence of a significant relationship between electrical pattern and form.

The studies of the electrical properties of plant embryos were sufficiently successful to warrant the extension of the measurements to plant seeds. For, if this field is as fundamental as we have assumed, it would follow logically that there would be significant correlations of electrical patterns with aspects of the living system other than form. Furthermore, since the earlier studies on growth had shown a steadily increasing potential during the growing period, it was thought that there might be a significant relationship between the initial measured potential and the growth capacity of the seed. There have been innumerable studies on corn with a good deal of evidence regarding growth rates and their relationship to genetic background. There was, therefore, a possibility of the discovery of further relationships between potential and genetic constitution. Exploration of these problems was attempted with the cooperation of W. R. Singleton, of the Connecticut Agricultural Experiment Station. The corn seed used were from strains which had been under study for some time. These differed considerably in genetic constitution and more particularly in the degree of hybrid vigor resulting from crosses between them. Four inbred strains were used and three hybrids. The potential measurements were made between the germinal and the micropylar ends of the seed. A statistical analysis of the results showed significant differences in the mean potentials obtained from each strain. One of the strains was a single gene mutant from the parent stock; yet the potential in the parent stock was more than four times that of the single gene mutant. It is surprising that a single gene change should be correlated with such a large change in

potential. The potential differences in the hybrids showed a gradation closely correlated with the measurements of hybrid vigor determined by field studies. One of the hybrids known as "Marcross" is an outstanding early season hybrid. The seed from this strain had the highest potentials of any of the hybrids. Likewise, in the pure strains there was a positive correlation between the results of field studies on yields and the measured standing potentials of the seed. In other words, the potential differences measured were such as to make possible a prediction of the subsequent history of the seed when planted in the field. These studies make it clear that there is a significant relationship between the measurable electrical properties of corn and the genetic constitution of the seed, as well as its hybrid vigor. Here we have the rather extraordinary finding that accurate potential measurements make it possible to predict the subsequent growth history of individual seeds.

Since the above study was dependent in large measure on field data collected before the potential differences were determined, a large number of seeds were measured, placing them in high, medium, and low potential groups. These were then planted under controlled conditions in the field, and their growth and yield studied. Oliver E. Nelson, Jr., Eastern States Fellow at the Connecticut Agricultural Experiment Station, realizing the possibilities inherent in such a study, agreed to carry out the field experiments. During the past three years many measurements were made on corn and the seed allocated to the three groups of potential differences. Each group was then planted in the field under controlled conditions, and at weekly intervals a variety of growth measurements were made. The first summer was, unfortunately, unseasonably dry, with consequent interference with the growth processes in the plants. In spite of the adverse conditions, the high-potential seed developed plants which grew faster, reaching an ultimately greater height, and produced heavier ears than did the low-potential seed. With only one exception, the medium-potential seed produced plants intermediate between the two extremes. The seed from these plants were collected, replanted the second summer, and yielded the same results in more striking form. Seed from these plants were then measured, and the plants from high-potential seed were found to have developed seed with high potential.

In addition to the above results, there was clear-cut evidence that the magnitude of the standing potential was related not only to growth capacity or hybrid vigor, but also to the genetic constitution. More work of this sort needs to be done, but results already achieved provide a promising lead to a quantitative solution of at least two basic plant problems—genetic constitution and heterosis.

One of the logical consequences of the field theory underlying all these experiments demands that variation in the electrical environment should be reflected in small but detectable alterations in the potential differences. It has been shown by Professor Harlan T. Stetson, of the Cosmic Terrestrial Research Laboratory of the Massachusetts Institute of Technology, that changes in the ionosphere significantly affect radio reception.

It seemed worth while, therefore, to design an experiment which would provide continuous records of the changing potential in a living system. If, at the same time, careful records were kept of changing temperature, humidity, barometer, sunspots, and possibly cosmic rays, it might be possible to detect whether or not correlates between any of these variables could be found. A growing tree was chosen for obvious reasons, and quite complete records have been obtained from three varieties

of trees—maple, elm, and oak—in Lyme, Conn., and in New Haven. No correlates were found among temperature, barometer, and humidity of either the magnitude of the standing potential or its polarity. Daily variations of a diurnal sort were found, as were also seasonal changes. Plots of all the data collected suggested the presence of at least three sets of cycles—two 6-month cycles; three 4-month cycles; and a shorter cycle of approximately 28 days, which seemed to be most closely related to the phases of the moon. The data exhibit the remarkable stability of these standing potentials, which, nevertheless, are subject to definite variations. It is difficult, however, to decide whether the determinations are endogenous or exogenous. If they are exogenous, they certainly are not due to local variations in external environment. The possible correlation with phases of the moon suggests a relationship between the local fields of the tree and the general electrical characteristics of the troposphere. The nature of the endogenous forces which might be present, producing variations in the electrical gradients, is quite unknown. It is hoped that by continuing this study over a period of at least ten years, covering approximately one complete cosmic ray cycle, further evidence may be obtained.

Evidence in the literature of experimental biology points to the importance of the electrical properties of living systems. Furthermore, the data suggest that these electrical attributes are of such widespread occurrence that, in all probability, they are of more fundamental significance than has been generally assumed. It seemed worth while, therefore, to study living organisms from the point of view that the electrical manifestations are a sign of a fundamental electrodynamic field. Such a hypothesis demands that in the laboratory there should be one-to-one epistemic correlation between the logical consequences and experimentally verifiable observations. The studies reported above exhibit such a relationship and, therefore, imply that the basic theory has merit. Moreover, since the theory derives from fundamental field theory in physics, it makes it possible to subsume under one heading the nature of the forces which impart design to living, as well as nonliving, systems.

PARTICLE AND FIELD CONCEPTS IN BIOLOGY

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THE preceding articles have demonstrated the fruitfulness of two physical ideas, those of particle and field, in different regions of biological research. An analysis of living structures into component systems and ultimately into particles, which is characteristic of biochemistry, has long been the prevailing method of investigation. As to its efficiency there can be no doubt, and its range has been vastly amplified by the recent discovery, in physics, of new particles and of new techniques for accelerating them. Hence the promise of usefulness of the particle approach to biology is unusually great today.

But the philosopher has noted a strange accompaniment of this trend of investi-

gation: there is a vague though widespread feeling that particle techniques, based on what is often called mechanistic reasoning, cannot solve the ultimate riddle of biological organization. Thus the biochemist has had to deal with peculiarly persistent aberrations such as vitalism and its *élan vital*, with entelechies and biological fields. None of these concepts can be defined as clearly and precisely as the ideas upon which they are grafted and which they are designed to make philosophically more acceptable. In particular the idea of a biological field stood as a challenge to the scientist who tried to understand it with formal precision.

Meanwhile, fields in the physicist's sense were found to surround living organisms and were studied with the use of ingenious electrical recording devices such as the electrocardiograph and the encephalograph. The potentials measured were of the alternating variety and showed interesting correlations with function and with disease. Though their importance was not mistaken, their alternating character precluded identification with the growth-pattern fields postulated to account for biological organization.

The electric fields described by Burr seem to lend themselves more readily to such interpretation. Being constant in time or slowly varying and showing at the same time a most remarkable correlation with growth, the suggestion made by Burr and Northrop that they be looked upon as the scientific counterparts of the vaguer notions mentioned before seems attractive indeed. Furthermore, since all matter is known to contain electrified particles which generate (or are singularities within) fields, this shift of methodological emphasis from rigid particles of the billiard-ball type to electromagnetic fields as determinants of organic behavior will receive the physicist's enthusiastic approval.

Physics, too, has had to struggle with the famous antithesis of particles vs. fields, and its present verdict may be of interest to the philosopher of biophysics. I begin, therefore, with a brief account of the history of the particle-field controversy in physics and its resolution in modern electromagnetic theory.

While the concept of an atomic particle played a role in ancient science, it attained its quantitative importance when Newton formulated his laws of motion and particularly his law of universal gravitation. His theory populated the universe with innumerable mass particles, all of which attracted one another in accordance with precise mathematical equations. Newton himself was noncommittal with regard to the philosophic status of his particles, but his work led later to the conclusion that they were the ultimate, and indeed the sole, constituents of physical reality. This conclusion was reached spectacularly by Helmholtz in his famous lecture before the Prussian Academy of Science in 1847, a lecture which established for the first time the general validity of the principle of conservation of energy on mechanical grounds.

To appreciate the significance of this scientific event, it should be recalled that the five preceding decades had produced convincing evidence for the theory that heat is a form of energy and that both mechanical energy and heat energy are indestructible. Heat had been recognized as kinetic energy of particles. Now Helmholtz was able to prove the conservation principle on the following two assumptions: (*a*) all matter consists of particles; (*b*) all particles interact by central forces, that is, by forces which extend along the lines joining pairs of particles. The success of this proof, coupled with the knowledge that the conservation principle is universally valid, convinced the scientist of Helmholtz' day that the two premises which formed the

basis of the proof must be true as well. Thus the unique importance of particles in physics was established.

Physicists soon became aware that they had committed a logical fallacy in maintaining the premises as well as the conclusions of Helmholtz' proof, for it was soon shown that electrified particles violated assumption (*b*): the magnetic interactions which arise when they are moving cannot be described by central forces. Beside the particle, Faraday and Maxwell found good reasons, therefore, to introduce the *electric field* as a fundamental entity.

Such fields assign to every point of space a unique value of a physical quantity; for example, the electric potential or the vector electric field. It is in the nature of the field that it be a continuous entity extending over a large part of space, in contradistinction to the particle, which occupies a single point.

The structure of the electromagnetic field is determined by the equations of Maxwell, whose theory deals with fields only and leaves, in fact, no room for particles. According to Maxwell's equations, a charged particle is only a singularity in an electromagnetic field, the motion of which is entirely determined by the state of the field at any instant. All matter is described in terms, not of parameters relating to particles, but of quantities like the dielectric constant, magnetic permeability, and so on.

Newton's and Maxwell's theories represented two extreme positions which can no longer be upheld. Maxwell's theory, while eminently successful in a large domain, fails to describe the behavior of light waves in material media. It accounts for reflection, refraction, and absorption of light in a surprisingly elegant manner but renders no account of the phenomenon of dispersion. Lorentz discovered that this difficulty could be remedied only if both particles and fields are introduced into the theory. He proposed what is now called the electron theory of electrodynamics, a theory in which the properties of particles are no longer an immediate consequence of the structure of the field and in which a special hypothesis is necessary to account for the motion of particles within the field. It may be said, therefore, that modern electromagnetic theory has restored both particles and fields to an important place in physics. No longer is reality regarded as either one or the other. If Newton populated the world with hard peas, and Maxwell thought of it as clear soup, Lorentz has converted it into pea soup.

It should be emphasized that in a pure field theory such as Maxwell's, which purports to describe all electromagnetic phenomena and which represents "particles" as singularities in the field, there is complete equivalence between description in terms of particles and description in terms of fields. Strange as this may seem, it is a simple consequence of the fact that a mathematical function is determined by its singularities. The choice of point of view is therefore dictated by convenience, not by physical reality. In more recent theories, however, including that of Lorentz and certain modifications of it, this equivalence is destroyed; the particle has properties of its own which may not be derived from the field.

The field representation of electromagnetic processes, while more abstract than the mechanical concepts involved in the particle explanation, has numerous formal advantages. First of all, it relieves the scientist from the necessity of believing in action at a distance. According to this doctrine (which, to be sure, involves no logical contradiction but has frequently been regarded as objectionable by theoretical physicists), a particle in one place exerts an influence on a particle in a widely different

place, an influence which is not carried through an intervening medium. Newton, in fact, assumed that this influence is propagated instantly from particle to particle. The field concept avoids this difficulty by assuming the influence to be propagated through a field extending between the particles and at a finite velocity. Action at a distance is replaced by contact action.

In particle physics the motion of an electrical charge is determined by the positions and motions of all other charged particles. In principle, therefore, its exact motion can only be found from a knowledge of all the particles in the universe, including the most distant ones. The solution of this problem is practically impossible. With the use of the field concept, however, the situation becomes quite different. While every particle in the universe cooperates in producing a field in the neighborhood of a moving particle, it is this proximate field which determines the motion. To predict the motion, all that is necessary is to measure the field in the vicinity of the particle and then to apply the equations of Lorentz. Motion is no longer dependent on conditions in the far reaches of the universe but on situations in the immediate surroundings of the moving particle.

To illustrate further the usefulness of the field concept, one might refer to a specific physical example. Consider the interaction of solenoids and that of bar magnets. These systems are known to have the same fields, and thus it may easily be concluded that two bar magnets interact in the same manner as do two solenoids, as is found to be the case. To explain the similarity of action by reference to the particles which make up the systems would be difficult indeed. The advantage of the field concept is obvious.

Finally, the simplicity of the field is apparent in the usual statement of Faraday's law of electromagnetic induction. This law is most simply expressed in terms of rate of change of flux, that is, of a certain integral over the field strength. A statement of the law which has reference to particles would appear exceedingly cumbersome.

Introduction of the field idea into biology would seem both natural and fruitful in view of the well-known fact that all processes used in an explanation of biological processes are finally reducible to electromagnetic bases. This is, of course, nothing but a restatement of the triviality that all living organisms contain electrified matter. Nevertheless, it may be instructive to refer to one or two instances in which this reduction can be completely carried out. The laws of surface tension make possible an understanding of a variety of biological phenomena. While physiologists are often inclined to regard the forces of surface tension as something ultimate, it should not be forgotten that these tensions are nothing other than the intermolecular forces called into play between molecules having certain distributions of electrical charge. These forces can be calculated with some precision and may be shown to have their origin in the electromagnetic fields which extend from the charges in one molecule to those in another.

Osmosis provides a similar example. The usual explanation involves ideas of small and large molecules and rarely considers the cause of molecular size. In fact, the sizes of molecules are conditioned by the field interactions between electrified particles of which the molecules are composed.

Other examples could easily be cited. To explain nerve conduction, electrical motions are invoked, but the theory has not yet attained a form in which complete reduction of the processes to definite electromagnetic phenomena is possible. All these instances argue for probable usefulness of electro-

magnetic fields as explanatory concepts in biology.

After this general discussion of the status of fields and particles in physics we turn our attention to the specific findings of Burr. To attempt to derive the steady fields which he observes by solving Maxwell's equations in detail would be a hopeless undertaking in view of the technical complexity of the problem. Furthermore, since the precise positions of the charges within an organism are not known, the boundary conditions for the solution of this problem could not be ascertained. The best one can do is to assess, in the light of experiences gained in solving Maxwell's equations for simpler conditions, the reasonableness of the observed fields; and here it may be said that they do not contradict the theory in any evident respect. It is true that the potentials measured must satisfy Laplace's equation in the saline medium surrounding the organism, and a cursory examination of the measurements indicates that they do. Also, the presence of currents in the surrounding medium must alter the metabolic rate of the animal, but rough computation shows that the change is negligible in comparison with the normal metabolic rate.

Perhaps the simplest picture in terms of which the results may be presented is that in which the organism is replaced by a very large number of batteries connected in such a way as to give rise to the measured potentials. The elements are cells which are known to possess varying ionic concentrations and thus give rise to electromotive forces. How the cells are connected is at present a matter for speculation with respect to which the data have nothing to say.

The existence of the fields and their compatibility with the laws of physics is not surprising, but the discovery that they are correlated with axes of growth seems of utmost importance both in its practical implications and in its meaning for the development of theory. It is at this point that Burr's work departs from the lines of traditional physics. For the only explanation which classical physics seems to offer is in terms of electrolytic action, a rather lame proposal in view of the observations. Electrolysis might, to be sure, account for the deposition of matter in the region where the head of the salamander is formed (this being the cathode with respect to the external circuit) but one hesitates in venturing so crude a suggestion. The correlation of electromagnetic fields with growth must therefore be regarded as a novel discovery which present-day physics does not completely elucidate.

The field approach to biophysics raises several very interesting logical and philosophical questions to which brief reference will here be made.

The first concerns the logical relation between the batteries of our simple picture and the field. Certainly the two are closely related in the sense that if the state of the batteries were known the resulting d.c. field could in principle be calculated. The reverse, however, is not true. Knowledge of the fields does not suffice for a prediction of the detailed ionic states of the batteries nor of the connections between them. This lack of reciprocity of the logical relation between particle configurations (batteries) and the field may be of considerable scientific importance. If the problem of growth were connected with the state of the batteries, its solution would seem exceedingly difficult. According to Burr, it is tied to the state of the field, a knowledge of which can apparently be gained by direct measurement and without recourse to complicated particle configurations. From a methodological point of view this seems fortunate indeed and may hold promise of simplifying biology.

There is also the question as to the causal relation between the batteries and the field, a question not explicitly dealt with in the preceding article; but Burr and Northrop, in earlier publications on what they call their electrodynamic theory of life,[1] appear to have taken the attitude that the field may be primary in a causal sense. In accordance with this view one would have to think of the field as not entirely conditioned by the particulate state of an organism, but indeed as an agency connecting the batteries of our simple picture in a manner conformable to the external field. The origin of the primary field is somehow left unexplained, and the teleological questions which gave rise to the earlier field theories of biology have been side-stepped. It would be interesting to have Professors Northrop and Burr discuss this matter in greater detail. The physicist, at any rate, has very little to say about it.

It is a well-known fact that the static field surrounding a closed surface such as a salamander egg is entirely determined by the values of the electrostatic potential at all points on the surface. This leads to the curious consequence that the fields measured by Burr, fields which determine the growth of a three-dimensional, complicated, and finely organized object such as the salamander head, should themselves be completely determined by potentials on the *surface* of the salamander. A three-dimensional structure appears to be mapped on a two-dimensional surface. While this is by no means impossible, it is a result perhaps more surprising than the congestion of a large number of biological traits into the small space of a chromosome.

On the other hand, the proposals under discussion have also been subjected to criticism of the following sort. If the electromagnetic field is a growth-controlling factor, its precise character, its minute variation *within* the organism, should be important. External fields are at best merely the tail end, or the asymptotic part, of the significant fields within. Why, then, focus attention upon these secondary agencies? This is not a serious objection. For there are phenomena in atomic physics where a very similar situation exists without occasioning doubt. For example, in the scattering of particles by atomic fields, it is the *asymptotic* part of the wave function which determines the results. If there were a way of finding this asymptotic part without knowledge of the function near the atom, it would indeed be acceptable and a welcome simplification of the problem.

Before concluding, I may perhaps be permitted to inject an idea which, though it is only loosely related to the subject of the preceding articles, may be of interest in connection with the larger problem of biological organization.

Most of the points raised thus far deal with the question of mechanism in biology. The fields discussed are meant to implement the procedures of mechanistic physics to make them account for organization and growth. Modern physics, however, contains elements which are definitely nonmechanistic, elements which lend themselves, within limits, to an understanding of some processes similar to those of biological function. Foremost among them is the exclusion principle of Pauli; it may be shown to have an interesting bearing on what has been said. The principle is an abstract one requiring a certain symmetry of the states of a complex system. It is nonmechanistic in the sense that it does not allow the properties of the entire complex to be derived from the properties of its parts. In its simplest form it shows that no two elementary particles can be in identical states.

Its application to atomic systems is well

known. For example, the orbits of the electron in the hydrogen atom are determined by dynamical laws. When a second electron is added to the nucleus to form a helium atom its motion is no longer fixed by dynamical laws alone but is dependent upon the fact that another electron is present. Crudely, one might say that one electron "knows" of the presence of the other and adjusts its motion accordingly. This anthropomorphic statement is, of course, not to be taken literally; it is meant merely to imply that the effect of the exclusion principle is the same as would be the effect of such "knowledge." In the structure of heavier atoms this adjustment of the motion of one electron to the presence of others not implied by the dynamic laws of motion but imposed by the exclusion principle is of utmost importance.

Emergence of new properties is a common phenomenon in modern physics and one which can also be traced to the same principle. For example, when two hydrogen atoms interact they will either repel or attract each other. Once two atoms have formed a molecule and this molecule is approached by a third atom, the alternative of attraction or repulsion no longer exists. The two atoms will always repel a third. Thus a new property, invariable repulsion, or saturation of attractive forces, has superseded the former alternative. Finally, there is a host of other effects, frequently termed cooperative effects by physicists, to which the exclusion principle gives rise but whose inclusion here would lead too far afield. The general importance of this principle has been discussed elsewhere.[2]

While this purely formal element of modern physics leads us a considerable distance toward an understanding of some of the crucial biophysical problems, it does not of course promise to solve them all. It does, however, provide a beautiful example showing the fruitfulness of a single change in physical methodology, and it may not seem unreasonable to suppose that further principles of symmetry of this general type may some day shed light on a larger field of problems.

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THE COPPERHEAD

By Paul D. Harwood

I met you where the sun
Had warmed a patch of forest floor;
Tricornered spots of brown that often fused
Along your back of lighter tan
To form crossbands of hourglass shape
So close a match had made to patterns cast
By sun and shade in last year's leaves,
That I had overlooked you till you moved.

And many times, I found you under logs,
Retreating not from foes,
But from the cold a norther brought in March,
Or from the harsh, dry heat of mid-July.
My moving mass disturbed you not at all,
You merely watched with lidless eyes,
And tested with your black-tipped tongue
The littered space between.

And when I stepped too close,
Your head drew back
In preparation for defense,
The while your sulfured tail a warning rattled
On a dry and withered leaf.
An inch or so above the forest floor,
Your still head swung,
Alert with flashing tongue.

I left you crushed and broken,
The marvel of your body distorted by a blow,
For I have never found the strength
To leave you undisturbed.
The blow has marred my memory;
Yet in it lies a forceful image
Of simplest patterns and somber dyes
That bore a scaly beauty to my scanning eyes.

OKINAWA AND ITS PEOPLE—I*

By PAUL E. STEINER

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(On leave of absence from the Department of Pathology, The University of Chicago)

OKINAWA SHIMA, the largest island in the Ryukyu (Luchu, Loochoo, Nansei, etc.) Islands, was invaded by United States naval and military forces on April 1, 1945. The actual invasion was preceded by a prolonged but intermittent bombardment by planes and ships which began in October 1944. Our possession of this island was necessary to deprive the enemy of an important step in his chain of islands and to serve as a base for further advances. Because of the nature of the defense, particularly in the heavily populated southern end of the island, the combat was on a large scale. As a result, the country was heavily damaged and the population extensively dislocated. Advance information on these islands was, in many respects, defective because they had been isolated from the main commercial, cultural, and tourist currents. Because of their isolation, however, they had developed in certain cultural respects along independent and original lines. During reconstruction the American influence has been so great that life as it existed before the invasion is not being reconstituted. Even before combat stopped marked changes were observable in the people and their life. Some observations on the old Okinawa might be of interest to workers in the biological and social sciences.

Okinawa is a tropical island about 60 miles long and 3 to 8 miles wide, lying between 26° 04′ and 26° 53′ north latitude and 127° 38′ and 128° 20′ east longitude. It is roughly equidistant (350 miles) from Formosa, the China coast, and Kyushu, the southernmost large Japanese home island, and slightly farther from Luzon. It lies in a northeast to southwest axis. A large peninsula, Motobu, projects about 10 miles from the northwest side; a smaller one, Katchin or Katsuren, from the southeastern shore; and another, the Chinen, from the southern end. The island shows volcanic and coralline origins, the latter predominating. The northern two-thirds is rugged, rocky, and forested; the southern third is rolling and broken, but wherever possible it is used for farming and it contains most of the inhabitants. Shallow valleys, divided into small plots for farming, ravines, and coral outcroppings clad with flat-topped pines and cycads characterize most of the island. The highest elevations are about 1,600 feet in the northern sector and 500 feet in the southern. The coast line is protected by reefs in nearly all places. Several beaches with coastal flats up to about a mile in depth are found, but generally there is an abrupt rise to the interior plateau behind a narrow coastal region. Innumerable small springs and a few large ones are scattered over the island. Despite a heavy rainfall, no large streams are found because of the narrowness of the island and the extensive use of water-impounding and -utilization devices, which hold the water near its point of fall.

The climate is good. Temperatures are moderate: the maximum in summer is over 90°, and the minimum in winter is in the forties; the means range from 60° in January to 83° in July. There are no

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sudden variations in temperature from day to day or from night to day. The humidity is high at all seasons. The annual rainfall of about 80 inches is distributed throughout the year from about 10 inches in May and June, the wettest months, to 4 inches in December. Thunderstorms are uncommon and mild. Typhoons, up to two or three per month, may be expected at any time from June to December, but only one to three of them each year are severe. They usually come from the southeast, arising in the region of the Mariana, Marshall, or Caroline Islands; gusts of wind may reach velocities of up to 200 miles per hour during such storms. Living has been admirably adapted to this violent natural phenomenon, as well as to earthquakes, which can be felt about once a month.

The population of Okinawa was given as 460,000. The racial origin of these people is obscure; their earliest recorded history is said to date from 605 A.D., although their folklore goes back several thousand years. When asked whether they are Chinese or Japanese, they invariably reply that they are neither but Okinawans. It is believed that engrafted on the original racial stock, the Ainu, white (Caucasoid) people common to these and the Japanese Islands, were liberal admixtures of Chinese, Korean, Japanese, Filipino, and Malayan peoples. To these were added in a few communities in more recent centuries, very small amounts of Portuguese, Spanish, and possibly other European strains.

The Okinawans were mainly rural people, and the chief occupation was agriculture, followed by fishing. Several dozen villages and towns and a few cities had grown up. The prewar populations of the larger places were said to be as follows: Naha, 65,000; Toguchi, 20,000; Shuri, 17,000; Nago, 13,000; Haenna, 8,000; Itoman, 6,700; and Yonabaru, 5,000. Until recent times communications throughout the Ryukyus were poor. About five different dialects were found in the Ryukyuian language, three of them on Okinawa. Most of the older people could not speak Japanese, which was introduced in the schools some years ago as part of the program of Japanization begun about 1875 and intensified after 1900.

During the campaign I had the opportunity to perform a series of 150 necropsies on native Okinawans. From these observations were derived anatomical and pathological data which I have described in "Anatomical and Pathological Observations in Necropsies on Okinawans." (In Press: *Archives of Pathology*.) Further information on life in the islands was obtained by submitting, in interviews, about 400 questions to eight Okinawan physicians from various towns and to a number of English-speaking residents. Most of the questions concerned medical problems, but some of them dealt with social, economic, religious, and cultural conditions. These two sources of information were supplemented by personal observations during travel, with interpreters, about the island. Historical and religious information was obtained from several other compilations and from native Okinawans.

History. The earliest recorded event in Okinawan history dates from 605 A.D., when a trading mission sent by the Emperor of China tried to obtain information about the Ryukyus and make a trade agreement, both without success. Prior to that time historical events are legendary, and the origins of the people and their land are closely connected with their religious traditions inasmuch as the religion is a system of ancestor worship, which tended to preserve such information.

According to legend, the people and the land originated long ago from chaos when the sun-god, Teda-Ko, dropped from heaven Man and Woman, who gave origin to the first male, Shireku (Shineko, Shinireku),

and the first female, Amamiko (Amanikye), who created the land and the people. According to an alternate myth, the first people arrived in a storm from the islands of the South Seas. At what date the first people came is not known, but some villages claim that they have occupied their present sites for 3,000 years. The residence of the first people is variously described as at Kadaka-shima, Kori-jima, or Nago. Kadaka-shima seems to have had official support for its claims because the former kings of Okinawa visited the island for worship and prayer each February. It is a small island located in the entrance to Nakagusuku Wan off the east coast. (This has been renamed Buckner Bay in memory of Lieutenant General Simon Bolivar Buckner, Jr., Commanding General of the U. S. Tenth Army, who lost his life from artillery fire south of Naha on June 18, 1945, in the last days of the campaign.)

Modern ideas regarding origins are less romantic. Geologically, the island shows volcanic and coralline origins; racially, the people are considered to be a mixture of Ainu (Caucasoid), Mongoloid, and Malayan strains; culturally, they exhibit Chinese and Japanese influences engrafted on an older, possibly indigenous, culture.

According to legend, the Okinawans in turn gave rise to the Moro peoples inhabiting Mindanao in the Philippine Islands when a sailing vessel was wrecked in a storm. Being unable to build a new ship, they were forced to remain. In support of this story they point to similarities in the languages and in the religious custom of praying to ancestors represented on the name plates in the family shrine.

In 1945 four ancient castles were found, located at Shuri, Nakagusuku, Yontan, and on Katchin Hanto near Haebaru and built on strategically located, rugged hilltops which commanded wide views. The construction was of hewn stone fitted into thick, massive walls filled with rubble. They were said to be about 500 years old but, even so, to be the successors to an older series of prehistoric castles which have disappeared. They are survivors of the feudal days. Shuri Castle was the last dominant castle, the ancestral home of the last Okinawan kings, and, more recently, an administrative and governmental center.

Since the dawn of recorded history the Okinawans, although nominally independent, were influenced by both China and Japan and at times paid allegiance and tribute to both. At one time Okinawan sailing vessels carried on widespread commerce with the Asiatic mainland and the islands of the Western and Southwestern Pacific. The island was first brought under a single rule in 1187 by one Shuntem-O, a king with Japanese ancestry. Thereafter a succession of kings ruled for 700 years. During part of this time China, particularly under the Ming Dynasty (1368–1644), exerted the dominant influence. Buddhism was introduced from China in the fourteenth century, and the Okinawese princes were educated in China or by Chinese tutors.

When Japan adopted the closed-door policy in 1638, Okinawan commerce, which was important, was stifled and an agricultural economy was forced on the island. Commodore Perry visited Naha on his way to and from Japan in 1853–54 and he urged on the American Congress that coaling and observation posts be established there. In 1875 Japanese troops occupied the island, and tribute to China ceased. The king was reduced to viceregal rank, sent to Tokyo, and Okinawa was incorporated into the Japanese Empire. Since then efforts to Japanize the island have been intensified.

Anatomy. The Okinawans are small, dark, well-proportioned, Oriental people. The skin is usually dark brown. This deep pigmentation is no doubt due partly

to the tropical sun. A few yellowish persons are seen and rarely one who is very light-skinned. I saw a fair-skinned child on northern Okinawa and another on an adjacent island, Haenza Shima. Both children otherwise resembled the usual natives. I was told that such people were not rare; they are attributed by the natives to European rather than to Ainu mixture. The hair is straight and black, and both sexes have on the average much less body hair than Americans. The body length of adults in a small series was 60.5 inches for men and 55.8 inches for women. Children are correspondingly small at the different ages. Young women are sometimes plump, but obesity is not found at any age in either sex; these people therefore appear quite muscular. The body is slender to stocky; the legs are moderately short, and an appearance of bowlegs sometimes seen is usually because of wide placement of the femoral heads rather than bowing. The carriage is erect, and the bearing, especially in women, is graceful. Heavy labor in the fields is regularly performed by the women, and they habitually carry burdens on their heads. The feet are broad across the toes; the great toe stands apart and is trained for grasping.

The facial features are Mongoloid, with slight to moderate development of the medial epicanthic folds and a moderately broad face. The appearance varies between two extremes: Some have a flat or a concave broad face, a large mouth, and a broad nose with sunken bridge, associated with a stocky body; others have a narrower face, with a straight, thin nose, and a slenderer body.

The internal anatomy shows several points of difference from Americans and Europeans. The organs are small in proportion to body size, with two striking exceptions: the spleen is smaller and the pancreas larger than would be expected. In Americans the normal spleen is slightly larger than the pancreas; in Okinawans the normal pancreas is about one-third larger than the spleen, and it is nearly as large as that of Americans. The colon is relatively greatly elongated, so that it has several extra coils. In the hepatic flexure it frequently exhibits an S-shaped curve, the transverse colon lies very low, and the sigmoid region forms a long, pedunculated loop. The suggestion is made that this elongated colon represents an adaptation to the prolonged use of a predominantly vegetarian diet, which has a bulky residue, analogous to that of herbivores. On dissection, the excellent muscular development becomes conspicuous because of the small amount of adipose tissue, both subcutaneous and deep.

Taken as individuals, Okinawans are indistinguishable from the Japanese; considered as a group, however, the Okinawans are darker, shorter, stockier, and hairier.

Pathology. Okinawa appears to have been, on the whole, a fairly healthful place. In comparison with the United States the infectious diseases were more important and the degenerative diseases were considerably less common, so much so in fact that the reasons therefor merit further investigation in the hope that etiological factors may be disclosed.

A few infectious diseases were less common than in the United States. These included the streptococcic diseases (notably scarlet fever) and their relative, rheumatic fever. Mumps was said to be rare, and most of the population seemed to escape whooping cough.

On the other hand, intestinal helminth infections were very common, though they seemed to cause little trouble. In the bodies examined 44.7 percent showed *Ascaris lumbricoides*, 34.7 percent had hookworms (chiefly *Necator americanus*), and a few of them were infected with *Trichuris trichiura*, *Enterobius vermicularis*, *Strongyloides stercoralis*, and tapeworms. Tubercu-

losis, the dysenteries, the diarrheas, and pneumonia were common and constituted the chief serious medical problems. Filarial infection, as revealed by the presence of microfilaria in the blood, had a high incidence, but its sequel, elephantiasis, curiously, was uncommon, indicating a benign infection despite the fact that the organism is said to be *Wuchereria bancrofti*. About a thousand lepers, collected from the Ryukyus, were under treatment in a leprosarium located on Yagachi Shima, a small island off Okinawa. This disease was stated to be decreasing. Malaria was not a big problem; a few cases formerly occurred annually, chiefly in the northern part of the island, where, however, after the invasion with its disruption and overcrowding an epidemic ensued among the natives. An outbreak of encephalitis, believed to be Japanese B encephalitis, also occurred; this was said by the native physicians to be an almost annual event. It was the only disease found which is not present in the United States, although its near relatives are entrenched here. The island appears to be heavily contaminated by a virulent form of tetanus bacteria, as indicated by the history of numerous cases in the past following childbirth and trauma and by the large number of cases in natives which followed war injuries. Dengue or a dengue-like disease was said to cause epidemics at intervals of a few years. Measles and conjunctivitis appeared in epidemics almost annually.

Yaws, schistosomiasis, leishmaniasis, tsutsugamushi, typhus, cholera, plague, anthrax, smallpox, rabies, and some other diseases did not occur, according to the local physicians.

The degenerative and retrogressive diseases appeared to be relatively rare. Arteriosclerosis, lithiasis (cholelithiasis and renal lithiasis, but not bladder stone), hypertension, malignant tumors, heart disease of all types, disorders of the endocrine system such as *diabetes mellitus* and hyperthyroidism, and nodular enlargements of the prostate, thyroid, and mammary glands appeared to be relatively less common than in the United States. The causes therefor were not obvious, but dietary and hereditary factors and a placid, low-tension but physically vigorous life, as well as the equable climate, may be important.

The sick were treated by home medication and by doctors who were in private practice. Most of them were native Okinawans, sons of well-to-do familes who had been educated in Japanese medical schools. They were, on the whole, well trained to recognize and treat the diseases encountered. Most of them did only minor surgery except that they treated fractures and drained infections; major surgery was generally referred to a few doctors who specialized in this work. Several examples of group practice had sprung up about small hospitals, which were found in the larger cities. Licensed native practitioners, who had been trained by serving time in a hospital, were also in practice. Childbirth was attended usually by relatives, less commonly by midwives, and by physicians only after trouble was encountered. Infant mortality was fairly high. The oldest inhabitants were said to be about a hundred years of age, and many lived past seventy.

The people possessed a great lore of herbs and medicines; most of their remedies were cathartics or so-called blood purifiers and strengtheners. Pharmacies, located in the larger towns, had a fair assortment of some of the modern drugs.

The Home. The homes, each a compact unit enclosed by a wall or hedge, were located either on the farms or clustered together to form villages and towns or, more rarely, cities. Whether single or in groups, they usually occupied land which was not of the best quality for farming

because of outcropping rocks or steepness. The homesites were frequently cut back into a slope to give protection against typhoons by embankments. The homes were connected with the farms by footpaths or by narrow dirt or cobblestone roads just wide enough to permit passage of small two-wheeled carts.

The buildings comprising the home were small, and the plan of their arrangement was fairly uniform. They were enclosed in a level square or rectangular space by a dense hedge of giant grasses, shrubs, and trees which grew directly from the level ground, or from a dirt wall, or from the embankment at the edges of the area if it was cut back into a slope. Wealthy people had a stone wall of fitted rubble or hand-hewn coral blocks. The hedge or wall, regardless of its composition, served to keep out prying eyes and to lessen the force of typhoons. The home compound had one entrance located in the center front. This was not guarded by a gate but by a short segment of wall or hedge several feet longer than the width of the entrance and set a few feet interior to it. Thus it was impossible to look directly into the home or to enter the compound on a straight line. It was necessary to turn to the right or left around the end of the short inner guarding wall, whose function was the same as that of the main wall. Legend gives its original purpose as that of keeping evil spirits out of the home since, supposedly, they were unable to negotiate the turns. The house nearly always faced south to take advantage, being open in front, of the cooling breeze which prevailed from that direction in the hot summer months and to admit the low-angle rays of the sun while keeping out the prevailing northern breeze during the winter months.

The following arrangement could be seen from the entrance of the typical rural compound: a small stable to the left, storage barn to the right, and a larger, rectangular building of better quality extending nearly across the back of the compound. This was the house. At its left end stood a small, partly sunken, thatched pit constructed of hand-surfaced, fitted coral blocks. This structure was a combination pigsty and latrine, whose important function was the conservation of human and animal excreta for use as fertilizer. A well from which water was drawn with rope and bucket was located in any part of the compound; nearby was a small outdoor pool. Part of the compound was paved with coral blocks in the homes of the affluent. A few small fruit trees or flowering shrubs, usually hibiscus, along the wall completed the typical arrangement. The position of the house was constant, but that of the storage building, stable, or pigsty-toilet was sometimes reversed. An upright, cylindrical stone or concrete cistern, about 30 inches in diameter and 6 feet tall sometimes stood between house and stable, collecting rain water from the roof of each. In the towns the storage barn in the right front corner was not uncommonly replaced by a building used for ceremonial occasions or for housing guests.

The house was usually built of wood with a mortised, flexible frame and thin board paneling inside and out. The roof was usually grass thatch, although red tile roofs were not uncommon. A brick or concrete lion or tiger head on the roof guarded the home against evil spirits. The stable, pigsty, and storage barn were usually thatched on bamboo attached to a rough timber frame. Most thatched buildings had heavy upright corner pillars of hewn coral securely fixed in the ground, between which were walls composed of rubble.

At worst, all the buildings were made entirely of thatch or with mud and rubble sides and thatch roof. At best, they were all built of hewn coral or wood with red

tile or even molded concrete tile roofs. Rarely, the house, or stable, or both had two stories.

The open front and right side of the house could be closed by sliding wooden panels, which ordinarily were out of sight. A second set of panels, constructed of latticed wood covered by rice paper, was sometimes placed about 2 feet interior to the outer set. Additional protection from sun and rain was given by wide, overhanging eaves, which were supported by a row of round poles, cut from the trunk of small eucalyptus trees in such a way that the spreading roots at the butt provided a wide, secure base. Eucalyptus wood, being resistant to termites, was used whenever possible in the framework of the house.

The kitchen was in the left end of the house. It had a dirt floor and was equipped with mud, tile, or stone hearths, cupboards, pottery jars for storage, and various utensils, mostly of home manufacture. There was usually no chimney, so the walls and roof were blackened with soot. The smoke was conducted under the roof through the garret over all parts of the house. This smoking, which gave a characteristic odor to the houses, was thought to preserve the wood from termites. Dried straw, twigs, and sugar-cane stalks were used for fuel.

Small homes had one living room; larger ones had two or three in a row across the front, in addition to several small rooms across the rear used for storage of food, weaving materials, and unused furniture. The main living room was called the shrine room because recessed into its rear wall was the family shrine. If the house was fortunate enough to have a second room, it was placed to the right of the shrine room. This was known as the porch room because it was open on two sides, front and right. If the house was favored with a third room, it was interposed between the shrine room and the kitchen and was used mainly for eating. All the main rooms except the kitchen could be converted into a single, large room by sliding back the disappearing panels. This was done during the day and for ceremonials. At night these panels were used to subdivide the space into sleeping rooms, and the family slept on thick straw mats laid on the wooden floor. The sleeping gear was stored in a built-in closet during the day.

The furniture consisted of the shrine, a low table, several small straight chairs, and small trays. Most of the furniture was lacquered, a finish suitable to the damp climate. On the wall were usually found framed pictures of any or all of the following: Emperor Hirohito, members of the family, Thomas Edison, Napoleon, and Abraham Lincoln. The large houses had a special seat reserved exclusively for the rare visits of royalty.

There were no modern plumbing and sewage systems. Houses were well ventilated, being partly open, but uncomfortably cool during three winter months because of lack of heating facilities. Artificial lighting was not needed because of the habits of the people.

The Family. A family was begun by a marriage ceremony, which was usually secular and participated in by both families concerned. It lasted from one to three days. The groom was usually about twenty-five years old and the bride about twenty; the extremes were eighteen to thirty-two and sixteen to twenty-five years, respectively. The average age at marriage, formerly younger for both sexes, has increased, owing to more years spent in school and in compulsory military training. The marriage was formerly arranged by the two families, sometimes through a middleman, but in recent years there has been an increasing tendency for young people to choose their own marriage partners—so-called love marriages. Mar-

riage enforced because of pregnancy was said to be rare. The custom of betrothal by presentation of the *tisaji*, woven from cotton in the form of a towel by the girl for the boy, was abandoned about 30 years ago when cheap machine-made Japanese cotton cloth made local cotton raising and weaving obsolete. Acceptance of this gift indicated that the male realized that alone he was incomplete, a defect which the girl by making the gift indicated she could remedy.

The newly married couple lived with the groom's parents if he was a first son; otherwise some other home was arranged. The complete typical family consisted of the paternal grandparents, the oldest son and his family, and any unmarried sisters from all generations.

Okinawan culture was patriarchal and patrilinear. The oldest male in the family, usually the grandfather, ruled with the power of a dictator. At his death the rule descended to the oldest son, provided he was twenty years old; otherwise it fell to the grandmother until her son reached his majority. Property was inherited in the same way as power. The oldest son inherited nearly everything, but he could share with his brothers if he wished or if the father required. This failure to divide the property explains how such large homesteads have been built up in a country where earning power and resources are so low; they represent the accumulated wealth and productive energy of many generations.

Formerly the backs of the hands of brides were extensively tattooed with blue-black pigments. This was a form of branding to announce to all that she had a husband. According to rumor, it was directed mainly against foreigners who might wish to molest the wife. This custom was abandoned about 30 years ago and in 1945 was seen only in women over fifty years of age.

The married couple soon had a series of children. The average number was said to be five or six; the extremes were none to twenty, and families with ten children were common. The babies were generally born two years apart, the mother nursing a child until the next one came along, which was sometimes as long as three years. Sterility was blamed on the woman, and it was the main cause for divorce on the island. Childless couples sometimes adopted a child. A man rarely had a second wife or concubine, and any such were kept secret and apart from the family home.

The women did all the work in the compound and much of that in the fields. They cooked, cleaned, wove cloth, and made clothes; collected, dried, and stored food; milled the grain; fed the animals; and planted and harvested the sweet potatoes and other produce, carrying burdens on their heads in baskets which they had woven. The men supervised and helped in the fields, did the skilled work in connection with building, construction, and irrigation, and fished. Many farmers were engaged in part-time work away from home.

The men controlled the finances and kept the money, but there was one exception to this custom. At Itoman, a fishing village where a few Portuguese are believed to have settled centuries ago, it was said the women kept a separate purse. Here it was even possible for an unmarried daughter to earn and retain money to be used as a dowry, which sometimes took the form of a new fishing boat for the bridegroom.

Diet. The main articles of diet were sweet potatoes, rice, and various types of beans, including soybeans. These were supplemented by green vegetables of many kinds, millet and barley, meat, fish, and fruit. Squash, tomatoes, radishes, carrots, eggplant, cabbage, chard, onions, asparagus,

and other vegetables were found in some gardens but by no means on every farm. They were eaten either raw or cooked, as were many types of green leafy plants. Taro, tapioca, and sago were produced for home consumption. Sago was made from the ubiquitous cycad, *Cycas revoluta*, which was used for food in several forms after treatment to reduce its inherent primary toxic factor. Fruits were not used in large amounts; the commonest varieties were a small sour orange, a small banana, papayas, and a few peaches. Black or brown sugar made from the cane grown on nearly every farm was eaten as such or made into confections; it was not used to sweeten food as is the Japanese custom. The natives considered it profitable to sell sugar and buy rice.

The chief source of proteins was soybeans. Most families slaughtered one or two hogs annually, usually at the New Year. The pork not eaten fresh was preserved with salt, refrigeration not being available; goats were also raised and used for meat by most families; chickens and ducks added a little variety. Fish was eaten several times a month in most homes. Beef and horse meat were rarely available to the average family. Milk from goats or cows was used only by those who were ill and by babies whose mothers had insufficient milk. Cheese was repellent to the Okinawans.

Dietary fats were chiefly lard rendered at home and soybean oil imported from Manchuria. Attempts, not very successful, were made to make beef tallow more usable by mixing it with soybean and other oils. Small amounts of oil were obtained from various types of nuts and seeds, including cycads, again without great success. Olives were not grown on the island, and coconuts, which are so important elsewhere in the islands of the Pacific, were very rare.

The diet appears to have been overbalanced by carbohydrates and to have had an excessively bulky residue. Superficially, it seems that it must have been deficient in proteins. However, when it is judged by fertility, longevity, incidence of disorders of metabolism, and evidence of deficiency diseases, it stands up very well. Prior to the war it protected against most infectious diseases except dysentery and pneumonia. It certainly was not conducive to a rapid rate of growth or ultimate large stature, criteria commonly used in judging diets in America until recently. Some cases of war edema were seen but only after a long period of inadequate food intake consequent to the disruption of normal living conditions by the war. The natives preferred their food to ours, but this is grossly unfair to us because they were judging mainly by our K rations.

[*To be concluded*]

THE INTERRELATIONSHIPS OF ENZYMES, VITAMINS, AND HORMONES

By BENJAMIN HARROW

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VARIOUS activities of the living organism need for their continuance the mediation of enzymes. Such problems as those connected with digestion, tissue synthesis and repair (including growth), reproduction, nerve conduction, muscle contraction, and metabolism (changes which foods undergo in the organism) are all under the control of enzymes. What, then, is an enzyme?

The answer can be illustrated by a specific example. In the stomach we find a substance, pepsin, which acts chemically on proteins in our foods, transforming them from complex chemical molecules (proteins) into relatively simple ones (largely amino acids). The actual amount of this pepsin is minute; yet it has the ability to transform enormous quantities of protein into amino acids without, apparently, being used up in the process. Pepsin is our conception of an enzyme; and this conception is in accord with the view that the enzyme is a catalyst—a substance which initiates or, still better, accelerates, a chemical reaction without itself undergoing any apparent change. Since the enzyme does not undergo any apparent change, a little of it goes a long, long way.

Pepsin is merely one example of hundreds of enzymes in the body, each with sharply defined functions—some, like pepsin, acting on proteins; some, like ptyalin in the mouth, acting on carbohydrates; some, like lipase in the intestine, acting on fats; some playing vital roles in tissue building and repair; some guiding, in a step-by-step process, the degradation of foodstuffs, ending with the elimination of carbon dioxide, water, and simple nitrogenous substances like urea. But all the enzymes have one property in common: they are catalysts, differing from inorganic catalysts by being synthesized within the various cells of the body.

The enzymes have another property in common: they are proteins. A number of them have been isolated, and in each case the chemist reports that in their chemical properties they belong to the proteins. But these elegant researches immediately raise a profound problem, which can be put in some such form as this: We know casein (in milk) to be a protein; we know albumin and globulin (in the white of egg) to be proteins; we know pepsin (in the stomach) and trypsin (in the pancreas) to be proteins; yet the pepsin and trypsin are enzymes, and casein, albumin, and globulin are not. Why not? What differences in the internal structure of a protein molecule are necessary to make one protein act merely as a food (casein, albumin, or globulin), and another as an enzyme (pepsin or trypsin)? We have as yet no answer to this question.

And while on this phase of protein chemistry, we may extend our observations to neighboring fields. Insulin, the hormone a lack of which gives rise to diabetes; several viruses associated with disease; and, in all probability, even the very gene itself have been shown to be proteins or substances closely associated with proteins. Again, what configuration of a protein is necessary for it to exhibit the properties of a hormone, a virus, or a gene?

Some of the substances just described

are proteins attached to compounds less complicated than the proteins. For example, the virus and the gene (or, more strictly, the chromosome) have their protein molecules attached to nucleic acid, a nitrogenous substance quite unlike a protein and much simpler in chemical structure, though complex by comparison with the structure of table salt (sodium chloride). Also, the enzymes taking part in the oxidation of foodstuffs in the body are proteins joined to several organic molecules not proteins. These "several organic molecules," in part, are identical in chemical make-up with some vitamins; from which it follows that some vitamins, at least, are needed to build (that is, synthesize) certain enzymes.

A VITAMIN has been defined as a necessary food for the body in the same sense that fats, proteins, carbohydrates, mineral salts, water, and oxygen are necessary foods. But there is one obvious difference between vitamins and our common foods: the former are utilized by the body in minute quantities. This fact has puzzled biochemists for a long time, but the answer is apparently at hand. If, as has been proved in several instances, the vitamin molecule is part of the larger enzyme molecule, since enzymes are able to perform their functions in minute concentrations, it follows that vitamins would be needed in doses equally dilute.

A lack of vitamins may give rise to diseases which have been made well known by the advertising fraternity—beriberi, pellagra, rickets—and we are told that vitamins are needed (in our diet, we would add, rather than as pills) to prevent these diseases. While this is true, the biochemist is more particularly interested in discovering the precise function of these vitamins when present in an organism which appears "normal." We have already more than hinted at this function; now we shall be more specific.

Two of the many enzymes which play dominant roles in the metabolism of the body are known as coenzyme I and coenzyme II. Both have been isolated and their chemical structure sharply defined. Part of their molecular structure —in addition, of course, to their association with protein—includes nicotinic acid. This same nicotinic acid is one of a group of vitamins belonging to the vitamin B complex, and its absence in the body gives rise to the disease known as pellagra. These observations lead to the following conception: Nicotinic acid is needed by the normal organism for the syntheses of coenzymes I and II, which, in turn, are needed for the functional activities of the organism. If nicotinic acid is absent from the diet, coenzymes I and II cannot be synthesized, with the consequent disturbance of certain bodily functions which show themselves, among other things, in the clinical form of pellagra.

Here we have a clear-cut example of how intimate is the connection between a vitamin (nicotinic acid) and an enzyme (coenzyme I or coenzyme II). And this example is but one of several such relationships.

CLOSELY related to the vitamins and the enzymes are the hormones, substances which are discharged into the blood by certain glands and which thereby influence the activity of certain organs. Many hormones have been isolated, and in a number of cases their chemical constitution has been established. Some, like thyroxine and epinephrine (adrenaline), are relatively simple substances; others, like the sex hormones, are more complicated; still others, like insulin, and hormones in the pituitary, are proteins, which makes them the most complicated of all.

As in the case of the vitamins, the absence of one of the hormones gives rise to a disease which is usually characteristic. Thus, a deficiency in the amount of insulin results in diabetes. But an overproduction of a hormone may be equally bad; for example, too much thyroxine (in the thyroid gland) may be the cause of popeyes in exophthalmic goiter.

Again, as in the case of vitamins, we ask ourselves the question: Precisely what role do hormones play when they are present in the normal organism? To repeat, as the definitions tell us, that they "influence" or "excite" organs to activity, leaves us with a feeling of vagueness as to just what is meant.

For at least two of the hormones a much more satisfactory answer has been suggested by Cori, the talented biochemist at Washington University in St. Louis; and it is quite possible that what is true of these hormones is true of others. To appreciate Cori's contribution we must make a few remarks on the subject of carbohydrate metabolism—changes which carbohydrates undergo in the body.

Normally, the amount of sugar (glucose) in the blood remains remarkably constant, despite variations in the intake of carbohydrate. If, however, the production of insulin is deficient, the amount of glucose in the blood increases, glucose appears in the urine, and diabetes results.

This view of diabetes, centering on the production of insulin and following the discovery of the hormone by Banting, has been modified as a result of the researches of Houssay, the foremost physiologist in Argentina. Houssay showed that one of the hormones of the pituitary—we shall call it the diabetogenic hormone—behaved as if it antagonized the action of insulin; whereas insulin tends to prevent the sugar in the blood from increasing, the diabetogenic hormone acts in the contrary direction: it causes an increase of the sugar.

Under normal conditions we have a "balance of forces," the insulin tending to depress the amount of glucose in the blood and the diabetogenic hormone tending to increase it. In abnormal conditions, as in diabetes, where the amount of insulin is small, the diabetogenic hormone from the pituitary runs riot, resulting in the overproduction of glucose in the blood and in the urine.

This balance of forces in the normal organism is of profound significance. For every plus in one direction we usually find a minus in the other. On this basis it might have been fairly easy to predict that if a substance like insulin tends to depress the sugar in the blood, there must be some countersubstance which tends to increase it. Pathological conditions arise when one or the other of these hormones varies in amounts other than normal.

One is tempted to apply this principle, this balance of forces, to the cure or prevention of a number of diseases. For example, in cancer, cells grow at an abnormal rate. Purely as a hypothesis, we may assume that normal growth is regulated by two factors, one in the plus direction and the other in the minus. If we assume, for example, that the factor in the minus direction disappears or becomes less potent, what is there to prevent cell growth from running riot? Obviously, this is an oversimplification of a complicated problem.

To return to Cori's work: When carbohydrates (or, for that matter, fats or proteins) are oxidized in the body to carbon dioxide and water, the change is not an instantaneous one but a step-by-step process. Between carbohydrate and carbon dioxide and water there are many stages, some known, some still unknown. Each link or stage or step is directly controlled by some enzyme or enzyme system. It follows, therefore, that if for some reason

the enzyme system at any one stage is not functioning properly, the disturbance is not confined locally but influences all subsequent chains in the step-by-step degradation. The break in one link in the chain is enough to cause a catastrophe.

One such link in carbohydrate metabolism is reached when glucose combines with phosphoric acid to form glucose-6-phosphoric acid. This combination is brought about by a specific enzyme called hexokinase. Cori now finds that the activity of hexokinase is, in turn, under the control of two hormones, one being insulin and the other a hormone derived from the pituitary (which we shall call the diabetogenic hormone). The latter impedes the action of hexokinase, whereas the insulin prevents the diabetogenic hormone from doing mischief. The insulin, in other words, is antagonistic to the diabetogenic hormone. Normally, both play their role by properly balancing their forces. But abnormally, in what we call pathological cases, this balance is upset.

If, for example, the insulin no longer functions effectively—owing to a diseased pancreas and the consequent decrease in the production of the hormone—the diabetogenic hormone is no longer controlled by an inhibitory force. If such is the case, this pituitary hormone prevents the enzyme, hexokinase, from acting; this, in turn, prevents glucose from combining with phosphoric acid at one stage on its road to ultimate oxidation and elimination. What is the result? The glucose, derived from the food that we eat, is prevented from going further and being used up; it, therefore, accumulates and floods the blood and urine with excessive quantities, bringing in its train the clinical symptoms identified with diabetes.

But from the standpoint of this discussion, the arresting observation is that the hormones, in turn, control enzymic activity. This is quite a change from the statement that hormones "excite various organs to activity." We are now told very specifically that hormones regulate the activity of enzymes and that the latter are more directly responsible for the various changes that much of the food material undergoes in the body.

We can now gather up our separate threads to present a unified picture. At the very heart of the activities we call life are the enzymes. To manufacture many of them, vitamins, among other things, are necessary; and to regulate the activities of some if not all enzymes, hormones are necessary. These are not three separate entities, each going its own way, but all three are intimately related to one another, with a cooperative plan so delicately handled that a failure of any one of them to act immediately upsets the dynamic equilibrium of the body as a whole, resulting in pathological symptoms, or disease.

As a postscript, the fruitfulness of the ideas discussed here can be further illustrated by another, and striking, example.

Enzyme systems are common to all things that have life—from the highest to the lowest; from the complex organism known as man to such lowly forms as bacteria. As is well known, some of the worst diseases to which man is subject are caused by certain types of bacteria—the pathogenic variety. Such bacteria may invade our bodies, grow and multiply, and cause endless havoc. How can we stop their growth and multiplication and so prevent disease and possible death? At various times different methods have been used with varying success. But let us now apply the principle involving enzyme systems. What if we disturb some enzyme system in the bodies of these bacteria and so prevent them from multiplying? What

if we break one link in the chain of reactions that go on in bacteria as they do in human beings? If we succeed, we may bring death to the bacteria rather than have the bacteria bring death to their unwilling host.

This is precisely what we believe the sulfa drugs, and probably penicillin, do to certain types of pathogenic bacteria; the sulfanilamide disturbs a link in the chain of enzyme reactions in the bacteria, with the result that the bacteria die—and man continues to live.

A miraculous result, some cry; but some also ask whether this is merely the survival of the lesser of two evils.

ECLIPSE

Again an age-old miracle occurs,
Known to the ancients even as to us:
An event to which the Odyssey refers,
And freely mentioned by Herodotus.
Again man holds before his finite eyes
The smoke-stained glass, to peer into the sun,
And sees a pearl corona; darkened skies
Design a preface to oblivion.

What are the centuries to sun or moon,
Whose courses cross with measured interval?
Now midnight interviews the crystal noon,
As scientists record the miracle;
Here Science charts the moon's acceleration,
While man is lost in blinded admiration!

MAE WINKLER GOODMAN

THE SELECTION AND TRAINING OF FUTURE SCIENTISTS

By PAUL F. BRANDWEIN

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Our civilization is a race between education and catastrophe.—
H. G. Wells.

WORLD events justify special concern with the education of future scientists. Such an education should start early, should be continuous, and should assure us a constant flow of science-talented youth. The teacher in graduate school grumbles about undergraduate training; the college teacher grumbles about training in the high schools. The link between undergraduate and graduate school is easily discernible; that between high school and college is obscure. Nonetheless, it is real; it is important and has played a significant role in the past in the training of our scientists. Dr. Bush's provocative report *Science, the Endless Frontier* clearly recognizes the part high schools must play in such training.

It is my purpose here to describe the preliminary work done in the training of "future" scientists in the Forest Hills High School, which accepts students with varied gifts, opportunities, and economic background. This so-called Training Method, together with other devices, may be helpful in predicting and selecting science-talented youth in high schools. The fact that this article will concern itself with one high-school method useful in discovering, encouraging, and training science-talented students should not lead us to think that science work begins in high school. Science programs are beginning to extend into the elementary school. Soon, the gods of the budget willing, our citizens will have a closely knit science education during the 12 years of their public-school training. The effects on science and scientists of such a program are not difficult to envisage.

At this time, however, the science-talented student is not a clearly defined personality. Does such an individual exist? Or are we thinking merely of a student who combines high intelligence and an ability to work with an originality and initiative which sets him apart only in degree—not in kind—from his fellows? In other words, there is talent in art, talent in music. Is there also a special talent in science? While this question is exceedingly interesting and important, we need not concern ourselves with it here.

This article will deal with those students whose work in science is such that they will perhaps increase its techniques and enlarge its boundaries—in short, with the future research scientist. The phrase "future research scientist" does not imply final judgment; it is a tentative, still unreliable, designation of a student who may distinguish himself in science.

LET us follow an entering freshman class of approximately 350 students at the Forest Hills High School. All of them take a course in general science. This seeks to convey understanding of the common phenomena in their environment, the problems of life and living toward which science contributes; namely, use of natural resources, use of energy for doing the world's work, getting along with other men, and the conquest of disease. The first step in selecting and training the embryo scientist occurs here.

Careful scrutiny is exercised. The progress of students who show any signs of

distinguishing themselves in science is noted during the first few months. Attention is given not only to those whose grades are high, but also to those whose grades are low but who show an ability to work with their hands—those who may have a "hobby" interest in science. These first distinctions are vague, and we would have them so. For our purpose is to give each student the environment to make the utmost use of his gifts and opportunities.

At the end of the first term, those who have shown an interest as well as an ability to work in science are given an opportunity to work in our laboratories during their free periods before, during, and after school. Those who are interested generally ask for this opportunity. Those who are not interested but who have ability—as shown by their work in class—are invited to work. They are permitted to select their field of work or they may be guided into a suitable field. Some of the activities they may choose include the following:

1. Preparing teaching materials in chemistry, physics, or biology.
2. Maintaining a large school museum of a wide variety of living and preserved specimens.
3. Assisting a specific science teacher in his field of special interest.
4. Maintaining a vivarium of forms particularly useful in biological work. Thus students learn to maintain insects and mammals as well as cultures of the common protozoa and algae.
5. Science work in a variety of activities such as *The Science Journal* and the Chemistry, Physics, Biology, or Engineering clubs. They may help prepare broadcasts for the Science Broadcast Club or draw and make models in the Bio-Arts Club. The Weather Bureau, the Agassiz Nature Club, the Museum Curators, the Science Projects, the Cancer Committee, the Youth Council on Health, and the Laboratory Technicians' Club offer other opportunities.

This club program is broad, purposely so in order to attract and hold those interested in science.

During the second term this program of guidance continues. The result is that many good students enter into so-called extracurricular science work. It is our belief that this work is not extracurricular but should and must be an integral part of school work in any science training program. Our students go on to a second year of science work.(This is, for the vast majority of students, a course in biology.) There are many factors responsible for this. Two not inconsiderable ones are the sympathetic viewpoint toward science of the administrative officers and the guidance department of the school and the favored economic position, in general, of the student body. But we also feel that the wide program of science activity offered is in itself a factor.

In the second year of science, selection of students for special science training begins. Toward the end of the first term of biology, students are selected, on the basis of I.Q., reading score, and grades in the first three terms of science, to enter a so-called Biology Honor class or classes, depending on the number available. Out of 350 entering students, some 40 to 60 find themselves in this group. Those students whose programming difficulties make it impossible for them to enter the class are nevertheless given opportunities for similar work.

Generally, students of the highest caliber are found in this class. Among them are the students who are in the first quartile of their graduating class. Regularly, the first 10 in scholastic standing are to be found in this honor class.

Several purposes are served by having these students in one class. They are given a different course of greater difficulty, of more advanced material. They are capable of a high appreciation of scientific method and its social implications. They are given work which stimulates them to develop high efficiency in the laboratory and field. More important than these, perhaps, is that considerable time is spent in personal guidance, so that the opportu-

nities and advantages of entering science are opened to them. The net result is an important preliminary step. Every student from this class takes a third year of science, and approximately 96 percent of them take a fourth year, continuing with physics and then chemistry. Approximately 40 percent of these will take a fifth year of science.

It should not be assumed that these are the only students who enter upon further work in science. Our responsibilities in science education are twofold:

1. We are responsible for a program of general education. This attempts to give those who will not become experts in science the basic understandings, skills, and appreciations that will enable them to cooperate closely with those who will become, or are, experts. This program must also, we feel, have special and practical application to the problems of living, so that the student sees scientific methods at work in solving his personal and community problems.

2. We are responsible for a program of special education. This attempts to select and give special training to those who may become our scientists of the future.

Thus, while practically 70 to 75 percent of our student body of 2,800 are taking science work each term (many of them elect 4 years of science), approximately 200 to 240 students (around 40 to 60 are selected each term after the second year) are in this special science program.

But other aspects of the education of these special science students are emphasized. They are urged to get wide training in mathematics, social studies, music, art, and manual skills. The relation of the scientist to his socioeconomic milieu, the mirroring effect of science on society, is stressed at every point.

Thus, of 350 entering students, approximately 40 to 60 will enter the honor classification and become special science students. Are we justified in making this selection? We have permitted students with scholastic averages of 80 and 85 to enter this classification and given them the same training. The student with a special hobby such as radio or the collection of insects is also selected, regardless of his grades. Whether we are justified depends on the data we will gather from the future achievements of these students in collegiate and postcollegiate work. Every student is given the opportunity to show his ability and avail himself of special training, but we have found that those who have an average grade of 90 or over are able to take the best advantage of the type of training to be described below.

The students selected each term are given the opportunity to—

1. Engage in some "original" research work on the high-school level. Each student is under the close guidance and observation of a teacher. (This work will be described briefly later.)
2. Learn the expert use and operation of laboratory equipment of all types (analytical balance, microscope, electric oven, autoclave, etc.).
3. Learn laboratory techniques (histological, bacteriological, techniques of analytical chemistry, work with glass, etc.).
4. Gain special skills in shopwork, including handling of common materials, wood, metal, etc.
5. Read representative college texts in biology, physics, and chemistry.
6. Take adequate training in mathematics.
7. Prepare exhibits of their work for demonstration before other students, at science fairs, or local exhibitions.
8. Prepare reports of their work (in their senior year) in writing for the school science journal or for other journals.
9. Engage in seminar activity at regular meetings of the Forest Hills High School Science and Mathematics Honor Society. Students who have shown competence in science are eligible for election to the society. Students who offer the best reports of their work will in turn be invited to submit their work at a Biology Congress sponsored by the New York Association of Biology Teachers or to exhibit it at the Science Fair sponsored by the Federation of Science Teacher Associations of New York.
10. Engage in the Annual Science Talent Search of the Westinghouse Educational Foundation.

This is the basis for what may be called the *Training Method* for selecting science-talented students as opposed to the *Testing Method*. Reliance is placed on the observation of these students at work rather than on any given battery of tests. This method is in its preliminary stages of development, but it is described here because of its relationship to the problem of staffing our science laboratories.

In a good many of these activities, the actual scientist can see a picture of the scientist-in-embryo. And this is especially true of the research activity engaged in by these youngsters. Here the student faces a problem he has never faced before. No solution is available in textbooks, and it may take two or more years of work to reach even a tentative conclusion. For instance, do zygospores of *Rhizopus nigricans* germinate? Many textbooks figure such germination. Several of our students find no such evidence on the basis of fine investigation. They find their conclusions supported by authorities in the field. How long does digestion take in the food vacuoles of different protozoa? Why does *Chaos chaos* appear to have only a regional distribution? What factors influence sporting in species of *Coleus*? What effect does the gas produced by *Tribolium confusum* have on other insects? Other work includes a study of the embryology of *Physa*, diapause in Cecropia and other insects, development in vitro of certain plant embryos, studies on a modified method useful in the recovery of silver in photography, the structure of soils in the vicinity of Queens, studies on aberrant electrostatic effects, and others. There is no doubt in my mind as I have observed these young people at work that they go through the operations of the scientist at work.

As they work, they grow. They make the scientist's methods of thought their own —at least in regard to the problem at hand. And perhaps it is not astonishing to find that they feel that these methods hold much promise for use in investigating social as well as natural phenomena. Is this naiveté? Is it just the commonplace rashness or idealism of youth?

This early training and emphasis in the social responsibilities of scientists is of the utmost importance. These boys and girls are still plastic. Perhaps if we train our students in high school and earlier to see themselves as citizens first and specialists later, we shall not have the situation which Dean Harry J. Carman describes in these terms: "In public life we are ruled by scientific ignoramuses, and in the scientific laboratory we have, for the most part, political and social illiterates." The elimination of this state of affairs is a prime objective of our program of general and special education.

In the second year of high school we have, then, 40 to 60 youngsters who are ready and willing to embark on an extended period of work. They are given laboratory space. They must read source material, plan experiments, order materials, construct equipment—in short, over a period of two years, carry out in a small way the methods which serve the scientist. Their teachers are ready to advise them, but advice is forthcoming only when the problem presented is worthy of consideration by the sponsor and is not indicative of laziness, poor thinking, or poor working method. In the case of the latter, the student is given whatever guidance is advisable and turned back to his work.

During the two-year period, many students drop out of the work. Some find that athletics and social events are more important to them than science; others are not fitted—through lack of even the simplest manual skills—to carry on the work; still others lack originality. A few are lacking in honesty or a sense of responsibility and are advised by the sponsor to seek other work, but only after failure

has attended many attempts to produce desirable changes in attitude by methods at our disposal. And, of course, there are those who cannot learn to work with others. Again, we have never released any of these until attempts have been made to make desirable changes, and in some instances we have retained these youngsters till the bitter end—especially when their basic qualities have warranted it.

In any event, by the beginning of the latter half of the senior year we have perhaps 10 boys and girls who have participated in most of the 10 activities previously listed.

From these 10 senior special science students, come the 3 or 4 seniors who, we believe, may be scientists of the future. These 3 or 4 are given a sort of apprentice training in their senior year. They are placed in industrial, college, or research laboratories to assist scientists at work. For this we are grateful to the many individuals in college and industrial laboratories who have given generously of their time and energy. These students distinguish themselves by winning extensive honors in the various activities sponsored by different organizations.

Forest Hills High School is now five years old. Since February 1945, when the first class which had been with us 4 years was graduated, this program has yielded 13 students whom we consider to have promise as research scientists, to have the ability necessary to contribute to science. But approximately 200 students have been given similar training. While we feel that these 13 are of the caliber to make research scientists, we think that these 200 may also enter the scientific field and achieve a measure of success. We do not predict the same kind of success for them as for the 13 but we shall check our predictions against the actuality.

These 13 have won a good many honors. Seven of them placed in the Westinghouse Science Talent Search, 3 among the first 40 finalists; 3 have won the Gold Award for work presented at the Biology Congress in New York. They graduated in the top 20 of their respective classes.

A rating scale is in the process of being developed which will be refined in further testing. This scale has considerable attractiveness from our point of view. The items selected for rating are scholarship (as determined by scholastic average), manual skill, originality, responsibility, honesty, capacity for work, and ability to work with others. We are also developing "paper-and-pencil" tests that seem to be promising. With these, we may be able to predict success in science.

ON THE basis of our preliminary observations, it is obvious that high I.Q. alone, or high scholastic average, does not guarantee success in science. Our preliminary observations indicate an approximate lower limit in I.Q. of 125–130 (Otis) for the achievements (honors) which these 13 accomplish.

Also, it is our very tentative view that the high-school hobbyist in science (the collector of insects, etc.) who does not have wide general ability may make a good technician but may not make research contributions of high caliber. This conclusion, of course, depends on a close check against the future activities of a good number of hobbyists and a good number of our well-rounded students. Five to 10 years from now we shall be able to make a more reliable statement.

We may draw a very rough sketch of certain characteristics of this "future" scientist. It must be emphasized that this is subject to change depending on further observations. He has an I.Q. whose lower limit is about 125–130, an approximate reading score of 15–16 (9), and a remarkable capacity to work and learn. He is within the upper 10 of his

graduating class. He distinguishes himself in the caliber of his work by winning honors. He has broad general ability. His scholarship shows few weak spots; he is competent in mathematics and languages as well as in science. Usually, he shows high manual skill and inventiveness.

He may be of good or poor economic background and of native or foreign extraction. In general, he shows high social consciousness and is ready to see the relationship of science to society. He believes that the force of science can be used for the good of society. He is good material to turn into the scientist or any variant thereof. But like any embryo, he needs nurture. Shall we waste him?

Is he poor? Then he may not go to college. If he does go, will he be lost in the shuffling and reshuffling of thousands of college freshmen? Or will there be close cooperation between college and high school to make the most of his ability? Or should he be treated like any other student—left to take his chances, so to speak?

We feel that there should be a comprehensive program throughout the United States that will stimulate and maintain the best science teaching in the high schools as well as in the colleges. This program would need funds, but it would also need highly improved teacher-training facilities. There should be a carefully planned program (a national foundation, perhaps) to select science-talented students and subsidize their training. Again this calls for funds, but it also calls for the development of selecting devices. The experiences of the Westinghouse Science Talent Search, of special high schools (such as the Bronx High School of Science in New York), as well as the experience of teachers throughout the country, need to be examined.

It is urgent that science look to its sources. These are: first, a steady flow of the highest type of young men and women; second, the facilities to train them. And, most important, we need—desperately—the teachers who have the training to develop the future scientists of the United States. Science must look to the development of those who are its mainspring and hope.

SEEING AND BELIEVING*

By C. JUDSON HERRICK

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SEEING, they say, is believing. This is true, no doubt, but sometimes what we believe we see does not square with what is really there to be seen. Every such experience is a challenging problem, a problem that science is expected to solve because we have to make our livings in the world as it is, not as it may seem to be. If I am seated in a stationary railway coach looking at a train passing on the adjacent track, I may believe that I am moving very fast. The speed of the relative movement of the trains may be correctly judged, but my orientation with reference to it is wrong.

In science, just as in common life, the true meanings of things are expressed as relationships, and the meaning of a thing is not an impersonal abstraction. It means something to somebody, and this relationship of the observer to the thing sensed is an ineluctable component of every experience, whether or no the individual has any awareness of the experience.

The dogma enshrined in scientific tradition that scientific truth is somehow detachable from the experience that validates that truth and employs it in the search for the meanings of things must be thrown into the discard along with the other discredited mythologies. All data of science and all generalizations derived from these data are defined relativistically from the standpoint of the observers upon whose experiences they depend. This relationship to the observer is inevitable; we cannot escape it, for it inheres in the very nature of experience. What we must try to do is to enlarge our experience so as to include all the relevant data and so avoid the pitfalls which entrap us when essential components of the situation are ignored.

Let me illustrate by a few examples from my own experience.

My brother Clarence and I, on one of our scientific excursions, went from Cincinnati to the hill country of eastern Kentucky. On the return we took a night boat down the Ohio River, and on awakening in the morning I was annoyed to find myself disoriented. The feeling that we were moving eastward and upstream was irresistible, and it was unaffected by any rationalization about it. The fixed idea was impervious to the knowledge that we were actually going in the opposite direction. This has been a common experience with me, and I resolved to try to analyze it and see what actually happened in my mental process when the false orientation was corrected. As we approached Cincinnati I seated myself comfortably at the bow of the boat and intently searched the shores for the first glimpse of a familiar landmark. Unfortunately, my attention was distracted for a moment, and upon raising my eyes there was the silhouette of a well-known house against the sky on the right bank. It was on the summit of Mt. Lookout just where it should be, and the apparent direction of our movement was instantaneously reversed without effort and without the slightest participation of conscious experience. This experiment has several times been repeated, and I have never been able to observe anything that would help to explain how the reorientation was accomplished.

Another problem of orientation has repeatedly been encountered in my subsequent travels. How do we judge distances

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of objects in the field of view? Or is it a judgment? Perhaps it is a direct perception, or it may be that neither perception nor sensation, as conventionally defined in terms of awareness, are involved but some quite unconscious sort of nervous adjustment.

On my second visit to New Mexico, I arrived at Socorro after midnight, was taken to the home of my brother Will, who was a U. S. deputy land and mineral surveyor, and immediately went to bed. In the morning my first glimpse of our surroundings was a narrow vista seen through the window opposite to which I was seated at breakfast. The field of view was almost completely filled by a roughly conical pile of rock pointing upward into the sky. I was perfectly familiar, from previous experience, with the clarity of that atmosphere and the difference between the aerial perspective there and in my home environment in Ohio. I speculated: Is this perhaps a mine dump a few hundred yards away or a big mountain on the far side of the mesa? Without asking questions, I tried my best to estimate its distance. There was neither foreground nor background, and I could find no way to determine how big or how far away it was, though every detail was brilliantly clear. Not until I was outside, with a wide sweep of horizon in view, did I recognize that it was the peak of Socorro Mountain five miles away, a peak which I had seen from a different angle on a previous visit.

This readjustment of my visual habits to a different atmospheric condition had a curious sequel. After a few weeks in the mesa country, most of the time in travel by wagon, upon my return to Ohio the recent readjustment of visual habits persisted. My house in Granville was on College Hill, looking out across the wide flat valley of Raccoon Creek to a backdrop of lower hills on the opposite side. Rising from the floor of the valley at the foot of my own hill was a conical upthrust known appropriately as Sugar Loaf. On the morning after reaching home I looked out across the valley and over the bare summit of Sugar Loaf, where there was a group of people. To my amazement, they were all giants!

The explanation came to mind at once, but the illusion persisted. The men were about a quarter of a mile away, and the intervening haze dimmed their outlines more than twice that distance would have done in the clearer air of New Mexico. The lines of the visual angle projected upon my retina by each man were, accordingly, extended outward to a greater distance, and his stature rose to gigantic proportions. I was perfectly familiar with every foot of that topography, and the actual distances were known. The apparent size of these people was certainly not given to me by any process of rational judgment; indeed, I knew that it was irrational. It is as if the sensory experience was presented to consciousness as a finished product completely fabricated below the level of awareness.

A similar problem confronted me on my first visit to the Grand Canyon. Again my arrival was at a late hour of a very dark night, and I retired at once in one of the cabins provided by the hotel. In the early morning I dressed hastily, keyed up to a high pitch of excitement in anticipation of the big thrill. A few steps from the door took me to the parapet and I looked down.

Yes, here was a chasm; but how big and how deep was it? Was this the Grand Canyon, a mile deep, and was the opposite line of cliffs the North Rim, thirteen miles away? Or was I looking into an insignificant lateral tributary with dimensions measured in hundreds of yards instead of miles? To my chagrin I could not answer this question. It was like looking at a photograph of a gully in a clay bank which might be an inch deep or a hundred feet. The whole circle of the horizon was spread

before me, but there was not a landmark that gave a clue for estimating distances or dimensions. Instead of the anticipated thrill of amazement I got only puzzlement, distrust of my own perceptive acumen, and shattered complacency.

If I had turned away and left the Canyon at that moment, it would have been to join the hosts of little men who look at big things without seeing them, typified by the English tourist who had registered at that same hotel a short time before. He was an inveterate globe-trotter in the conventional British tradition and costumed for the part. Stepping to the parapet, he looked out for perhaps ten minutes and then returned to the desk with the query, "When is the next train out?"

"In twelve minutes."

"Righto. You may send my luggage down to the station."

"Why, aren't you going to stay to see the Canyon?"

"Aw, I've seen it."

Not until I had walked down to the river and up the six thousand feet of tortuous trail to the opposite North Rim and camped for several weeks around and within its castellated walls did I feel that I had begun to grasp the stupendous magnitude of the thing and to appreciate its glory. After several such visits I feel that now I have seen it, and this experience has made me a bigger and so (I trust) a more humble man.

THESE incidents suggest three topics that men of science usually shy away from as of philosophic rather than scientific interest; but that is a mistaken attitude, for science is vitally concerned with the relations between appearance and reality, and so we must examine critically the nature of the evidence upon which we must base our judgments. First, we must have some sort of working hypothesis about the relations existing between our knowledge of the objective world and the use that we make of this knowledge in reasoning about it; that is, between what we may call, for short, objective, or sensorimotor, experience that is projected outward and those subjective experiences that have no such external reference. We must inquire into the nature of experience itself as we employ it in scientific work and try to arrive at what I call a natural history of experience.[1] In the second place, our ideas of the nature of scientific evidence and explanation need to be clarified. Third, how do the conclusions reached on these questions bear upon the place of science in human economy and practical problems of adjustment?

The first of these topics is basic for science. Illusions of sense, for instance, must be analyzed, not only in terms of objective realities, but also in accordance with what we can find out about both the cerebral mechanisms involved and the accompanying subjective mental processes. All these things really exist, and we cannot understand the illusion without taking them all into account. The discovery of these relationships is a scientific problem which will never be resolved by metaphysical dialectic.

It has long been recognized that introspection can give us only the end result of a process, a large part of which goes on without any awareness of it. So the subconscious has invaded the domain of introspective psychology; but, since these underlying physiological processes are inadequately known, a way of escape has been sought by personifying them and endowing these fictitious daemons with capacities which, so far as we know, are exercised only by rational persons. These subterfuges must be replaced by more exact knowledge of what actually goes on in the body preceding and during the conscious process as we know it introspectively.

Our ignorance regarding the bodily concomitants of the mental life has in other quarters led to the choice of a different, indeed an opposite, form of escapism. Acting, I suppose, on the principle that what you do not know will not hurt you, all introspective experience is ignored, ruled out as an epiphenomenon of no significance in the control of behavior—this in the face of the evident fact that every purposeful act that we perform is motivated and directed by affective and rational mental acts.

The dilemma can be resolved only by frank acknowledgment of the well-established fact that our conscious experience and the subconscious (that is, actually unconscious) bodily activities that underlie this experience and generate it operate in accordance with laws which, in the present state of our knowledge, are not directly commensurable. No refinements of introspective skill in observation and analysis can be expected to reveal to direct awareness the mechanism by which this awareness is created, for the laws of thought as revealed introspectively and the laws of operation of material structures as examined extraspectively are unconformable. The converse, of course, is also true. It is a vain hope to anticipate that we shall ever be able to construct a mechanical model of cerebral activity designed in accordance with the laws (known or unknown) of objective mechanics that will give us direct insight into the mechanism of subjective experience.

This is a cardinal principle of traditional metaphysical dualism, but this philosophy unfortunately is incompatible with any truly scientific approach to the body-mind problem. Nature cannot be split apart in this fashion. And, again, to deny the validity and primacy of either objective or subjective experience is a subterfuge that merely obfuscates the problem. Neither traditional idealism nor traditional materialism is scientifically tenable. The only way open to the naturalist is to recognize the disparity of introspective and extraspective experience and then try by some indirection to discover the still unknown laws in accordance with which their obvious interrelationships are explicable. This again is an approachable problem.

The second topic suggested here raises these questions: What do we mean in science by explanation? And to what kind of evidence must we appeal in a search for explanation?

Everyone agrees that science should describe all that is found in nature and how it operates, but eminent authorities insist that science has no right to ask why things are what they are and why they do what we see them doing. Nothing could be more absurd. The structure of things, how they work, and how they came to be what they are, these are essential steps toward comprehension of their meaning; but we do not grasp the full import of that meaning until we know why they have this structure and why they behave as they do.

In the *Encyclopaedia Britannica* (11th ed., **24**, 403, 1911) we read: "An 'explanation' proves to be simply a restatement of a phenomenon in terms of other phenomena which previously are familiar to the mind, and therefore appear to be better understood."

This definition of scientific explanation which makes it scarcely more than a tautology misses the essential point of it. The restatement of a phenomenon in terms of other familiar things has value for clarification of what it is only because it brings to light additional relationships which before were not apparent. The familiar things are better understood only insofar as their relationships are more completely known.

Here is a rock that looks like a tree trunk. It is not a piece of a tree, of course, because

it is made of mineral, not wood. Chemical analysis identifies the mineral and sets it in its proper niche in our classification of familiar minerals. But this does not explain why it looks like a tree. Grinding and polishing a broken end reveals a pattern of rings like that seen in living trees. Here is an additional relationship that justifies the inference that this rock is a petrified tree. The botanist may find that its structure agrees closely with that of some well-known living species, and so he gives it the same name. He counts the rings and tells us how old this tree was when it fell. He may even give us some reliable data about the climatic changes that occurred during the life of the tree. Now the geologist is called in to examine the terrain and tell us about how many thousand years ago this tree lived. We now have a scientific explanation of our specimen in terms of verifiable relationships, an explanation that is far more than a mere list of other familiar things. We know why this thing that appears to be just a piece of rock looks like a tree.

Darwin described many ingenious devices by which insects are induced to cross-pollinate flowers. Doubtless the bees and the moths do not know why they do this, but there is a reason. The intricate floral structures of orchids and the correlated behavior of the insects are quite meaningless apart from the end to be achieved. This is why they exist. The organic realm is shot through with similar adaptive adjustments. The primary meaning of all adaptations is survival; and beyond this are countless others, all of which may be subsumed under the term satisfaction. The fangs of the tiger are adapted for the satisfaction of hunger, and we men have set up aesthetic and ethical standards of conduct because these give us our deepest satisfactions. Life is not aimless. All vital activities are directed toward some end; it may be food, mates, or exuberant playfulness just for the fun of it. This is what life is for, why we live—to get as much satisfaction out of it as possible.

This hedonistic view of life is not ignoble. It is as broad and as rich as life itself, embracing every vital interest from mere survival value on through the limitless hierarchy of values up to our finest aesthetic and moral aspirations. We cherish these higher virtues because they are good for us and because they make for fuller and richer lives.

In the inorganic realm it is harder to find reasons for things, and this perhaps is why so many scientists take the easy way out and pass the buck to the metaphysicians. We know how a geyser was made and why it operates as it does in accordance with familiar hydrothermal principles. But why is a geyser? And why is the Grand Canyon? The structure and growth of the Rocky Mountains have been well documented; also the correlated volcanic, seismic, orographic, climatic, floral, and faunal changes. The how is fairly well known, but why? The further course of these changes can be projected into the future with some assurance, and this predictability of natural events is one of the greatest triumphs of modern science. We can forecast the time when the Rocky Mountains will be reduced to a peneplain, just as other mountain ranges are known to have been leveled in past geological ages. Tide tables, weather predictions, and all the rest are possible because all natural processes are interrelated in an integral orderly system. The task of science is to discover the laws of this order and to show how to adjust our human affairs in accordance with them. Some of these, like the phases of the moon and the generations of men, come in repetitive cycles, and when the full cycle is known, any observed phase can be set in its place in the cycle and the next phase predicted.

Many natural processes have directive trends that we can discover; otherwise prediction of future changes would be impossible. Insofar as the directions taken by these movements of cosmic events are recognizable, we can begin to understand why particular incidents happen as they do. We get the meaning of the detail, its scientific explanation, only insofar as we learn the place it occupies in the larger pattern of directive movement. These patterns of inorganic directive (as distinguished from random) movement can be recognized all the way down the scale of magnitudes from nebulae to electrons; and, as has been pointed out by Lillie,[2] they have some characteristics in common with those which in organisms we call adaptive.

All this leads us back to our claim that the why of things is the cardinal scientific problem. The meaning of any natural thing or event cannot be fully grasped or explained scientifically until we discover its relations to the other components of the orderly flow of process that we call our cosmos. Not all of these relationships are relevant to any particular inquiry. A critical or pivotal "referent" (to borrow an expression suggested to me by Dr. S. H. Bartley) is selected, deliberately or unwittingly, and others are assembled with reference to it.

If the proximate goal of any movement, organic or inorganic, is recognized, our explanation is satisfactory up to that point. Radium spontaneously disintegrates into atoms of different structure and properties; an incandescent mass may become a planet with people living on it; a continent was desiccated by climatic change, and fishes became amphibians; a man wants to talk with his distant associates, and he invents a telephone. In none of these instances is the course of events random or haphazard. From the standpoint of the observer, there seems to be an end to be achieved and an orderly process directed toward that end. The mechanism of this apparent teleology is intrinsic to the natural system in operation; it is not imposed upon it from without. This is what we mean by saying that it is natural, not a mystic thaumaturgy. Moreover, cosmic events do not run in uniform cycles; the figure is spiral, not circular. Throughout all nature new things emerge, and these too must be explained. This doctrine of emergence also must be kept within the frame of the natural.

FINALLY, how does this naturalistic conception of experience and of the part that experience plays in scientific explanation bear upon our present problems of adjustment to the realities of the world in which we are living?

The naturalist's attitude toward nature and his interest in its mysteries differ from those of the intuitionists, whether these be artists, poets, or philosophers. The naturalist wants to find out, not only what there is, how it came to be, and how it works, but also the why of things. His understanding of the meaning of what he sees is incomplete until he has some comprehension of its *raison d'être*. His sensitivity must be as acute and discriminative as that of the artist, though applied in a quite different realm of creative artistry in a quest for values of different order. And his imagination must be not less vivid and nimble. He gets satisfaction out of his facts, just as the artist does, but for him the full measure of satisfaction is achieved only when the raw facts are set in a frame of relationships that reveals a valid explanation of them.

"Art for art's sake" has been a popular slogan, but the great artists have not practiced it so. Art for life's sake is a truer motto, for the successful artist—painter, poet, or actor—lives his art, and if he cannot make it live in others it is feckless and sterile. So also in the life of science "truth

for truth's sake" is meaningless verbiage. The truth that makes you free is live truth incarnate in human struggle and aspiration. Truth for life's sake gives point and poignancy to our search for the truth about things and the meaning of things.

A reorientation of the scientific outlook is indispensable if science is to meet the obligations thrust upon it by the exigencies of our time. The method of science must infiltrate every human enterprise. We must learn to regulate all our affairs, personal, social, and international, in the light of actual experience, critically evaluated, rather than blindly to follow outworn tradition or appeals to prejudice, greed, and lust for power. Science is not the only avenue to this goal, and we must work for more harmonious collaboration with our colleagues in the so-called humanities.

The general neglect and misunderstanding of the true methods and objectives of science are shockingly prevalent. A recent writer[3] solemnly assures us "that the modern scientist no longer considers that he can explain anything." The same issue of THE SCIENTIFIC MONTHLY that contains this benumbing potion supplies the antidote. Professor Benjamin[4] in an enlightened plea for the humanizing of science remarks, "Seeking for reasons is, in the broad sense of the word, science itself."

Not long ago I published a plea for the recognition and enhancement of human values in all scientific work.[5] These human interests belong in science because there would be no science apart from them, and we work for the advancement of science because it is good for us and because it enlarges the range of human satisfactions. This is the why of all science and in the social sciences this is conspicuously the crucial issue. I asked:

> In what kind of a world do we want to live? This is the most urgent problem of today and tomorrow. We can get what we want within reason if enough of us join our forces We can exploit other people in our local communities and in the community of nations and acquire wealth and power—at the cost of continued disorder and sooner or later of war—or we can cooperate in competitive enterprises in lawfully ordered ways and win peace and prosperity.

Social comity and harmony imply cooperation and some measure of renunciation of personal and national sovereignty. In current practice we recognize this in the domain of sport. In a boat race the two teams are intensely competitive, but the competition is regulated by rules of the game previously agreed upon. The good sportsman abides by these rules and does his utmost to win in conformity with them. It is a curious anomaly of our culture that in our hours of relaxation, in our recreations and sports, even in our prize fights, we voluntarily cooperate in competition in accordance with the rules, but in the serious affairs of life, in business, politics, and diplomacy, we revert to the laws of the jungle. No advanced and progressive civilization can be built on this foundation. If our humanism cannot emerge from this morass of bestiality, we do not deserve anything better than the turbulence which now obstructs every effort to stabilize our social structure.

To all superficial appearances we are drifting in industry and diplomacy toward perdition, at the mercy of stupid leaders intent only on personal, group, or national advantage, with ruthless disregard of those principles of fair play upon which all real social progress so far achieved has depended. This conflict of intransigent belligerents is not illusion. It is all too real throughout our social structure, from the family circle to what we optimistically call the United Nations. But fortunately this is not the whole of the picture.

Under the surface of the recrudescence of

struggle for supremacy there is a strong undercurrent of genuine humanitarianism which plays the game of life according to different rules and for different objectives. More efficient socialization of the struggle for existence and for a place in the sun is what separates man from brute. Language and all our rational and aesthetic skills are the instruments employed. In the course of this struggle standards of decency, honor, consideration of the legitimate rights of others, and some measure of sacrifice have been slowly—very slowly—so deeply entrenched in human nature that no temporary or local reversions to barbarism and bestiality can long survive. Tyranny, whether on a small scale or large, has within its essential structure the elements of its own destruction. For this we have ample scientific evidence in the history of the evolution of human culture, and equally good evidence that faithful observance of well-conceived standards of honor, justice, and intelligently directed teamwork have actual survival value in our world today. This, too, is not illusion.

Human nature is basically sound, vigorous, enterprising, and decent. However various may be the standards of propriety among different races and peoples, some standards there always are, and in general people conform with the mores of the clan. But these social standards are not immutable, and the immediate task is a discriminating appraisal of past experience, especially of recent history, to learn which of the numberless patterns of social organization and practice that have been tried yield the social stability and opportunities for further advancement that we crave. This problem can be solved only by application of the scientific method, not by wishful thinking.

Our scientific explanations are never completely finished, and the meanings discovered are approximations, not ultimates. But they are the best we have, and past experience is our best guide. More knowledge will point the way to more successful application of this experience, but knowledge is not enough. We must learn to discriminate illusion from fact and then to interpret and utilize these established facts for judicious selection of the objectives that we want to reach and of the methods to be employed for their attainment. And to this knowledge we must add faith, courage, and the will to win what we must have if we hope to survive.

The Apostle John warned, "Judge not according to the appearance." Such a judgment can lead only to despair if Thomas Moore was right,

> This world is all a fleeting show,
> For man's illusion given;

for we have to adjust to this world somehow, and it is up to us to learn how to do it. Shakespeare's appeal,

> Here we wander in illusions;
> Some blessed power deliver us from hence,

was appropriately written in his *Comedy of Errors*. Our deliverance from illusion must be won by our own efforts, and science must point the way.

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PRIVATE FLIGHT TO THE BOSTON MEETING

By GEORGE V. CAESAR
Staten Island, N. Y.

DR. LANGMUIR learned to fly before I did. But if he attended the sixth Boston Meeting of the A.A.A.S. I doubt if he flew there. I think I was the only member who did. This at least seemed plausible when I crawled out of my little cabin plane at Logan Field in that icy gale of Thursday afternoon, December 26.

To begin with, I had no intention of flying up to Boston. I had planned to travel more conventionally—and in boredom—by rail from New York. But the unusually lovely weather on Christmas Day seduced me. The urge to fly, suppressed in winter, boiled to the surface. It seemed likely that the fine weather might hold over the week end. So I phoned the La Guardia Airways Weather Bureau for their opinion. It was less optimistic: a cold front was moving east across New England, marked by intermittent snow squalls and strong winds, due to pass over Boston Thursday morning. Nevertheless, "contact" flying conditions should generally prevail, and Friday should be "clear and visibility unlimited" (CAVU). Saturday was doubtful, Sunday also, but I could get more certain forecasts on Friday. Was I capable of negotiating pretty high winds in landing at Boston on Thursday? I thought so. Later I wasn't so sure.

The next move was to rent a ship. Most private pilots still find it economical to rent their flying time. It was short notice, but I managed to lease a 3-seater cabin plane of 100 h.p. at Hadley Field near New Brunswick, N. J., a safe comfortable little craft cruising at better than 100 mph on about 6 gallons of gas per hour. I retained my rail reservations pending a final decision early Thursday morning.

Christmas evening I pored enjoyably over the two sectional aeronautical charts of the U. S. Coast and Geodetic Survey covering the New York and Boston areas, scale 1:500,000 on the Lambert Projection, in which straight lines represent approximate Great Circle courses. A wealth of detail, all for the benefit of pilots, is engraved on these remarkable charts. I drew a pencil line from Bridgeport, Conn., to Boston, marked off 10-mile intervals, measured the true course by reference to the middle meridian, and added 14° for mean magnetic variation. This gave a sufficiently approximate magnetic course subject to the plane's compass deviation correction. It was unnecessary to plot the course for the 80-mile leg from Hadley to Bridgeport, the north shore of Long Island Sound providing a sufficient guide. From Bridgeport to Boston was 136 miles. As an alternate route, in the event of poor visibility, I proposed to follow the Sound shore further east to the neighborhood of Stonington, where the New Haven railroad bends inland, and guide on it to Providence, R. I., a course from Bridgeport of about 153 miles. The difference of 17 miles might be well worth uncertain groping in low visibility over a compass course. I fly for fun. Also, I want to be an "old pilot."

Thursday morning was misty. But the weather forecast remaining the same, I drove to Hadley from Staten Island, noting cheerfully that the pall of fog and smoke was thinning rapidly under a clear sun and spanking breeze. Not a sign of that front of cold air and the precipitation preceding it

as the warmer, moister air is thrust up. Soon I was skimming over New York City at 3,000 feet, a stiff wind from the left and enough of it on the tail to boost my ground speed to better than 120 mph. Visibility was excellent; I could pinpoint my position; Long Island Sound was indigo blue, very beautiful; I was warm and comfortable, relaxed, happy.

Then with a rapidity almost incredible the weather picture altered. From warm blue skies I was heading squarely into a dark gloomy wall of wind-torn stratus, its frayed bottom well below my level. I throttled back, put the plane's nose down and dove under the curtain, the long sinuous fingers of cloud reaching vainly for me. Here was the front. And the air in and around it was boiling. I eased the shock of the "waves" by slowing down to around 70 mph air speed and steering out over the water where the bumps were not quite so violent. A few minutes more and the visibility shut down in white driving lines of snow—not dangerously down, but I knew it would be wise to land before things could grow worse. The municipal field at Bridgeport was not far ahead near the tip of a long peninsula. Peering out a little anxiously, I soon spotted its huge dark cross, the white hangars, and the little pimple of the control tower, which I kept constantly in sight as I circled in a wide upwind approach. Without benefit of radio, I was waiting for the green light. It came: a bright cheerful wink, and I knew I was cleared to land. That warm air-lines office felt very homelike.

Airport hospitality is genuine and wholehearted. At large commercial fields, nonscheduled flying is a nuisance at best. But I have never failed to get prompt and courteous assistance when I needed it. At Bridgeport I was kept posted for more than an hour on the rapidly changing weather conditions as the front passed eastward. At 1140 hours I took off with Providence and Boston reported CAVU, wind shifting to NW and increasing. Bridgeport still had a solid overcast, but it was lifting and the snow had ceased. I decided to follow my alternate route. Pilotage would be easier and the bumps softer, at least until I had to turn inland.

However, I overlooked the possibility that I might overtake the front while skirting the Sound. And around New London I did catch up with it, snow squalls and all. But I knew that as soon as I could turn northward at Stonington I should be flying steadily into better weather. This was no mean comfort, as at only 500 feet above the rails I cut corners on their long curves over low hills dusting thickly with snow. On and on I flew, the wind a trifle ahead, the snow cold and dry and showing no tendency to stick. My position in the low visibility was uncertain but caused no worry. Somewhere ahead were Providence and CAVU, down below were the guiding rails, the real pilot.

And so they were. At around 20 miles south of Providence I broke abruptly into brilliant winter sunshine, a glorious sight and a glorious feeling, too. There remained only a 65-mile battle with the rising wind.

It was 1330 hours when my wheels touched down on Logan Field. I say "down," advisedly. I had to plant the wheels down with forward stick pressure and plenty of power. No 3-point landing would do in that wind! I dared not let the tail get down. Later I learned that the wind at ground level was 30 mph with gusts up to 50. I literally flew along the runway, only a few pounds of load on the wheels, until close to the control tower where I had something of a lee and could very slowly and carefully turn left toward a hangar storing light planes. I looked desperately for ground help. To climb out without a man at each wing tip was impossible. Help finally arrived, and my faithful but temperamental

steed was tied down securely. That wind was icy. But somehow I felt overheated as I walked to the hangar.

Flying time from Bridgeport via my longer route was 1:50, an average of only about 85 mph. Total time from Hadley was 2:30, for an over-all average of around 93 mph. Gas and oil costs were $3.92. The landing fee at Boston was $1.00; no charge at Bridgeport, as at most other fields.

A taxi took me to my company's branch office, thence to the University Club, where I was very hospitably put up. Walking to the Armory was an ordeal that afternoon, as those of us who arrived Thursday will affirm. The wind was still rising, and I gave thanks that I had landed when I did. After registering I spent the rest of the afternoon at the intensely interesting Science Exhibit, well worth the trip in itself. It was simply too frigid that evening, and the enticement of the Special Meetings insufficient to lure me from the warmth and comforts of the club. I dare say, if truth be told, I wasn't alone in finding excuses to stay indoors.

Next morning, Friday, was beautifully clear and—to me at least—bitterly cold: around 0°F. But the wind, fortunately, was only moderate. Still, the short walk to the Statler was hardly a pleasure. I attended the morning General Technical Session, listening to extremely interesting addresses by Vice Admiral Bowen, Dr. Waterman, and Rear Admiral Parsons, Director of Atomic Defense. Admiral Parsons closed his talk with motion pictures in color of Operation Crossroads. The underwater explosion, in particular, is indescribable. No one who saw those pictures can ever forget them.

Unhappily, I could not wait for the afternoon session. The weather forecast that morning was ominous for Saturday, Sunday, and probably Monday. I had to be back Sunday afternoon at the latest and could not chance it. A pilot, especially a private pilot constrained to fly contact, is a slave to weather. And so, after a quick lunch and another call at our office, I was back at the airport again, not a little apprehensive as to how difficult (if not impossible) it might be to start my engine after a night in zero weather.

My fears were justified. The hangar help, with kindest intentions, had tried to warm up the engine for me but succeeded only in running down the battery actuating the self-starter. There was only one thing left to try: get some willing husky to swing the prop the while I goosed the primer and muttered. The engine came to life eventually, and I was off at 1345 hours after taxiing for about half a mile in front of two huge airliners, who seemed to chase me like two fat geese after a little frog. Probably I annoyed them more than they worried me.

The return trip was uneventful until I neared New York. I flew a long route of about 275 miles, via Worcester, then southwest to intersect the Sound shore at Saybrook. The visibility was wonderful, and I wanted to see as much as I could of New England. From Worcester south I flew at around 500 feet above the hills, both to reduce a moderate head wind and enjoy the thrill of flight at low altitudes. Generally this is to be deprecated, but, sparingly indulged in and with perfect visibility, it is a great luxury. The air was as smooth as the waters of a sheltered lake. I sat as on a magic carpet beautifully responsive to the slightest touch of the controls, skimming in a beeline at 100 mph over lonely farmhouses, hills, valleys, frozen lakes, an occasional little town. One had the curious dreamlike feeling of a free disembodied spirit aloof utterly from all cares and responsibilities of the flesh. It is a spiritual hygiene to be thus alone, a state for which there is a desperate need in modern living. The love of flying and its flaming evangelism stem from this sense of isolation and freedom

which our pioneer ancestors craved, the outstanding characteristic of our race.

As I turned westward along the Sound, the high thin overcast began to lower and thicken. The forecast for Saturday looked correct, and I was glad to be going home. At 1530 I flew across New Haven harbor, the familiar towers of old Yale in the distance. There was but one hour more of daylight and close to 100 miles to go. I pushed the throttle nearly wide open. It would be a close thing, but if need be I could land at Flushing or Staten Island. Still, I ought to make it, in spite of the head wind which was increasing a little. But when I came opposite Stamford the thickening gloom ahead began to breed doubts. In any event I would have to pull up to at least 2,000 feet to fly safely in reduced visibility as I approached New York. But if I did, could I still see the ground? It was not clouds which I was running into, just smoke and light fog. I could sense the top of this "smog" belt overhead. I climbed slowly to 2,000. I was nearly "on top" but dared go no higher. I could only just see the indented shoreline. I would hold this altitude and course for a few minutes. If visibility got no worse I would push on; if it thickened the slightest bit more I would descend and fly back to Bridgeport. But the conditions held—no better but no worse—so I flew steadily on, guiding by that scalloped shoreline as Long Island Sound narrowed toward the East River. Without it I would have been lost instantly. For awhile it was unpleasant flying. I realize now that I would have done well to spend that night at Bridgeport and I am not proud at all of having got away with about 15 minutes of semiblind flying

I was somewhere opposite La Guardia Airport when the visibility suddenly extended. It was not unlike the opening-scene effect of a moving picture, and I never saw one I liked better.

I landed at Hadley at 1630 with a few minutes of daylight to spare, average ground speed about 100 mph. Private air travel is still in the pioneer stage. It has its drawbacks; I have not tried to gloss them over. But to me it is well worth its tribulations. I hope to fly again to another A.A.A.S. Meeting.

Book Reviews

SOCIAL RECONSTRUCTION

Research and Regional Welfare. Robert E. Coker, Ed. vii + 229 pp. $3.00. Univer. of North Carolina Press. Chapel Hill. 1946.

THIS is a volume of 14 papers on the need for, possibilities of, and methods of maintaining research in the interest of regional and national welfare presented as part of the Sesquicentennial Celebration of the University of North Carolina at Chapel Hill in May 1945 by "distinguished leaders in a dozen fields of action and learning." Included also are an address by the Honorable R. Gregg Cherry, Governor of North Carolina, "Research for the Commonwealth;" a foreword by Louis R. Wilson and an introduction by Robert E. Coker, both of the University of North Carolina.

The volume brings together a most fascinating array of facts and figures on the Southern problem as it relates to the national picture, weighed by the best minds in the fields of agriculture, industry, medicine, public health, social science, education, and government, presented at the hour of the South's greatest opportunity. Signposts of potential progress are set; a plan of action is proposed.

"Too long has the South been the dependent colonial province of a financial industrial empire reaching from Boston to Chicago," says President Frank P. Graham, of the University of North Carolina, in calling upon research to find the solution. Wilson Compton, President of Washington State College, adds: "The Industrial East has been the Mother Country. The South and West have been the colonies." He cites figures to show that the South is rich in raw materials which are being siphoned off elsewhere for fabrication to be sold back as manufactured necessities.

Wilbur A. Lazier, Director, Southern Research Institute, calls attention to a score of economic and social log jams in the South that research can dislodge and points to the possibility of bringing privately sponsored scientific personnel together for this purpose. And Raymond R. Paty, President, University of Alabama, sees hope: "Today the South is more research-minded than at any other time in history." He believes the problems of the small-town businessman need more attention.

On the subjects of health and humanities, such scholars as Russell M. Wilder, of the Mayo Clinic, James S. Simmons, Office of the Surgeon General, U. S. Army, D. S. Allen, Johns Hopkins University, and Avery Craven, University of Chicago, show what research has done and can do for the South. In his paper, "History and Social Reconstruction," Dr. Craven makes one of his many great contributions. He refers to the traditional culture and gracious living of Southern people, adding: "Their loss will be a nation's loss." He says:

> Happiness is not entirely a matter of things; it is more than prosperity. It has to do with a way of life and a set of values. Traditions cannot be ignored without cost, and the South by merely becoming like the industrial North will not automatically end all her troubles or gain all satisfaction. She might gain something of richness by looking backward as well as ahead.

Other papers are presented as follows: "Research and Industry as a Factor in Southern Development," by Milton H. Fies, Southern Research Institute; "Needs and Opportunitites for Research in Indus-

try," by Reuben B. Robertson, Champion Paper and Fiber Company; "Research in the Fisheries for the Betterment of the South," by Harden F. Taylor, Atlantic Coast Fisheries Company; "Research and the Southern Farmer," by George J. Wilds, Coker's Pedigreed Seed Company; "The Moral Responsibility of Research," by David E. Lilienthal, Tennessee Valley Authority; "Wartime Science Builds for Peace," by Georges F. Doriot, Office of Quartermaster General, U. S. Army.

PAUL D. SANDERS

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STARS IN THEIR COURSES

The Star Atlas and Navigation Encyclopedia. S. S. Rabl. 161 pp. Illus. $5.00. Cornell Maritime Press. New York. 1946.

THIS book, according to the introduction, is intended for the yachtsman with his first boat, the student-navigator, the amateur astronomer, and all who like stars and the sea. Obviously, a text of 161 pages cannot cover completely the practice and science of navigation and nautical astronomy, as the title suggests. It is, however, a volume from which the amateur or student can learn the essentials of piloting and celestial navigation. It is written in everyday language in a breezy style, which along with the double-column pages, wide margins, and leaded type, makes for inviting and comfortable reading.

The illustrations profit by the large format, 8½ by 11 inches. There are a number of full-page illustrations of star charts and the important constellations. The perspective drawings of the celestial sphere and astronomical triangle and the illustrations of latitude observations, circles of equal altitude, and the like are probably as good a picturization of these subjects as can be found in any textbook.

The first chapter deals with mathematics—simple arithmetic, logarithms, elementary trigonometry, and graphics. This last is dealt with at some length; the advantages of this method over old-time methods of taking into account current and wind in laying down a course are clearly explained.

Succeeding chapters deal with the essentials of piloting, the compass, nautical astronomy, old and new navigation methods, position plotting, the sailings, stars and star-finding methods, and, finally, an interesting, if somewhat fanciful, chapter on primitive navigation. The chapter on the use of bearings is accompanied by an illustration of a distance-off projector for bow and beam bearings whereby this most common of piloting problems can be solved without calculation.

The articles on the principles of construction of both Mercator and gnomonic charts are supplemented by several large, well-drawn diagrams, which should be a help to the student in understanding this important subject. The explanations of the line-of-position computations are likewise well illustrated. The article on the principle of the sextant is accompanied by an animated-cartoon type of drawing which can be understood at a glance.

An important part of the book seems to this reviewer to be the chapter dealing with Modern Navigation Methods. The discussion presents simply and clearly (illustrated by actual examples) the pros and cons of the various short-cut methods for solving the astronomical triangle that have been developed in recent years.

The practical navigator who may be a little hazy on nautical astronomy will find much in this text to help him and will immediately note the few inaccurate statements and impractical methods outlined. An example of the former, found under longitude determinations, is that each minute of time makes a difference of 15

miles, which is of course only true at the equator. The author devotes considerable space to the work of the navigator in observing stars throughout the night, whereas stars can only be used with assurance during about one-third of the twilight periods, a relatively short time. The statement that cross bearings are good only when the vessel is "standing still," if followed in practice, would deprive the navigator of one of his most useful methods of piloting.

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VICIOUS CYCLE

India's Population. S. Chandrasekhar 117 pp. $2.00. John Day. New York. 1946.

FOUR hundred million people are a lot of people. An increase of 50,000,000 people in 10 years is a big increase. Yet India's population is now more than 400,000,000, and it increased 50,000,000 during the 10 years preceding World War II. This is an increase in *one* decade of as many people as the total population of Great Britain.

In this informative and easily read book Mr. Chandrasekhar describes the suffering and the extremely low level of existence of one-fifth of the earth's people. Indirectly, the book brings to mind the living conditions of more than half of the world's people who live in Asia but who do not have even as reliable records as India.

This book has many interesting statistical tables and, in a sense, has the backing of such American population authorities as H. P. Fairchild, Frank Lorimer, Frank Notestein, and Warren S. Thompson, who wrote the introduction. It is divided into three parts: (1) Demographic Facts (facts which are shocking); (2) Public Health (of which there is practically none); and (3) Toward a Population Policy (which tries to explain a way out of an almost insolvable problem).

Here are a few of the cruel facts the author gives us. Some 40 percent of the girls married in India are below the age of fifteen. A widow, be she fifteen or fifty, is banned from remarriage. This means that about 12,000,000 women do not participate in active motherhood. Yet on the average, 200,000 mothers die every year during childbirth or from ailments connected with it. The author explains the causal connection between these factors and how the vicious cycle rolls.

The author tells of housing conditions of the great masses of Mother India which may be difficult for the average American to believe are physically possible; for instance, six families—30 human beings—living in one room 12 by 15 feet!

Despite a percentage increase in literacy in India, in 1931 there were 315,000,000 illiterates, and in 1941 the figure rose to 341,000,000. As in so many other cases, education just cannot keep pace with the birth rate. And if it were not for an extremely high death rate the problem would be even more difficult.

India, as almost everyone knows, has a very high birth and a very high death rate, but the official figures do not tell the whole story. When allowance is made for underregistration of births and deaths, both rates are about three times as high as those of the United States during "normal" years. And as India's population is about three times as large one can imagine the tremendous wastage of human life.

There is one almost unbelievable fact about India's birth and death rates which is suggested but not described in so many words. This fact is: If India's death rate could suddenly be lowered to the level of that of the United States or England,

with her present birth rate India could fill five earths as large as ours and as full as ours is today, in one single century.

In the concluding part of the book, the author shows the crying need for a rational program of population limitation, soil conservation, reclamation of wastelands, irrigation, industrialization, and a more just distribution of wealth.

One newspaper reviewer is quoted on the book jacket: "One of the great illusions of the twentieth century—that India's burden is too many people—is quickly and expertly destroyed by Mr. Chandrasekhar." The author is a keen thinker and a good writer, but he is hardly a magician. To share the wealth equally in India and at the same time reduce the death rate without reducing the birth rate would indeed produce equality—equality in poverty. While India's high death rate certainly needs to be reduced as quickly as possible, there will be little real progress unless the birth rate is reduced even more quickly.

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WILDERNESS SHANGRI-LA

Driftwood Valley. Theodora C. Stanwell-Fletcher. ix + 384 pp. Illus. $4.00. Little, Brown. Boston. 1946.

NEARLY everyone at some time dreams of going into the wilderness and staying there "away from it all." Few do, and fewer still either enjoy it or get anything out of it, except perhaps a book of adventure describing their good or their unpleasant times. Mrs. Stanwell-Fletcher and her husband, the latter recently recovered from a bad accident, chose a spot on the edge of unsurveyed territory in northern British Columbia, more than 200 miles from a railroad and 30 miles from their nearest Indian neighbors.

A cabin was built to their own specifications, with help from Indians and from a Danish prospector who just happened along. The reader lives with the couple from August 1937 through autumn, winter, spring, and the following summer. An occasional visiting Indian dropped in, coming long distances on snowshoes; sometimes they were loquacious, sometimes silent, but always they had an appetite for huge quantities of hot tea. In the summer of 1938 the author's father and mother came for a two-month visit. Once a plane was forced to land on Lake Tetana, on the shore of which the cabin stood, and two dapper city men, surprised to see a white woman in this part of the world, stayed until the plane could take off again.

There were numerous side trips, each of them a major expedition as far as the Stanwell-Fletchers' experience went. They camped in the bitter weather, as cold as 40° or 50° below zero, stopping under a tree and cooking food that sometimes froze before they could eat it. They fished, hunted game, and collected natural-history specimens to be sent to the British Columbia Museum.

Then by snowshoe, dogsled, and plane, they came out to the civilization of St. George after a year and a half of cabin life. After a short vacation, believe it or not, they returned to Tetana and spent another year, this time partly in the high hills hunting mountain goats.

The book is in journal form, and the author has painstakingly set down voluminous details of the life they led and the food they ate, with observations on the birds and beasts that were their close neighbors. Migrations of birds and animals were watched and recorded. The ducks on the lake, a flock of trumpeter swans, the jays about the cabin, are described, and there are some beautiful passages on the timber wolves and their behavior.

It takes continuous work just to keep

alive in this far northern country, and yet in the appendix there is an astonishing list of a whole museum-full of specimens that were collected and either sent back or taken to the museum.

This is a book for leisurely reading, not one to be rushed through, and it will be enjoyed by those who have been in the outcountries, by those who want to go, and by those who fit into neither of these categories. Not the least of its interest lies in the numerous attractive sketches of animal life done by Mr. Stanwell-Fletcher.

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TREES AND VINES BY PLINY

Natural History. Pliny. Vol. IV, Books XII–XVI. Translated by H. Rackham. 556 pp. $2.50. Harvard Univ. Press (Loeb Classical Library). Cambridge. 1945.

THIS latest portion of Pliny to be translated for the Loeb Library is also the last by H. Rackham, late fellow of Christ's College, Cambridge, who died while revising the page proofs. Mr. Rackham's loss will be keenly felt, for he was doing a fine job with Pliny, and there are still six volumes of the *Natural History* to be translated for this series. This fourth volume of Pliny comprises the books on trees and vines and is a self-contained unit. One might say, offhand, that botany could not offer Pliny much scope for the fantastic lore so evident in his treatises on animals, but such a person does not know Pliny. There are, for example, the bush that will turn into stone if taken unawares and the submarine forests of the Red Sea. This volume is primarily, however, a treatise on the economic botany of the Roman world, especially the vine and its delectable products, the olive, and the other fruits beloved by the Romans. As such, it should appeal to all botanists, connoisseurs of wine, and *bon vivants* in general.

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FRUIT AND VEGETABLE CROPS

General Horticulture. Thomas J. Talbert. 452 pp. Illus. $4.00. Lea & Febiger. Philadelphia. 1946.

A REWRITTEN and expanded version of *Fruit Crops* (published in 1939), *General Horticulture* is the outgrowth of 30 years of teaching and investigational work by the author at the University of Missouri. It is intended as a beginning undergraduate text for schools and colleges.

Nearly 300 of the 452 pages of the book deal with North-Temperate deciduous fruits, but short chapters have been added on vegetable crops, on beautifying the home grounds, on tropical and subtropical fruits, and other general horticultural topics so as to broaden its scope. Nevertheless, the principal treatment is of the North-Temperate deciduous fruits.

The subject matter is complete so far as fruits are concerned. There are few, if any, important phases which are not covered. The treatment of each of the many topics is of necessity brief, but adequate without becoming involved. If there is any criticism, it lies in a too brief discussion of vegetable crops, which have now become so important in horticulture.

Throughout the presentation, the horticultural point of view is dominant. Practical considerations and the real purpose of horticulture in providing food for mankind and in providing a living for the producer are kept constantly in the forefront. Selected references at the end of each section make it possible to pursue any particular subject in greater detail.

The printing is well done, the type is

sufficiently large for easy reading, the illustrations are clear and well chosen, and the binding is good—in general superior to the make-up of many books that appeared during the war years.

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AN INTERPRETATION OF WEBER

From Max Weber: Essays in Sociology. Translated, edited, and with an introduction by H. H. Gerth and C. Wright Mills. xi + 490 pp. $5.00. Oxford Univ. Press. New York. 1946.

MAX WEBER (1864-1920) was one of the great scholars of Germany. He

> ... belonged to a generation of universal scholars, and there are definite sociological conditions for a scholarship of the kind he displayed. One such condition was a gymnasium education, which, in Weber's case, equipped him in such a way that the Indo-Germanic languages were but so many dialects of one linguistic medium.... Urbanism, legal history, economics, music, world religions—there is hardly a field which he left untouched. He thus continued the tradition of encyclopedic scholarship of Wundt and Ratzel, of Roscher and Schmoller (pp. 23, 44).

With these qualifications and interests he proceeded "to write social history in the grand manner." During Weber's time this type of wide-ranging scholarship frequently passed for sociology for want of a better definition of the latter term. Actually it was a philosophy of history. It is therefore an error, so far as sociology is concerned, to say, as does the blurb on the book jacket, that "over the last fifteen years [Weber] has become a leading influence in American thought." For American sociology during the past fifteen years has turned away from the philosophy of history and increasingly has taken its cue from the other sciences, to its very great advantage from all points of view.

The above characterization of Max Weber's place in the intellectual world is in no way a detraction from his contribution or from that of his translators. He displayed vast erudition and many a penetrating insight. Among other things, he showed an understanding of the difference between social science and propaganda which many physical and social scientists today seem unable to grasp. Weber was quite explicit on the subject as, for example, in the following passage:

> To take a practical political stand is one thing, and to analyze political structures and party positions is another. When speaking in a political meeting about democracy, one does not hide one's personal standpoints; indeed, to come out clearly and take a stand is one's damned duty. The words one uses in such a meeting are not means of scientific analysis but means of canvassing votes and winning over others. They are not ploughshares to loosen the soil of contemplative thought; they are swords against the enemies: such words are weapons. It would be an outrage, however, to use words in this fashion in a lecture or in the lecture room. If, for instance, "democracy" is under discussion, one considers its various forms, analyzes them in the way they function, determines what results for the conditions of life the one form has as compared with the other. Then one confronts the forms of democracy with non-democratic forms of political order and endeavors to come to a position where the student may find the point from which, in terms of his ultimate ideals, he can take a stand. But the true teacher will beware of imposing from the platform any political position upon the student, whether it is expressed or suggested.... One can only demand of the teacher that he have the intellectual integrity to see that it is one thing to state facts, to determine mathematical or logical relations or the internal structure of cultural values, while it is another thing to answer questions of the *value* of culture and its individual contents and the question of how one should act in the cultural community and in political associations. These are quite heterogeneous problems.... The task of the teacher is to serve the students with his knowledge and scientific experience and not to imprint upon them his personal political views (pp. 145–46).

The first 74 pages of this book are devoted to a biographical sketch of Max Weber and a brief account of his intellectual orienta-

tions and approach to social phenomena. The remainder of the book consists of translations of selected essays from Weber's work, principally on the subjects of the power structure of society and the subject of religion. There are also chapters on Politics and Science as vocations and a concluding section on Social Structures, mainly of India and China. Twenty-two pages of notes and an Index conclude the volume.

The main purpose of translations is to make available to a larger audience ideas otherwise restricted to a smaller public. From this point of view, if the ideas are of any importance at all, translations are to be welcomed. Especially is this true when the translation is from the German, for, as Mencken once remarked, German is the only language that seems to have been constructed with deliberate malice. This dictum could perhaps be well illustrated from the German of Max Weber. As the translators point out in their preface, his sentences are "Gothic castles;" "mental balconies and watchtowers, as well as bridges and recesses, decorate the main structure." Readers will therefore gladly pardon the translators for having reconstructed with a free hand this heavy and cumbersome architecture. I suppose there will be the usual quarrels as to whether the result in all respects truly represents the original author's meanings. The present reviewer professes no competence to pass on such questions because there often is really no way of determining in "literary" writing of this sort exactly what the author did mean. Although the present text is still in many places unnecessarily long-winded and abstruse, the present volume is doubtless the most readable translation and interpretation of any of Weber's work now to be found in English.

Whether the material translated is worth the effort will again be a matter of opinion with which this review cannot concern itself to any great extent. The distinction of Weber in his time and in the fields in which he worked have been noted above. Insofar as the appearance of the translation at this time represents another of the periodic "discoveries" of some European author, usually after his work is obsolete, and an attempt to stage a revival over his remains, these efforts will be pretty sterile. This is no reflection upon Weber, for, as he himself said, "In science, each of us knows that what he has accomplished will be antiquated in ten, twenty, fifty years." Certainly the same applies to philosophies of history. After the 25 years and more since most of Weber's writing was done, I believe Weber would be the first to admit the applicability of this statement to his own work, especially as far as sociology is concerned. I think I have all due regard for the insights and prodigious labor of men like Pareto and Weber. But I think they had been thoroughly superseded in this country by the time their words became generally known. Nevertheless, the translators deserve praise for their devoted labors in bringing out a handsome book in a form in which the English-reading public may judge for itself the importance of Weber's contribution and, incidentally, the justice of the present appraisal of it.

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THE GOOD EARTH

Forest Soils and Forest Growth. S. A. Wilde. xx + 241 pp. Illus. $5.00. Chronica Botanica. Waltham, Mass. G. E. Stechert. New York. 1946.

FOREST soils cover about one-half the total land area of the globe. "Forest soil is the medium that produces nature's most magnificent crop. . . .Since time immemorial the daily needs and inborn inquisitiveness of mankind have stimulated interest in the soils of the forest."

In *Forest Soils and Forest Growth*, Dr. Wilde has summarized his exhaustive review of the world's literature on forest soils. He brings to the reader all the rich store of accumulated Russian soil and forest lore. Probably nowhere else can be found in one book such a concentration of references, abstracts, and summaries of forest soil literature. In this book he has recorded also the essence of his years of experience in teaching and research in Europe and America.

One outstanding quality of this book is its clarity. The book is true to its title: The author leads the reader through the necessary applications of chemistry, physics, biology, hydrology, and ecology; but he retains coherence and unity by never straying far from the central theme, forest soils.

Another refreshing quality of the book is its courageous originality. The author does not hesitate to suggest constructive changes in, and additions to, present accepted usages. For the expression "humus incorporation into the mineral soil in the formation of a mull," he presents a new word, "melanize." Melanize is derived from the Greek word *melas*, meaning "black" or "dark." The sound of the word suggests mellow—the exact quality of a good mull soil.

The text is systematically arranged. From an introductory setting including the history of the rise of the science of forest soils and the general nature and importance of forest soils to silviculture and land use, the author develops his theme. Under Genesis of Forest Soils he explains the processes of melanization, podzolization, and gleization. The reader learns how a mull, a podzol, or a gley soil is formed and why each develops under prevailing conditions. The author explains the relation of climate, topography, and parent material to soil groups.

Forest soils are treated in two major groups, upland and hydromorphic. The author's separation of soils according to leaching and gleization makes the genetic soil groups of the world easily understandable.

At length, having classified the soil groups, the author explains the relations of trees to soil. Chapters on physical and on chemical properties of soil, soil organisms, and forest humus widen the reader's perspective.

The text moves from the simple essentials of a forest soil to the relations of soil to trees and of trees to soil and then to practical application of the science of soils to silviculture. The author clearly explains the necessity of forest soil surveys and site evaluation in advance of attempts at afforestation.

Finally, soils and tree planting, silvicultural cuttings, nursery practice, fertilizing, and parasitic control in forest nurseries complete the book.

Original drawings throughout this book make easy reading. Little woodland sketches at the chapter ends add an intimate artistic touch to the general excellence of the text.

JOHN T. AUTEN
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Comments and Criticisms

ZENO AND THE MATHEMATICIAN[1]

> Achilles had overtaken the Tortoise, and had seated himself comfortably on its back. . . .[2]

Mathematician: You here, Zeno? A pleasant surprise, indeed, as I can now ask you personally what you meant by your paradoxes. Was it jest or was it serious when you said a flying arrow could not move?

Zeno: Newcomers in eternity are always surprised—because they did not acquaint themselves with our philosophy when they were still in existence. I am just as real now as I was when my existence appeared to the experience of others; and whether my disappearance from the experience of others has taken place a long or a short time ago—a long or a short past with reference to their own times[3]—makes absolutely no difference as to my being. I am.

Mathematician: I would never have thought of that when I was on earth—I was so busy measuring changes, movements and velocities of bodies that I never had time to think—but here I am compelled to admit that you are; I could not have started this discussion with you if you were not. But now, what about your unquenchable paradoxes? I am eager to inform you that we mathematicians have solved them by means of the calculus of convergent series approaching the limit symbolized by zero; and instantaneous velocities are a commonplace in modern physics—we read our speedometers every day, you see. You must enjoy the news that your problem has been solved, even though we ourselves do not understand how we do it: whether there is an actual infinity of numbers in such a series and whether a limit is actually reached or not. Still, we do it. We have made progress. The motionless arrow only seemed a problem to you because mathematical methods were so crude in your time.

Zeno: You advanced mathematicians seem to me no less naive than the simple mathematicians of my time. They said the same thing and also solved the paradoxes *ambulando*. They simply did it. As Lewis Carroll, a good friend of mine, put it, they would better have taken to football than to thinking I am delighted, of course, that your methods of measuring relative velocities are so nicely perfected now and I celebrate with you such improvements in this art. In Greek we call this *technê*. But my paradoxes, you remember, were written in defense of Parmenides. They were never merely mathematical riddles. If they were only that, they would not be paradoxes.

Mathematician: What is a paradox?

Zeno: Understand, friend, that in asking this question you move outside your calculating habits. And when I say "outside" I do not mean a geometrical proposition, but a different logical question. Now, if you have made your mind up beforehand that only mathematical riddles represent serious questions, then you had better abstain from pursuing this further. You are now warned!

Mathematician: I don't know what you are driving at. But for the fun of an argument, go ahead; I am ready to listen.

Zeno: That is something, but I am afraid it is not enough. Nevertheless, may I take your word that you are willing to consider my argument as an hypothesis?

Mathematician: Yes.

Zeno: Your "hypothesis," then, that I am, must be either true or false.

Mathematician: Do you mean to say that some people have verified your existence in their experience, and have talked about you?

Zeno: That is another hypothesis which must be true or false. How do you verify it?

Mathematician: I myself experienced that other people refer to you as having existed.

Zeno: That is another hypothesis. We need, it seems, quite a "number" of them. And the more you examine them in detail, the more prolific and the more uncertain they become. But take as many and make them as detailed as you please, do all of them prove that I am?

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[1] Court, Nathan. The Motionless Arrow. *Sci. Mon.*, **63**, 249, 1946.

[2] Carroll, Lewis. *What the Tortoise Said to Achilles*. Complete Works, 1225. Modern Library.

[3] Mueller, Gustav E. Experiential and Existential Time. *Philosophy and Phaenomenological Research*, **6**, (3), 1946.

Mathematician: Of course not. All of them only sum up to this that I read, that other people read, that still other people (including yourself) had an experiential impression of a man named Zeno.

Zeno: Yet all the while you tacitly presuppose that you and all the others and I myself *are*. And this ontological presupposition of all the many and fluctuating empirical hypotheses remains one and the same for all of us, regardless of whether we lived earlier or later in time or whether we found ourselves located in Elea, Italy, or in Norman, Okla. To verify this one and same Being experientially is impossible, because you and I and everybody must *be*, before we can start having experiences in time or before we can make abstract experiential calculations of temporal data. "Being" *logically* precedes temporal existence and temporal experience, and it is the one and same Being logically "in" all those times, take them as long or cut them as short as you want to. Being is "in" all experiential and existential movements but does not move.

Mathematician: You use undefined terms, and your argument, therefore, makes no sense to me.

Zeno: As long as you do not say it is nonsense, you sit perfectly safe in the dubitability of your earthly memories. Otherwise, if you say either "It is nonsense" or "Only my earthly memories are true," you would end up in saying that "I am" is nonsense—and that would be unconditionally paradoxical, don't you think?

Mathematician: I don't know since you have not defined your paradox.

Zeno: Excuse me, here comes Parmenides who maintains that experience is the seat of all errors. He may enlighten you better than I can who am only his admirer May I introduce you? Venerable father, here is another one of those mathematicians who complains that we have not told them what the paradox is.

GUSTAV E. MUELLER

The University of Oklahoma

PARAPSYCHOLOGY

Only just yesterday did I get as far as "The Brownstone Tower" in your August number; and, as I pondered over Table 1, an idea, or what seemed to be such, struck me with sufficient force to induce me to write you this letter.

The idea is that your assumption that members prefer articles relating to their sections is not well founded. It seems to me that, in SM, they like to find what they can't get in their own technical journals. I am classified under M; but my interest in SM is not because I expect any engineering articles in it. Almost anything but engineering is what I am looking for; because SM, to me, is a source of general information not merely about various subjects but also about what people are thinking and doing about those subjects. Sometimes they are subjects that I did not even know had been conceived; and then I am indeed delighted.

An article that would especially interest me would be one on psychic research or, rather, *about* psychic research. I mean it would not be an investigation of "psychic" phenomena but an investigation of the investigation of such things. For, you know, there *is* something going on over there in that field; and I cannot satisfy myself that they are all crazy.

RUDOLPH H. KLAUDER

Ocean City, N. J.

ATOMIC WAR

Discussion of the atom bomb in connection with war sounds fantastic, and equally so ideas to avoid this catastrophe. Yet Russell and Wells have both said such a war will be the end of civilization as we know it; and within the month well-known men, including educators, have told youth there is no chance for a one-world government in this generation, one saying, not until after the next and possibly still another war. The ideas of control, if I am not mistaken, seem to lie in agreements to stop research, disclose mineral deposits, submit to routine inspection; and for us to get rid of our stockpile. Yet this is no solution. It merely strips us, on the word of others, and depends on whether or not we (and they) are clever enough to formulate a spy system to make it stick. So the fear will be constant. In this dilemma the old adage that "like cures like" may be recalled. Army ideas and plans of concealed forts bearing bomb-laden rockets, trained for instant dispatch on any world capital, are no secret. They were outlined in a recent popular magazine. If, therefore, it were known such exchange bases and messengers of destruction were constantly alert through the world, no one would be first to press the button, no matter what the provocation. Indeed, were a system of confirming the offender ready, perfectly worked out, all other nations could join in immediate retaliation, which would certainly settle matters for such a nation. We will have to meet the fear by accepting it boldly and equally with materialistic science—not words on paper. Nor would any prior agreement to such action be required. We could do it and let it be known, then give our data

to responsible United Nation members. A plan for knowing who pulls the switch first and when would thus become a matter of mutual interest, and quickly. I am unable to conceive of any other possible plan simply because, if we keep the secret, it will come out anyhow and leave us as naked, then, as any other nation. A drastic threat to the very existence of humanity requires heroic measures.

H. E. WOODCOCK

Chicago, Ill.

JUSTLY CYNICAL

While it is true that the experimental aspect of science and its data, theories, and applications justify *Homo sapiens* in possessing a fair amount of self-respect, any pride regarding a general understanding of Nature, or even a reasonable theory of it all, is a delusion of grandeur only

It can be said without risk of serious contradiction that no published theory, hypothesis, assumption, or guess about the essence of Nature has ever shown better validity as a fundamental explanation than that once proposed by Arrhenius for the *origin* of life upon the earth, namely, that of being the result of germs having been wafted from some undermined outside source by the pressure of light. Every attempt to describe a creation or beginning for the universe or a galaxy starts out with assuming a *fait accompli*, and as for fundamental maintenance no explanation proposed on a materialistic basis has ever been plausible, even when advanced with great academic authority.

The not very ancient hypothesis of an electromagnetic ether associated with matter, as it was concurrently understood, is well within the memory of all but the youngest scientists. The theory at the time was not considered to provide for gravitation, although with the great gain in experimental evidence an ether could be inductively described that would do so. The mere fact that the electromagnetic ether hypothesis was difficult to handle mathematically in itself did not justify its abandonment. A simpler mathematical treatment of the data has lead to great confusion, complexity, and contradiction in major theories. It has been recently admitted by competent authority that no progress has been made toward a unified general theory in the past six years and, inferentially, that the trouble is not so much due to the intricacies of a hypothesis of a nonmaterialistic universe of potential superenergy with the abandonment of the present concepts of matter as such, as that the very character of existing mathematics disqualifies it in attempts to penetrate the present scientific "stone wall." It has been said that what is needed is another Newton and a new kind of mathematics. Let it be hoped that a possible newly organized logic of sufficient power to cope with the situation may not be handicapped by the name mathematics.

CHESTER B. DURYEA

Bay Shore, N. Y.

IMPORTANT BOOKS

May I point out a bibliographical inaccuracy in J. J. Schifferes' interesting article in the September issue of the SM? On page 237, under the heading "The Ten Most Important Books in the History of Science," he lists as item 3 the "*Novum Organum* (The New Atlantis). 1620, Francis Bacon. . . ."

This must be a slip of the pen, since the *Novum Organum* and the *New Atlantis* are different works. The *Novum Organum* (1620) is of great importance in the history of science since it is the first attempt to explain a kind of inductional method for the sciences. It is written in the form of a series of aphorisms but is really a systematic treatise. Much of it is devoted to a minute analysis of the method as applied to an investigation of the nature of heat. The *New Atlantis*, on the other hand, is a fragmentary Utopian romance, first published posthumously. Although it contains a brief fanciful sketch of how scientific research would be organized in the perfect state, it is of no importance whatever in the history of science.

RUFUS SUTER

Washington, D. C.

DISAPPOINTMENT

Frankly, and only with the thought of constructive criticism, I must tell you that I have been much disappointed with the editorial content of SCIENTIFIC MONTHLY. There is too much mediocre material in it and too much that requires a long stretch of the imagination to connect it with science.

With the scientific research laboratories of our great educational institutions and our various industries making real progress in science in many directions, there should be available a vast amount of material of really scientific nature to make a monthly publication worthy of this great Association. Your "Science on the March" is a fine title and suggests just what we want, but it does not begin to keep us posted on the progress of science.

J. M. WATSON

Takoma Park, Md.

The Brownstone Tower

THOSE who could not attend the recent Boston meeting of the A.A.A.S. probably read about it in the newspapers during Christmas week. About 60 science writers and reporters were in the press room at the headquarters hotel writing stories of the meetings from manuscripts and abstracts of papers that were submitted in advance by their authors. It would be interesting to know what these reporters chose to write about. They are skilled in recognizing stories of potential interest to the public, and their choice might help to guide the editor of the SM in his search for interesting articles. Because no arrangement was made with the reporters present to get this information, it would be possible now only to find out what the newspaper editors chose to publish of the copy submitted to them. It would be interesting also to analyze the geographical distribution of the stories on the meetings in cities of, let us say, more than 100,000 population, in terms of total space allotted and that given to certain stories. No doubt the meeting was most fully reported in Boston, but what of other cities?

I had hoped to make some such investigation in time to report my findings in this issue, but was too busy to do it. A principal article may be written on this subject later.

Aside from conversations with old friends and contributors to the SM, I had two memorable experiences: First, I heard Admiral Parsons talk about Operation Crossroads at Bikini, certainly the most elaborate and spectacular experiments ever made, and I saw the Kodachrome movies of the two tests. Second, I attended a meeting of the A.A.A.S. Council in the Faculty Room of University Hall, which stands in the center of the Harvard Yard. The Yard was covered with crusted snow that night. Arriving early, I walked alone through that Colonial campus, the darkness broken here and there by house lights and the gleaming ice near them. It was a privilege to see the old faculty room of Harvard. There were the likenesses in portrait and statuary of many of Harvard's great men, including two of her famous scientists, Asa Gray and Louis Agassiz. President Conant had only to walk across the Yard from his home to preside at the meeting at which another great Harvard man, Harlow Shapley, was elected president of the A.A.A.S. for 1947. Harvard and Yale divided the honors that night: Shapley and Mather of Harvard for president and member of the Executive Committee, and Sinnott and Baitsell of Yale for president-elect (1948) and member of the Executive Committee, respectively.

The retiring president, Dr. C. F. Kettering, called attention to the SM in his address at Symphony Hall. He said:

> It is often just as important for the business man, the lawmaker, and the general public to know of discoveries in the field of science as it is for other scientists to know of the technical details. . . . Science will be advanced furthest when its results and problems are better known and understood. *Our* SCIENTIFIC MONTHLY publishes less than 15,000 copies. . . . We should aim at a circulation of several hundred thousand.

Note the word "our," which I have italicized. If the members of the A.A.A.S. will regard the SM as their own medium for reaching the public, if they "can or will write it so that the average man or student can understand," we shall be on our way to the circulation envisaged by Dr. Kettering and the consequent advancement of science among the educated public.

F. L. CAMPBELL

THE SCIENTIFIC MONTHLY

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THE FLAMINGOS OF ANDROS

By PAUL A. ZAHL*
Haskins Laboratories, New York

MOST grotesquely beautiful of birds, flamingos, when approached in their native haunts by an intruder, suddenly sweep into the air like a thousand jets of flame. When seen from a distance, a rising flock may look like a quivering sheet of red fire boiling up out of the surface of a lake. These take-offs are accompanied by rasping, stentorious, gooselike honks coming simultaneously from a thousand angry or frightened throats.

Such a spectacle is not to be found in parks and zoos, where flamingos stalk about with cool superciliousness. Captive birds have had their wings cut and cannot fly, and in confinement they quickly lose the delicate beauty and nervous vitality of wild flamingos. To observe the real thing, one must go deep into the tropics and seek out the flamingos in their native rookeries. Even that may soon be impossible, for the magnificent flocks once found in vast numbers on some of the islands of the Caribbean appear to be dwindling. The shy flamingo has always sought the most isolated locations for his home, and now that airplanes streak through the Caribbean sky, the solitude of the mangrove swamps is no longer secure.

Some authorities fear that the fate which overtook the dodo 300 years ago will eventually overtake the flamingo. For example, the great rookery on Andros Island in the Bahamas which 10 years ago had a population of over 30,000 flamingos, has now been entirely deserted, with only the ghostly remnants of myriads of disintegrating nest mounds remaining as mute evidence of the existence of the former bird-city. The vast flocks are gone, and only sharks and herons inhabit the environs.

Andros Island, lying north of Cuba and off the southeast tip of Florida, is the flat top of an immense submerged coral mountain. Its mesa lifts slightly above the surface of the deep, warm Caribbean, but large portions of it are still a few feet below the surface, giving rise to dangerous shallows. Everywhere underfoot is jagged

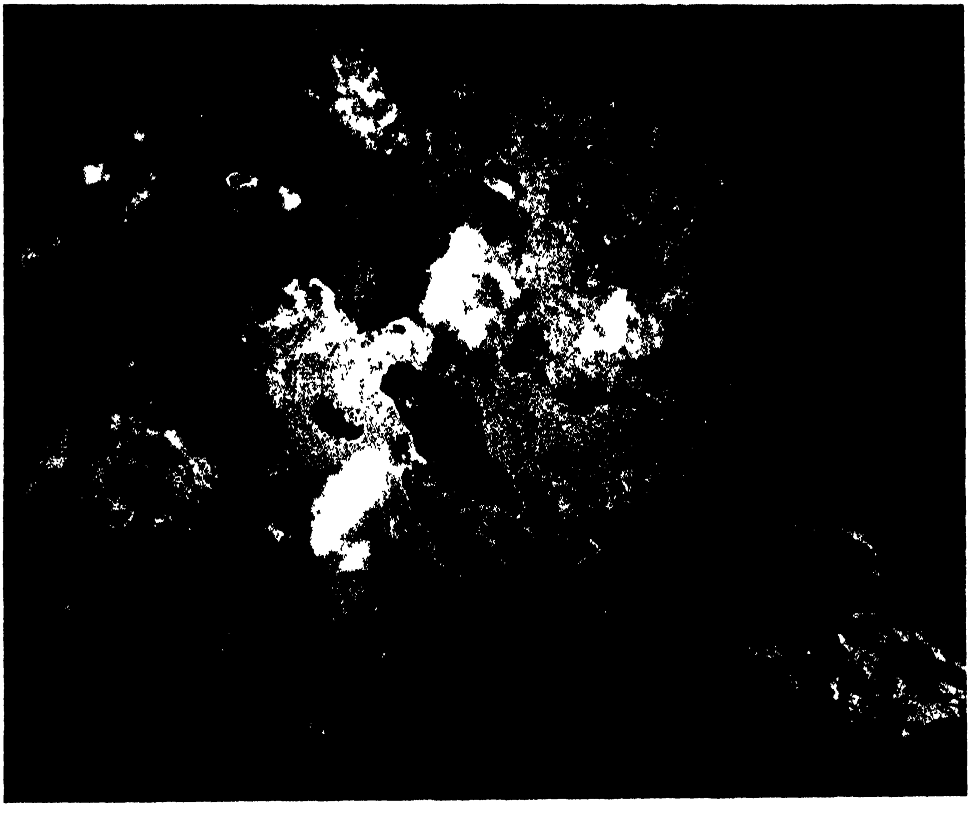
* The author wishes to acknowledge his respect for, and indebtedness to, former Commissioner of Andros E. W. Forsythe, O.B.E., whose pioneering conservation efforts in behalf of the Bahamian flamingo deserve wider recognition and continued support. Grateful thanks, too, are due to various members of the Anglo Bahamian Petroleum Company, for their many kindnesses during certain phases of the expedition, and to Duane Featherstonhaugh, who accompanied the author on this trip.

and treacherous honeycomb coral rock, and one must wear thick soles or have thick skin on his feet.

A hundred miles long by 50 miles wide, Andros, an old buccaneer refuge, is now inhabited on its east coast by a scattering of black natives, who engage off and on in fishing and in sponge, conch, and coconut collecting. These inhabitants, a jolly, happy lot, live a slow and langorous life, in spite of the fact that the skies and waters off the southeast of Andros constitute the womb of the wild hurricanes which in late summer are apt to rip up the east coast of the United States. Each night the natives, by latching all doors and shutters, close themselves almost hermetically into their white coral-rock huts—but not to keep out the winds or mosquitos. It's the spirits which are kept out by such measures.

That's the east coast. The west coast, the interior, and the southern third of the island are uninhabited and have only recently been explored. Up in Nassau, Andros is known as the Outer Island "where the flamingos are," although not more than a handful of Bahamians, and as few foreigners, have ever gone to the trouble to seek them out.

Flamingos nest in late May and June, just when the rainy season is beginning. Since all birds are most interesting during the nesting season, that's the time we



Courtesy of the Hydrographic Office, U. S. Navy

THE GRASSY CREEK REGION OF SOUTHERN ANDROS ISLAND

THIS AERIAL VIEW SHOWS CAYS AND SHALLOWS IDEAL FOR THE FORAGING AND BREEDING OF FLAMINGOS.

planned for our trip. We also had other biological objectives on Andros, but they are irrelevant.

Between breeding seasons wild flamingos are unapproachable, flying off skittishly when they see anyone approaching them even from great distances. Stories are told how native poachers, spotting a single bird feeding in the middle of a shallow bight, will carefully observe the length of time the bird's head stays under water. In feeding, the flamingo submerges its head for, say, ten seconds, during which time its beak sieves the coral sand of *Ceritheum* mollusks. Then up comes the head, the bird looks about for intruders, and into the water again goes the head for another assault on the mollusks. The bird is disturbed only by moving objects. The native takes advantage of this. Each time the flamingo puts its head under water, the huntsman, carrying a mangrove branch, runs forward. Then, just before he expects the bird's head to come up, he crouches into the water, holding the mangrove branch as a blind. This up-run, down-quiet process is repeated ten or twenty times or until the native is close enough for the final rush. The technique can, of course, be applied only to single birds, for when in groups—which is most of the time and especially during the mating and nesting season—some birds are always on guard while the others are feeding. The slightest movement, even half a mile away, will alarm the sentinel and will send the entire flock skyward and over the horizon.

We left Nassau by sailboat and landed at Mars Bay, a palm-studded settlement on the east coast of Andros about 30 miles north of the lower extremity of the island. From here in a sailing dinghy, we proceeded down the coast, turning inland at what is known as Grassy Creek. This term is a misnomer, for not only is there no grass, there is also no creek. In fact, the lower part of Andros is not even an island, but consists of thousands of coral masses extending a few feet above the surface of the water and interlaced by shallow salt-water channels which often widen out into huge salt-water lakes. On the coral sand bottoms of these shallows live tens of millions of *Ceritheum* mollusks. These are tapering, spiral-shelled organisms that constitute the sole diet of the flamingos. When we spotted the *Ceritheum* we knew we were getting into flamingo country. Occasionally, as we made our way inland, the fin of a curious shark would cut the water beside us. At first exciting, this sight soon became commonplace.

CERITHEUM MOLLUSKS 4

OCCURRING BY THE MILLIONS ON THE CORAL SAND BOTTOMS OF THE ANDROS SHALLOWS, THESE ORGANISMS ARE THE EXCLUSIVE DIET OF FLAMINGOS

Since this part of Andros is unmapped, we did not know exactly where the rookery was supposed to be; but from instructions received in Nassau from E. W. Forsythe, former Commissioner of Andros, and from some vague knowledge on the part of our

boatman, we knew the location approximately to within a 5-mile circle. So we set up camp as close to the center of this circle as we could.

Our first sight of flamingos came the very day we were setting up camp. In the late evening sunlight we saw streaking southward a line of 8 huge birds—not pink, as were the flamingos we had seen in zoos, but vermilion, with black wing coverts. These creatures silently and swiftly drew a line of red across the sky. Head and neck straight out in front, long legs heavily aft, and flying with the wing movement and the formation characteristic of the goose family, the octet soon disappeared over the horizon. We learned later that this habit of a flying flock suddenly to disappear in the distance was not due to rapid flight nor to their settling in the water, but rather to a penchant, characteristic of many members of the goose family, for swooping down from an elevation of, say, 200 feet to one of about 2 feet. Flying thus so close to the water that their wing tips actually skim the surface, an illusion of disappearance is created which for a long time puzzled us and led to much useless searching.

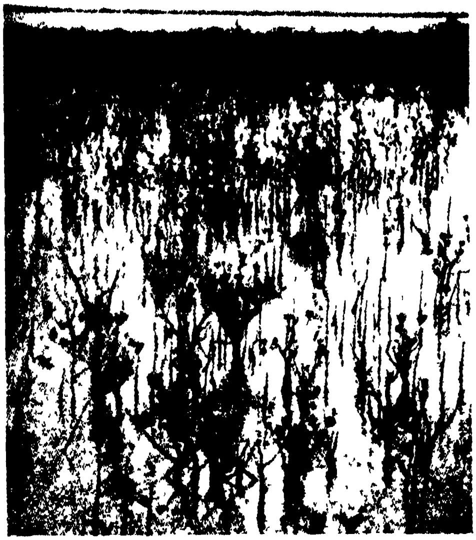
We carefully noted the direction in which the birds had disappeared, assuming, since it was toward evening, that they were rookery bound. We thought, in our naïveté, that by searching in the direction of the birds' disappearance we would quickly find the nesting grounds. So, that night in our hastily built camp, even though miserable (for a torrential rain had started, followed by swarms of mosquitos), we had happy thoughts of next day locating the colony of tens of thousands of flamingos which Chapman had found here in 1905. Forsythe reported that in 1940 the colony was still extensive, with a population of possibly 30,000. But when we had talked to Forsythe a week earlier in Nassau, he had had a disturbed note in his voice. For some years before the war, a warden had been posted near the colony. But we were the first visitors to the area in the five years since the discontinuance of the warden. Forsythe was grateful for our proposed visit, for rumors had come to him by way of the native grapevine that the flocks had gone. He knew, too, that during the war years one of the R. A. F. ferry routes had passed over Andros. He had heard stories of how the weary pilots would sometimes for diversion sweep low when they spotted flamingos and gleefully give chase to the terrified birds. Could these shy creatures take such punishment? Forsythe clearly registered his qualms. We had qualms too, initially; but that night in camp after having seen the 8 flying sheets of flame our confidence was high.

Next morning, by the time the sun was shoving the pink clouds up off the eastern horizon, we were making our way through the mangrove in the direction in which the birds had vanished. The mangrove "swash," as it is onomatopoeically called, is merely shallow salt water in which the mangrove plant is the principal flora. The swash bottom is of coral mud, or marl, much stickier than fresh-water muds and most tiring to negotiate. Beyond the mangrove swash area we came to a large salt-water bight a mile or so across and about 4 feet deep. We alternately waded or rode in a portable rubber boat in the appropriate direction as far as our unfamiliarity with landmarks of the region would wisely permit. We had sent our boatman back to Mars Bay and were now alone and quite at the mercy of this land of coral, mangrove, salt water, mosquitos, crabs, and sharks. Several times during the day we saw flocks of flamingos in the distance, some having as many as several hundred birds. The confusing thing about these flocks was that they appeared from almost any direction and vanished in the

same illogical way. At the end of the third day we concluded that by following the birds we would simply be drawn in all directions and would probably succeed only in getting ourselves lost, without finding the nests of our evanescent quarry.

Then one morning, just as we were preparing to leave camp on another searching jaunt, we were startled by the sound of a single gunshot faraway across the bight to the southeast. As far as we knew, this part of Andros was completely uninhabited. Somewhat alarmed, we set out in the direction of the shot, wading across the bight slowly (constantly, as always, on the lookout for sharks and sting rays) from sand bar to sand bar. At length on a distant bar we spied about 200 flamingo nest mounds. On 3 of them we found eggs, and on a fourth was a broken egg with the red yolk smearing its sides. Our excitement over this discovery soon sobered when we discovered fresh prints of a naked human foot in the sand and dozens of red and black feathers. Clearly someone that very morning had shot one or more flamingos and carried them away. This must be the work of a native poacher.

All natives on Andros know that severe penalties await anyone caught stealing flamingo eggs or killing the birds. But flamingos, both young and adult, as well as the eggs, are delicacies to cheer the heart of any gourmand. Reputed to be even more savory than chicken, the flamingos on Andros for this reason have several times faced extinction because of wholesale slaughter by the natives. The story is told that some years ago the only native who then knew the location of the flock came to the rookery during the laying season and filled his boat with thousands of fresh eggs, which he sold at a handsone price to coastal inhabitants. That year there were few new flamingos. It is also said that late in the summer when the nestlings were large but just in their pre-



ANDROS MANGROVE SWASH

A STICKY BOTTOM LIES UNDER THE SALT WATER

flight stage, two men, each holding the ends of a long thong, would rush groups of wading young and break the birds' legs with sudden jerks of the cord, thus making capture easy.

So a law was passed in Nassau, and, with assistance from the Audubon Society, a warden was appointed. That stopped mass slaughter but apparently not occasional poachers such as the one whose gunshot we had heard. We found no further evidence of him, however, perhaps he spotted our camp and made a hasty departure from the area. We were grateful to him, for his shot led us to our first nests. But, alas, except for the 4 eggs there was no sign of birds; the adults never returned. A few days of exploration disclosed thousands of other old, disintegrating nest mounds on adjacent islands and sand bars. Mangrove roots were growing over and through the nests. Clearly, the Chapman flamingo city had gone the way of Mayan cultures.

But what about the birds that each day crisscrossed the sky? They must have a home. We extended our searching farther and farther from the base camp. There were plenty of herons, gulls, and osprey, but only a few flamingos wading in the distance. Each night when we returned to camp we hoped that the next day would bring discovery. But days and weeks passed, and no rookery.

There was no fresh water on our camp island or, for that matter, on any of the other cays. We depended solely on the brown wriggler-filled rain water found in the dirty coral holes or on the rain water collected by our tent tarpaulin. Big orange crabs overran the island and supplied our principal camp diversion. Everywhere darting in and out of coral crevasses, these creatures were our constant companions. At night as we sat in our tent, dozens of them idled about like dogs begging for a morsel. Occasionally we fed them, but they also fed us; for we soon enough found out how tasty they became when dropped into boiling water. And the supply was limitless. No matter how many we ate, the total number of grotesque tent guests never seemed to diminish.

At length this became tiresome, and, after we had achieved our other fiological aims, we broke camp and made our way out to the coast, rather crestfallen, for, although we had seen many flamingos on the wing, we had failed to find evidence that they were breeding.

Then one morning at the mouth of Grassy Creek we bumped into Robbie Ferguson, a native Negro, who announced that he and he alone knew where the flamingos were nesting thousands of them. We listened skeptically but, after hearing his prayer that God might strike him dead if he were lying and because of his insistence that he would accept no payment if he didn't take us directly to at least a thousand active flamingo nests, we gave ourselves into his care and boarded his sailboat, which smelled of shark liver and putrefying turtle meat.

Robbie, we found, knew what he was talking about, because during the war years he had been watching the flamingos deserting the Chapman rookery and dwin-

A FLAMINGO GHOST TOWN

THIS IS A CORNER OF THE VAST ROOKERY THAT WAS DESERTED BY THE FLAMINGOS DURING THE LAST WAR

AMIDST EGGS AND NESTS IN THE FLAMINGO ROOKERY

BEFORE THE BAHAMIAN FLAMINGO LAWS WERE PASSED, NATIVES WHO KNEW THE LOCATION OF THE ROOKERY WOULD USE IT AS THEIR SECRET SOURCE OF FRESH EGGS. THE ABOVE HATFUL WAS FLOATING IN WATER.

dling in numbers. The remnant, he said had moved to the farthest southern tip of Andros, the most deserted and desolate spot the birds could find for setting up a final bastion against an encroaching world. And it was here one afternoon, after sailing down into what seemed almost the edge of the world, that we came upon our real flamingo rookery.

It was midafternoon, and the sailboat had become becalmed, so we set out in the dinghy and poled our way a mile or two into shore; then another mile through mangrove swash, and again into another salt lake whose surface was as smooth as glass. Suddenly from far across the lake came the faint conglomerate sound of a thousand distant squawks. We pressed on, now wading hip-deep; finally we began to make out on the remote shore a streak of pink color, which as we came closer could be discerned as a vast assemblage of flamingos. Through the binoculars they could be seen more clearly. Some were sparring with one another, with black-tipped wings flapping in graceful cadence. Some were straining their necks and looking in all directions. Small contingents of 20 or 30 birds with great fanfare were intermittently arriving at the rookery, presumably from distant feeding grounds. Other groups were taking off and circling the area.

The birds first spotted our approach across the bight when we were perhaps a thousand yards away. We knew this, for all at once the chatter of the flock turned into an apprehensive silence, and heads periscoped in our direction. The standing birds began to pace nervously, and many of the females arose from their nests and joined the males in their disturbed alarm. The clamor and chatter began again and became louder and louder as we got closer. The confusion became bedlam. I think we must have been no more than 100 yards away when, with the roar of a battery of Gatling guns, the thousand huge flamingos boiled into the sky.

From a tepid pink, the color suddenly changed to a bright red, for all wings, revealing carmine flanks and black covert feathers, were now beating furiously in the

tumultuous take-off. The sight was one of the most breathtaking I have ever witnessed in the world of nature.

Not quite all the birds took off. A few hardy females stayed faithfully on their nests or stalked anxiously close by. The main flock wheeled overhead, still creating an unearthly clatter; and, as we had frozen in our tracks, in a few minutes groups of 50 or 100 began returning. The sight of flamingos settling is almost as spectacular as that of the take-off, especially when an individual bird is watched. As he glides down the runway, his spread wings tilt gently back and forth as though testing, and the sight is somewhat reminiscent of a Piper Cub coming in. The landing gear is slowly lowered, legs extending down and slightly forward. As the toes touch the water, the bird goes into a rapid run, decreasing in speed until the momentum of flight is overcome. The take-off is practically this in reverse. The bird starts running at ever-increasing speed, leaning forward and virtually running on the surface of the water until wing motion finally takes over. It is all as delicate and rhythmic as a ballet. Robbie Ferguson says that even more interesting are the fledglings toward the end of the summer when they are first learning to fly. Apparently they do not have much trouble learning the take-off, but the landing technique comes slowly. During a critical period of several weeks he says that the young always crack up when landing, for they do not know how to use their legs for deceleration when striking the water. As a consequence, their otherwise graceful landing is attended by a series of awkward flip-flops.

Apparently males and females alternate in sitting on the eggs. The adults while brooding fold their long legs under the body very much as other sitting birds do The young are born after a 28-day incubation period; they are white, fluffy, chicklike creatures with straight beaks. For many months they are dependent on regurgitated parental food because their slow wing development prevents flight to distant feeding grounds. Furthermore, their beaks and necks do not become specialized for *Ceritheum* foraging until relatively late in life. It is the peculiar beak and neck adaptation on which the adult birds depend for survival. They arch their submerged heads so that the head is actually inverted

A FLOCK OF UNDISTURBED FLAMINGOS

AS THE OBSERVERS APPROACHED ACROSS THE BIGHT AND CAME WITHIN A THOUSAND YARDS OF THIS FLOCK, THEIR CHATTER SUDDENLY TURNED INTO AN APPREHENSIVE SILENCE AND THEIR HEADS PERISCOPED.

FLAMINGOS ABOUT TO FLY
IN THE TAKE-OFF THE BIRDS RUN AT EVER-INCREASING SPEED UNTIL WING ACTION FINALLY TAKES OVER.

and the beak pointed backwards. Then with a swinging motion they sift the coral sand for *Ceritheum*. While doing this they slowly move forward in the water. The number of *Ceritheum* needed to maintain a large flock of flamingos must be astronomical. Since *Ceritheum* constitutes the sole diet of the wild birds, it is clear why the young have to remain dependent on parental feeding until their straight beaks have developed into the clumsy feeding organs which give the adults their grotesque appearance.

The colony was now thoroughly disturbed, birds coming and leaving at about the same rate. We could see from the distance that each nest mound had one chalky-white egg on it. We were perplexed, however, by the sudden realization that floating on the surface of the water through which we were wading were literally hundreds of eggs. We began to notice, too, that entirely submerged on the floor of the lake--about 2 feet deep at this point--were hundreds of completely inundated nest mounds. It was obvious that at the beginning of the season the birds had started building their mounds along the shore, not knowing that the late spring tides would bring the water level up about a foot. All the earlier nests had had to be abandoned, and of course the eggs were carried off by the rising water

Another tragic observation, which, by the way, does not mark the flamingo with too high an intelligence, is that every time the flock takes off, dozens of eggs are knocked from the mounds and are broken Yolk and embryo remains are found everywhere. For this reason we revisited the rookery as few times as possible, for each time that we caused a general alarm we knew we were contributing to the extinction of many potential birds. Also, whenever the flock was away from the rookery (which was only when the birds were disturbed), vultures

WILD FLAMINGOS IN FLIGHT

UNLIKE THOSE SEEN IN ZOOS, THEIR COLOR IS VERMILION WITH BLACK WING COVERTS. THEY FLY WITH HEAD AND NECK STRAIGHT OUT IN FRONT, WITH THEIR LONG THIN LEGS STRETCHED OUT TO THE REAR.

appeared from nowhere, ready to swoop down on the eggs or young. Lizards, too, quickly put in their appearance as egg robbers.

As we moved away from the rookery that day, the birds, some of which had been circling and others of which had landed on an adjacent lake, began coming back. By the time we were 1,000 feet away, splashing through the bight hurriedly (for the sun had gone down and our boat was still miles away), the majority of the birds had returned to the rookery. I looked back from a distance of perhaps half a mile and saw the same streak of orange-pink on the far shore as had been there when we approached.

FLAMINGOS belong to the family Phoenicopteridae, which consists of only 3 genera and 6 species. *Phoenicopterus ruber*, the roseate flamingo, is found on a number of the West Indies islands, although the islands of Inagua, Abaco, and Andros have been considered to be the principal breeding places of the Carribbean birds. If the situation observed on Andros may in any way be considered typical, then serious thought should be given to the possibility of the birds' eventually becoming extinct. The depressing history of the roseate spoonbill in Florida is evidence that conservation measures should be taken long in advance of the actual extinction threat.

There is little doubt that the deserted rookery which we found on the old Chapman site has an unhappy significance, especially since the new rookery on the southern Andros cay is numerically nowhere near the size of the former Chapman colony. From the conservationist point of view, it is of interest to inquire into the

reason for this migration and shrinkage. It does not seem likely that food supply is a factor. Andros flamingos feed entirely on the *Ceritheum* found in the bights and salt-water shallows so abundant all over the southern part of the island. These vast feeding areas are just as readily available from the Chapman location as from the new rookery. Too, in the immediate vicinity of the old rookery we found a plethora of *Ceritheum.* One doubts, furthermore, whether the specialized *Ceritheum* diet is as important to flamingo euthenics and survival as was formerly supposed. The flock of defledged flamingos maintained at Hialeah Park, Miami, has for a number of years been fed on synthetic diets entirely lacking in *Ceritheum.* These captive birds, although not as gorgeous in coloration as the wild flamingos, nest, mate, and brood very successfully.

As to the possibility of epidemiological factors in the disappearance of the Chapman colony, little can be said other than that during our visit, at least, there was no sign of sick or dead birds.

One doubts that the activities of native poachers have contributed in any major degree to the disappearance of the birds. Poaching has been going on ever since the settling of Andros Island. Large-scale or organized poaching was effectively stopped by the Bahamian flamingo laws. Such predators as the one whose gunshot we heard are probably of little consequence. Evidence for this is the fact that the birds survived in full number until as late as 1940 despite isolated cases of individual native molestation.

SUBMERGED FLAMINGO NEST

AN EGG FLOATS NEAR THE INUNDATED NEST.

FLAMINGO NESTLING

The influences which brought about the desertion of the Chapman colony are probably of a more general nature. And one is rather inclined to the conclusion that the flamingo migration and diminution are more properly ascribable to the hysterical flight caused among the birds by airplanes.

The dissolution of the Chapman rookery appears to be synchronous with the outbreak of war in 1940. The immense air bases and training centers on New Providence Island, only about 100 miles from the Chapman rookery, certainly introduced a widespread disturbance in the Bahamian skys. As stated before, it is quite common knowledge that many a young, mischievous pilot reduced the weariness of his flying

hours by giving chase to the conspicuous red birds in the vicinity of Andros. The consequence of such nerve strain on the already nervous personality of the flamingo is not difficult to imagine. Also, during the war the American military forces made an aerial map survey of Andros, including the Chapman rookery area. This day-in, day-out air activity could hardly have added to the flamingos' sense of security.

In the past year or two the whole of the island of Andros has been subject to elaborate oil-prospecting activities. Although the surveyors and engineers with whom we made contact during our trip were most careful not to cause alarm to any flamingos they happened to encounter and though they had not as yet reached the southern extremity of the island where the flamingos are reconstituting themselves, it is undeniable that their motorboats plying up and down through bights and channels rarely before visited and the concomitant air activity constitute a nerve-racking intrusion so far as the flamingo is concerned.

One can also venture the guess that the birds of the Chapman colony, early in the war years, found the airplane disturbance intolerable and at length fled to the most remote extremity of the island to set up their new rookery. If each time an airplane comes over, the usual egg damage occurs from the sudden flight of the birds, the reproduction rate of the flamingo is certain to decrease, perhaps dangerously near to the extinction point. In the new rookery we found only a few thousand birds. There were 30,000 in the Chapman rookery in 1940. Where are the missing thousands? Possibly there are other new rookeries elsewhere on Andros, to which the birds fled, although we know of none. Possibly many of the birds migrated to such less disturbed islands as Inagua or Abaco. In any case, during the war years, what appeared to be a stabilized rookery became deserted, and the noble efforts of the Audubon warden and such conservationists as former Commissioner Forsythe to preserve this colony have been virtually undone. The Chapman flamingo city was as surely a war casualty as was Amsterdam or Leningrad. Whether the birds in their new rookery will recover and develop into the vast colonies of former days cannot be predicted. If oil is discovered on Andros and if large-scale drilling and pumping occurs, one cannot but envisage a dreary prognosis for the flamingo.

One can only hope that in the normal advance of man and his technology, the flamingo will not be subjected to continued or excessive disturbance. Setting aside a preserve for the bird is probably impracticable because of the vast areas of salt shallows required to feed large flocks. This is especially difficult if such areas are of commercial value. Andros Island until recently was a desolate and forgotten land. That was why the flamingo prospered. If commercial exploitation sets in for oil or any other resource, the fate of the bird, at least on Andros, is sealed. The beautiful creatures already have too many cards stacked against them to make frequent human intrusion anything but disastrous. Biologically the flamingo is so highly specialized an organism that adaptation to disturbance or to new or changing conditions is difficult or impossible.

MICROBIOLOGY IN THE U.S.S.R. IN 1946*

By SELMAN A. WAKSMAN
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IT HAS been said that the "germs know no national boundaries." One might paraphrase this truth thus: The search for germs and their role in human health and in human disease knows no national affiliations. This was brought home to me very forcefully during the summer of 1946 when I had an opportunity to look into the progress made in the Soviet Union during the war years in the field of microbiology.

A knowledge of the language and an acquaintance, either personal or by correspondence, with many of the Russian microbiologists made it possible for me to cover much ground in a short time. My survey of the development of microbiology in the Soviet Union was more intensive than extensive since my visit was limited mainly to Moscow, with a shorter visit to the second largest city, Leningrad. In these two cities the scientific organizations of the country are largely centered. Here I had ample opportunities to meet a number of scientists from other centers and thus gain an insight into the development of microbiology in the country as a whole.

BACKGROUND

Following the epoch-making investigations of Pasteur, microbiology progressed in Russia along two lines very similar to those in the U. S. A. and in other countries: general and agricultural microbiology and medical bacteriology. The outstanding representatives of these two fields of microbiology in Russia were S. N. Winogradsky and I. I. Metchnikov, respectively.

The background for the development of general and agricultural microbiology in Russia is to be found in the work of the great Russian botanists, plant pathologists, and plant physiologists. It is sufficient to mention four names: L. C. Cienkovski, who was a botanist by training and later became famous for his work in protistology, chiefly in bacteriology and protozoology; two of Cienkovski's most brilliant students, M. Woronin and A. S. Famintzin, both of whom became famous for their work on root-nodule bacteria and on slime-producing bacteria, respectively; and D. I. Ivanovski, who first observed and described the infectious nature of the filtered juice of leaves with tobacco mosaic disease, namely, plant viruses.

Winogradsky, the greatest Russian microbiologist and soil bacteriologist, was a student of Famintzin. It was Winogradsky who in 1891 organized, at the Institute of Experimental Medicine, the first microbiological laboratory in Russia. Upon his retirement in 1905, his place was taken by his assistant, V. L. Omelianski. Upon the death of Omelianski in 1928, he was succeeded by V. L. Issatchenko, who later became the chief of the Microbiological Division of the Academy of Sciences, which was established in 1930.

The development of medical microbiology in Russia is closely associated with the name of Metchnikov, zoologist, pathologist, and bacteriologist; N. F. Gamaleia, who became famous for his work on plague, cholera, tuberculosis, and virus infections; D. K. Zabolotni and G. D. Belonovski, both of whom have made important contributions to our knowledge of the epidemiology of typhus, of cholera, and of other infectious diseases.

* Paper of the Journal Series, New Jersey Agricultural Experiment Station, Department of Microbiology, Rutgers University.

The Academy of Sciences and the All Union Institute of Experimental Medicine (VIEM) are the two outstanding organizations that devote considerable attention to the subject of microbiology, from a broad general and agricultural point of view, on the one hand, and from an applied medical point of view on the other. In addition to these two important organizations, the corresponding academies in the various constituent republics, notably in the Ukraine (Kiev, Kharkov), devote a great deal of attention to this subject.

Mention must also be made of the various ministries, such as those of public health, manufacture of medicinal preparations, food industries, and fermentation industries; the universities, agricultural colleges, and other teaching and research institutes; and, finally, the agricultural experiment stations.

Some of these organizations have special institutes devoted to the manufacture of microbiological products. For example, the Institute of Epidemiology and Bacteriology in Moscow is concerned with the manufacture of toxins, antitoxins, serums, vaccines, antibiotics, and other bacteriological products, and the Institute of Microbiology in the name of Zabolotni in Kiev is concerned with the manufacture of various medicinal and agricultural bacteriological products.

Certain problems in the field of microbiology, both fundamental and applied, overlap into several of the above organizations. The subject of antibiotics, for example, is one such problem. At least five of the ministries and the Academy of Sciences are vitally interested in the search for new antibiotics, in the manufacture of those that have already established their practical value, such as penicillin, and in the application of these antibiotics to disease control.

A detailed analysis of some of the work in the field of microbiology in the U.S.S.R. at the present time can perhaps best be presented in this brief survey by describing some of the activities of the Academy of Sciences and by drawing attention to the development of the subject of antibiotics.

ORGANIZATION OF THE INSTITUTE OF MICROBIOLOGY OF THE ACADEMY OF SCIENCES OF THE U S S R

Director of Institute: V. L. Issatchenko

Assistant Director: M. F Kriss.

Scientific Secretary: A. E. Kriss.

1. *Division of General Microbiology:* C. I Kuznetzov.
2. *Division of Ecology of Microorganisms:* A. A. Imshenetzki (*Vice-Director*).
3. *Division of Bioceonology of Microorganisms:* N I. Krassilnikov.
4. *Division of Soil Microbiology:* E. N Mishustin
5. *Division of Systematics and Philogeny of Microorganisms:* V. N. Shaposhnikov.
6. *Division of Viruses:* V. L. Rishkov

Total number of senior scientists	12
Total number of junior scientists	13
Number of laboratory assistants	23
Office personnel	5
Total staff .	62

ACTIVITIES OF THE INSTITUTE OF MICROBIOLOGY

As the plan of organization indicates, the activities of the institute cover the various aspects of general and agricultural microbiology. This includes the ecology of microorganisms, their physiology, chemical activities, and their role in soil processes and in water basins and as causative agents of plant diseases.

Of the problems of a general microbiological nature, only a few need to be listed here. M. N. Meissel and assistants are studying the function of growth-promoting substances in the growth of microorganisms, especially the action of thiamine upon yeast. The function of thiamine as a biological regulator of cell structure is differentiated from its role in the activities of the cell. The synthesis and accumulation of thiamine were found to be influenced by the particular conditions under which yeast fermentation takes place. Methods

for the quantitative determination of thiamine and thiazole are based upon the use of the fungus *Endomyces magnusii.* A study has also been made of the phenomena of hypervitaminosis and avitaminosis among microorganisms, resulting from an unbalanced relation of thiamine and biotin. This led to an investigation of the volatilization of vitamins, a phenomenon characteristic of the thiazole fraction of thiamine. Microorganisms were found to be capable of absorbing from the air this thiamine fraction and of synthesizing the complete vitamin molecule. By utilizing microorganisms as test agents, it was possible to demonstrate that certain growing plants, such as peas, give rise to volatile thiamine or thiazole. This was also found to be the case for nicotinic acid and for p-amino benzoic acid

The effect of ergosterol on yeast fermentation is being studied by T. A. Tausson. The formation of carotenoids and their role in the physiology of red yeasts are being studied by Miedviedieva. A study of the effect of carcenogenic substances on the microbial cell brought out the fact that

V. L OMELIANSKI
PICTURE TAKEN FEBRUARY 25, 1925.

S. N. WINOGRADSKY
SHOWING THIS FAMOUS RUSSIAN MICROBIOLOGIST ON HIS NINETIETH BIRTHDAY, SEPT. 14, 1946, AT BRIE-COMTE-ROBERT, FRANCE

considerable accumulation of such substances may take place, these may be retained in the cells for a long time. The localization of carcinogenic agents in the microbial cell results in a sharp irritation of the cell followed by formation of mutants.

Considerable attention is given to the problems of structure, life cycle, and systematics of microorganisms. Kudriavtzev is working primarily with yeasts, Krassilnikov with actinomycetes, and Imshenetzki with bacteria. In a series of papers published in *Microbiologia* for 1942–46, Kudriavtzev demonstrated that the classification of yeasts, based upon the recognition of types that have become fixed as a result of continued cultivation under artificial conditions, may result in a totally different perspective from a comparable study of yeasts in their natural environment. Such phenomena as the size and shape of the cell, the formation of giant colonies, and the relation of the organism to gelatin and to the nitrogen source should not be considered as factors which determine the development of organisms in their

TWO MICROBIOLOGISTS
V. L. ISSATCHENKO (*left*), DIRECTOR OF THE MICROBIOLOGICAL INSTITUTE OF THE ACADEMY OF SCIENCES; N. I. KRASSILNIKOV (*right*) CHIEF OF ONE OF THE DIVISIONS OF THE INSTITUTE.

struggle for existence and, therefore, not for species characterization. Because of this, the number of yeast species now recognized must be markedly reduced. For example, *Saccharomyces cerevisiae* and *S. ellipsoideum* were recently combined into the single species *S. cerevisiae*. The fact, however, that *S. cerevisiae* occurs largely under the artificial conditions of beermaking and wine making, where it develops a high fermentation rate, whereas *S. ellipsoideum* persists only in such substrates as fermenting fruit juices, suggests the existence of two independent types.

Imshenetzki has made a detailed study of the structure of bacteria and of bacterial dissociation. He does not recognize the existence of any correlation between colony shape and other characteristic properties of the bacterial cells. The ash content of the R and S variants of *Sarcina flava* was found to be identical; both variants were equally resistant to high temperatures and both were alike in the intensity of their respiration. Rough variants were frequently found to be more active than smooth ones, which is contrary to the dissociation theory.

Krassilnikov and A. E. Kriss have concentrated their attention upon the study of the Actinomycetales, their morphology, their systematic position, their physiology, and especially their antagonistic properties. Several monographs have been published, most important of which are the two by Krassilnikov on the Actinomycetales and related organisms and a key to the Actinomycetales.

Krassilnikov and his group have also been concerned with the relation of microorganisms to the root systems of plants. The state of the roots of plants and their secretions greatly modify the nature and abundance of the bacterial population of the rhizosphere. Under certain conditions of plant cultivation, this results in an accumulation of phytopathogenic bacteria and fungi. The role of microbial antagonists in combating these plant-pathogenic organisms has been investigated. The function of "mycolytic bacteria," first elucidated by Chudiakov in 1935, was given special consideration. These bacteria, largely of the *Pseudomonas* and *Achromobacter* types, have a marked lytic action upon *Fusarium* and other phytopathogenic fungi. It is sufficient to mention the bacteria which dissolve the fungus *Verticillium dahliae*, the causative agent of cotton wilt, and the bacteria which dissolve *Fusarium*, which causes fusariosis of pines, flax, and cotton. Favorable results are said to have been obtained from the use of such bacteria for the purpose of combating plant diseases, not only under controlled laboratory conditions but also in the field. The seeds of the plants are treated with

cultures of the bacteria (bacterization), resulting in a reduction in plant destruction caused by fungi and in an increase in crop yields. The treatment of soil with cultures of the fungus *Trichoderma lignorum* grown on sugar-beet pulp is said to give a marked increase in crop yield, owing to improvement in soil structure and to the formation of phytohormones which favor plant development.

The problem of nitrogen fixation, both by root-nodule bacteria and by free-living bacteria of the Azotobacter type, continues to receive marked attention in the Soviet Union. In addition to the universally accepted principles of legume inoculation (Nitragin preparation) and of the importance of legumes in crop rotations, problems not so universally recognized continue to receive attention; for example, the claim that leguminous plants are able to fix nitrogen under sterile conditions not only in the presence of root-nodule bacteria but also in the presence of certain other bacteria. One must classify into the same category the claim that inoculation of soil with cultures results in a greater nitrogen potential in the soil. These assumptions led to the development of "bacterial fertilizers," or "soil improvers." One of these preparations, designated as "Azotogen" and consisting largely of an *Azotobacter* preparation, is being utilized on a large scale throughout the Soviet Union. It may be emphasized here, in passing, that whereas there is no question of the great value of the inoculation of leguminous plants with specific root-nodule bacteria, the use of *Azotobacter*, as well as of the so-called ammonifying and phosphate-mobilizing bacteria, and even of plant-disease-combating organisms for soil inoculation under field conditions, has not found favorable application in this country, and the value still remains to be established.

Geographical distribution of specific bacteria, such as *Bacillus mycoides*, and their

D. I IVANOVSKI
AN EARLY INVESTIGATOR OF PLANT VIRUSES

role in soil-forming processes is the subject of special investigations by Mishustin and his group. Southern strains of *B. mycoides* were found to have a higher temperature optimum and a greater potential for multiplication than comparable strains found in areas under colder climatic conditions. The distribution of spore-forming bacteria in the soil was found to be closely correlated with the transformation of organic matter. Other bacteria as well were found to serve as good indicators; thus, the presence of the *E. coli-A. aerogenes* group determines the degree of soil pollution. A higher moisture content favors bacterial development, whereas lower moisture favors fungi and actinomycetes.

The study of viruses, begun so auspiciously in 1892 by Ivanovski, has been continued in the Soviet Union by a host of investigators. It is sufficient to mention the work of Rishkov and his associates in the field of plant viruses and of A. A. Smorodintsev and L. A. Silber on animal viruses. Rishkov, now in charge of the

C. A. ORBELI
SECRETARY OF THE ACADEMY OF SCIENCES FOR THE BIOLOGICAL SCIENCES. HE IS ALSO THE DIRECTOR OF THE PAVILOV INSTITUTE.

virus division of the Microbiological Institute of the Academy, is the author of a book on virus diseases of plants and of a series of papers on the isolation and nature of plant viruses. Smorodintsev's work on influenza virus and antiserum has attracted universal attention and has found wide application in this country as well. Silber is connected with the Institute of Epidemiology and Bacteriology; he has recently written a treatise on virus diseases of animals.

Numerous other investigations in the field of general and agricultural microbiology are being carried out in the Soviet Union, either in the various academies or in some of the other research and teaching institutions. These deal with problems pertaining to agriculture and industry. Examples include the work of Novogrudsky on the microbiological processes in semidesert soils, of Panosian on ammonia formation in Armenian soils, and of Krassilnikov and associates on the microbiological problems of "soil fatigue." The industrial problems comprise the work of Shaposhnikov on citric and lactic acid fermentations, of Brockaia on thermophilic proteolytic bacteria, of Imsenetzki on thermophilic cellulose-decomposing bacteria, and of Tausson and Seliber on fat synthesis and fat decomposition by bacteria and actinomycetes; the work on the bacteriology of hydrocarbons, notably their decomposition and the role of this process in the origin of petroleum; the occurrence of bacteriophage in sewage purification and its function as an indication of fecal contamination; and much other work.

ANTIBIOTICS

I found considerable interest in the subject of antibiotics in the Soviet Union. At least five of the ministries, the Academy of Medicine, and the Academy of Sciences are devoting time to this subject. Although I spent most of my time in Moscow and only a few days in Leningrad, I had ample opportunity to visit the most important laboratories devoted to this subject, as well as to speak to various scientific workers from other parts of the country.

The ministries in which the interest in antibiotics is centered include: Ministry of Public Health, Ministry of Manufacture of Medicinal Preparations, Ministry of Food and Nutrition, Ministry of Fermentations, and Ministry of Heavy Industries, notably those branches which deal with biological supplies. Several institutes in the Academy of Sciences are interested in antibiotics, notably the Institute of Biochemistry and the Institute of Microbiology. Other organizations, such as the Institute of Epidemiology and Bacteriology, the Institute of Tuberculosis, the Institute of Physiology, and the Institute of Malarial Diseases, all of which are centered in or

around the VIEM (All Union Institute of Experimental Medicine), a special organization largely under the control of the Ministry of Public Health, are also interested in this subject.

The investigations on antibiotics carried out in these institutes and laboratories are partly of a research and partly of an applied nature. They embrace practically every branch of antibiotics studied in the United States and in Great Britain during the war period. The results of the survey can best be briefly summarized on the basis of each group of antibiotics.

1. *Fungi producing penicillin and penicillin-like substances.* Very little progress has so far been made in the Soviet Union in the manufacture of penicillin, although one or two pilot plants appear to be in the process of construction and several laboratories have concerned themselves with the isolation of penicillin-producing organisms, as well as with the production, isolation, and purification of penicillin and penicillin-like preparations. The small amounts of penicillin produced are primarily by the bottle process. Among some of the penicillin-like materials may be included penicillin-crustosin; this substance is a typical penicillin produced by *Penicillium crustosum*. It may also be of interest to mention here aspergillin produced by *Aspergillus niger*, which also appears to offer limited possibilities as an antibiotic agent.

2. *Spore-forming bacteria.* A tyrothricin-like preparation, which was designated as gramicidin S, was isolated in the Soviet Union. This preparation was found to be tyrocidine, one of the polypeptide constituents of the tyrothricin complex. It is being produced on a pilot-plant scale. Its efficacy in controlling wound infections does not appear to be greater than the original tyrothricin isolated in this country by R. Dubos. Another type of antibiotic was isolated in crude form from certain spore-forming bacteria. This was found to be water-soluble and was said to be similar in its properties to streptomycin. It was designated as colistatin. It is thermostable, is adsorbed on charcoal, and is removed by acid alcohol. It is active against various gram-negative and gram-positive organisms. Its limited activity in vivo renders this antibiotic of questionable practical importance.

S. VAVILOV
PRESIDENT, ACADEMY OF SCIENCES, U.S.S.R.

3. *Antibiotics of actinomycetes.* Several antibiotics have been isolated from actinomycetes and described under the names of mycetin, litmocidin, and one or two others. None of these appears to represent any great practical potentialities, since they are either not active in vivo or they have only a limited type of activity. They are primarily pigments in nature. Some work is also being done with streptothricin and streptomycin, cultures obtained from the United States being used.

4. *Antibiotic from blood.* An antibiotic

was isolated from red blood cells by a method similar to that commonly employed in isolating tyrothricin. This antibiotic was designated as erythrin. It is said to possess antibacterial properties similar to those of tyrothricin, although it is much less toxic. Its use is limited to surface applications for wounds and other infections.

5. *RK or KR factor.* This substance may possibly be classified with antibiotics, although its action is directed upon cancer cells rather than upon microbes. Although considerable publicity has been given recently to this substance, the general impression gained from a careful examination of the facts presented was that the claims have not as yet been verified and that the possibility of applying this factor to the treatment of human cancer is still very much open to question.

WHEN the second World War broke out, the major scientific institutions in the Soviet Union were moved away from their permanent locations to temporary positions in the East, especially in the Urals and in Central Asia. Although most of these institutions have been repatriated and are already beginning to function in a normal manner, certain difficulties, mostly of a purely physical nature or due to a shortage of trained personnel, were bound to occur. The progress of science is largely dependent upon the systematic collection of basic facts which govern natural processes. Once the normal course of scientific investigation is interrupted, there is bound to follow a period of adjustment which involves a certain loss of time in order to bridge the lost interval. A country that has suffered so much from the ravages of the war as has the Soviet Union is bound to show certain disorganized links in the progress of its scientific program. The most impressive thing observed by a visitor is the rapidity with which scientific recuperation has come about and how quickly the Russian scientists have made up for lost time. One must not be surprised, therefore, to learn that Russian factories are not as yet turning out large quantities of penicillin, or are not producing riboflavin by microbiological processes, or have not succeeded in establishing certain other industries which were developed in the United States and in Great Britain during the war. The surprise was to find that not only were the Russian microbiologists turning out large quantities of materials essential for combating important human and animal diseases, such as various toxins (including that of tetanus), vaccines (including that of typhus), serums, and other bacteriological preparations, but that they have also made an impressive start in the development of new fields, such as antibiotics.

APTITUDE TESTING

By JAMES F. BENDER
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MANY a veteran has come home from the wars to find himself—a captain or a major—definitely unfitted for his old clerical job. He now wants something commensurate in responsibility and compensation with what he had in, say, the Air Corps. How can he find out what he is really fitted for? In the Army he learned a good deal about aptitude tests. Perhaps their applications in education and industry will help him in his search for the right job.

The General Classification Test, given to several million Army and Navy personnel at induction, contained subtests of simple arithmetic, vocabulary, and analysis of cubes. The psychologist announced to the inductees that the test would measure "their ability to learn." Its origin goes back to 1908 when two French psychologists, Binet and Simon, published the results of a fifteen-year investigation of the question: How can we devise a more accurate method of identifying subnormal children in the Paris schools than the procedure based upon general observation?

Using empirical methods, they tried large numbers of tests upon many children of various ages to discover which test would differentiate children of various levels of mental ability. In that way they obtained *norms*, or averages. A youngster of six years of age who could pass the tests that had already been passed by a majority of six-year-olds was called normal in intelligence and given a mental age, or M.A., of six years.

Thus, the M.A. when put over the C.A. (chronological age) gives the I.Q., or intelligence quotient. If the result is 1, oftener written 100, the child is said to have normal intelligence; if his mental age exceeds his chronological age, then of course he is brighter than his peers, I.Q. 140 marking the beginning of the genius distribution. And vice versa, if a youngster of eight can achieve no more than an M.A. of six years, his I.Q. 75 classifies him among the less gifted, almost in the feeble-minded division. In order to succeed scholastically in a first-rate college, the student ordinarily needs a comfortable margin of 10 or more points above "normal" I.Q.

The Binet-Simon tests were soon adapted in America, the best-known revisions being those of Professor Lewis M. Terman, of Stanford University. During World War I the government asked a group of psychologists to devise tests to identify talented recruits for officer training. The result was the Army Alpha Test, based on the work of Binet-Simon and their successors and consisting of eight measurements: ability to (1) follow directions, (2) do simple arithmetic problems; (3) reveal practical judgment, (4) identify synonyms and antonyms; (5) unscramble disarranged sentences, (6) complete series of numbers, (7) see analogies; (8) reveal general information.

Questions are arranged from simple to complex: for example, in the arithmetic subtest, from "How many are 60 guns and 5 guns?" on up to "A certain division contains 5,000 artillery, 15,000 infantry, and 1,000 cavalry. If each branch is expanded proportionately until there are in all 23,000 men, how many will be added to the artillery?" Items in the fifth subtest are unscrambled and marked *true* or *false:* for example, "horses feathers have all;" "trees roses sea and in grow the;" "moon earth the only feet twenty from is."

The Army Beta Test was designed for illiterates and is composed of pictures, simple puzzles, number checking, mazes, and three other tasks, none of which depends upon reading ability.

These two tests were administered to approximately 1,727,000 men and were useful in eliminating the mentally unfit and identifying men who might make good officers because of their superior mental ages. The tests were indeed valid in predicting scholastic success, for among the men with a test rating above C+ only 8.65 percent flunked out of officer-training classes, whereas among men rating below C+, 58.27 percent failed their courses. Later in the war the scores were used to classify personnel according to the 275 occupations within the Army.

The tests also served as models for the numerous mental ability tests since used in education, such as the Scholastic Aptitude Test which is administered yearly by the College Entrance Examination Board all over the world to secondary-school students applying for admission to American colleges and universities.

Moreover, they were soon applied to business and industry. One of the first American companies to adopt intelligence tests chose 50 women to pack small, fragile articles. Careful records of their individual production were kept. At the end of a few weeks the most intelligent were either producing less than when they began or had quit; and, conversely, those with low scores increased their output and were satisfied with their work. High intelligence seemed to be deterrent to successful performance on that job; there was not enough challenge in it to sustain interest among the high scorers. Intelligence tests therefore have a negative as well as a positive value, for they are used to weed out those applicants who are too intelligent to become satisfactorily adjusted to a given job.

Intelligence, which may be roughly defined as the average of all a person's aptitudes, is tested by prescribing many different mental tasks. But certain work requires certain mental proficiencies. Take the job of bank teller, which requires above-average capacity in arithmetic. That is why scores on arithmetic subtests are weighted more heavily than those on vocabulary, for example, in screening applicants for tellers' jobs.

Another pioneer test developed out of the needs of the Army and Navy in World War I was Professor Robert S. Woodworth's Personal Data Sheet, designed to identify maladjustments of temperament. Also known as the Woodworth Psychoneurotic Inventory, it was used subsequently in education and in job placement. It formed the basis for the many later instruments measuring traits of temperament among children as well as adults. The Bernreuter Personality Inventory is widely used in business and industry. Its distinctive feature lies in its provision for measuring six traits of temperament on the basis of the same 125 questions. Each question is answered by *yes*, *no*, or *?* (meaning "Entirely unable to answer either *yes* or *no*").

Representative questions are: Do you daydream frequently? Do you get stage-fright? Have you ever had spells of dizziness? Are you troubled with the idea that people on the street are watching you? Do you ever argue a point with an older person whom you respect? Do you usually avoid asking advice?

After the scores are obtained by six different scales, ratings are assigned on (1) neurotic tendency, (2) dominance-submission, (3) introversion-extroversion, (4) self-sufficiency-dependency, (5) confidence-in-oneself, and (6) sociability. Bernreuter's description of one of the scales provides a sample interpretation:

> The individual who scores high on the B1-N (neurotic) scale shows a tendency toward neurotic

condition. Such an individual often feels miserable, is sensitive to blame, and is troubled by useless thoughts, by shyness, and by a feeling of inferiority. He shuts himself off from other people, he frequently daydreams, and worries both over things that have happened and over things that may happen. . .

The individual who scores low on the B1-N scale is an emotionally stable person. He is rarely troubled by moods, worries, or by criticisms of others He is self-confident, and is a doer rather than a daydreamer . .

Industrial psychologists report that job dissatisfaction is the greatest single cause of labor turnover, and approximately 60 percent of all job dissatisfaction is traceable to emotional maladjustments. The personality inventory is used, therefore, to detect emotional conflicts before the employee is placed. It also proves helpful in selecting salesmen and others whose human contacts require a certain temperamental type; for example, a proclivity toward extroversion is helpful in most kinds of direct selling.

A third use is exemplified in the rehabilitation of stutterers, whose inability to speak at will ordinarily results in feelings of frustration and kindred personality maladjustments. These can be isolated and their intensity estimated with the aid of personality inventories.

CLOSELY related are the interest scales. A person may have the intelligence to succeed in mechanical engineering, but, unless his interests are related to those of successful mechanical engineers, he is unlikely to make good as a mechanical engineer. The Vocational Interest Blank, by Professor Edward K. Strong, Jr., was standardized on successful adults in some 38 vocations for men, ranging from advertising man to Y. M. C. A. general secretary; and on about half that number in vocations for women, including author, housewife, and lawyer.

A more recent scale measures areas of interest. The Preference Record, developed by Dr. G. Frederic Kuder, evaluates mechanical, computational, scientific, persuasive, artistic, literary, musical, social service, and clerical interests rather than interest in specific vocations. Different norms are provided for the sexes The Preference Record is particularly helpful in guiding high-school youngsters who do not know where their interests lie.

Scales of general intelligence, temperament, and interests are of great importance, for these three bases of personality account in large part for personal success or failure. Specialized aptitude tests cover such widely different fields as law, teaching, selling, engineering, and medicine. They are composed of subtests dealing with tasks performed in the respective vocations. Some of them also include general information questions.

For example, in a widely used clerical aptitude test, there are subtests dealing with name lists and columns of figures to be checked, misspelled words to be identified, etc. In the George Washington University Test for Ability to Sell, one of the subtests deals with the ability to memorize names and faces. In the Ferson-Stoddard Law Aptitude Examination, a report of a legal case is studied for four minutes before the examinee is given the booklet in which questions are asked pertaining to the case. Such tests are ordinarily administered as part of a battery which includes the other kinds already discussed. They are tests of capacity rather than proficiency; that is, they are aimed at the innate abilities of the individual. Without her ever having been near a business office, a girl's score on a clerical aptitude test may indicate that she would make a good clerk.

Five particularly pertinent and important conclusions may be drawn from a study of aptitude testing:

1. "At least 50 percent of the population can succeed at least 50 percent of the time in at least

50 percent of the vocations." This knocks into a cocked hat the old belief that everyone is born into the world to do only one job well Man, the most adaptable of all animals, can ordinarily succeed in a generous selection of the almost 20,000 vocations listed by the U. S. Census

2. The "right" vocation is defined not only in terms of one's general intelligence, interests, temperament, and specialized capacities, but also by such important questions as: Does the vocation have a future? Is it overcrowded?
3. The right vocation is defined usually in terms of the long pull rather than the short haul, especially where gifted people are concerned. Where may I expect to be ten years from now if I follow the indicated educational and vocational advice? is a significant question when discussing results of aptitude tests
4. Aptitude tests when administered and interpreted by qualified personnel can be of great help both in guiding the individual vocationally and in putting the employment policy of management on an objective basis. This does not mean that all aptitude tests are equally valid (there are more than a thousand of them!); nor are they a substitute for common sense; neither can they take the place of the seasoned judgment of trained interviewers Rather, they are valuable adjuncts when used competently. Many companies report excellent results from using them in employee selection The president of a representative company states: "Psychological and aptitude tests have succeeded in weeding out 85 percent of untrainable men who applied for work "
5. Aptitude tests can be of considerable aid to the person who has not yet discovered his capacities. Test results indicate that few people use all their capacities; that most of us can do certain things about three times more efficiently than certain other things, that among average specimens of human beings the most gifted possess about four times as much natural aptitude along given lines as the least gifted.

School children, especially, are in urgent need of more vocational guidance based on aptitude testing. Witness the statement attributed to Dr. Henry C. Link: "Our educational system is better equipped to give eight years of wrong kind of education to its pupils than eight hours of competent professional guidance."

It is well to keep in mind that there is no quick, easy way to discover one's aptitudes and qualifications; that vocational guidance based on controlled aptitude testing cannot be given through the mails; that ordinarily adults should spend 8 to 10 hours taking tests under controlled conditions if the job is done right, preferably in sessions lasting no longer than 2 hours.

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THE PRODUCTION AND RIPENING OF RED BLOOD CELLS

A SUMMARY OF DANISH STUDIES

By AUGUST KROGH

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THE writer of this article was asked by the editor to present a summary of scientific work carried on in Denmark during the German occupation from April 9, 1940, to May 5, 1945.

Scientific work was never interrupted and not even seriously disturbed, but lack of chemicals and other materials, and especially lack of contact with the whole outside world, made itself increasingly felt.

An exhaustive survey of Danish work in all branches of learning is in preparation and will be useful for reference purposes, but I have preferred to summarize a single series of papers of outstanding merit and interest. The chief author of these papers is a young man, Claus Munk Plum, who had failed in the preclinical examinations in the Medical School of Copenhagen University and who was thereby barred from an academic career. His interest in research was so strong, however, that he accepted inferior positions in research laboratories, where he showed such aptitude for experimental work and obtained results of such importance that in 1944 the University could confer upon him the degree of Ph.D. The work here to be described was carried out mainly in the laboratories of the medicinal firm Medicinalco Ltd. (Chief: Erik Jacobsen, M.D.).

The first paper of the series, published in Danish in 1941, dealt with the reticulocytes in the blood of old persons as compared with younger.

Reticulocytes are young erythrocytes (red blood corpuscles), showing after special staining a network (reticulum) of threads inside the cell, which contains no nucleus. The relative age of such a cell can be estimated from the density of the network, which, from being very dense and filling most of the cell in the youngest stage, is gradually reduced to points and finally disappears. The idea underlying the study was to obtain, by counting the reticulocytes in proportion to the mature erythrocytes and their different stages in relation to each other, a measure of the regenerative power of the organism. The countings on 20 persons between the ages of seventy and ninety years showed a somewhat smaller total number of reticulocytes and a more pronounced dominance of the oldest stage, which could only mean that the cells ripened more quickly in old age. This result would probably have discouraged most people, but for Plum it provided a foundation for the hypothesis that the ripening of reticulocytes might be accelerated by some substance present in varying quantity in the blood.

An interesting procedure was worked out to test this hypothesis. The number of reticulocytes in normal blood is so small that it would be next to impossible to obtain a satisfactory accuracy in counting them, but when in a rabbit the number of red cells is reduced to 2–3 million per cubic millimeter by removing 30–50 ml. of blood each day, a very rapid formation of new cells will take place, and at any time some 20–30 percent of the circulating erythrocytes will be reticulocytes. When the cells are washed free of plasma, they are suitable for experimentation on the ripening process in vitro. Some washed cells were sus-

pended in the plasma under test, which could be taken from any animal, some in a solution of liver extract (a commercial product called Hepsol), and some as controls in a saline solution, kept for some hours at 40°C. Small samples were taken for differential counting at 2-hour intervals. It was found that both plasma and liver extract definitely increase the rate at which reticulocytes ripen into mature corpuscles.

A great deal of time-consuming and very tedious work was spent in establishing a fairly accurate measure to express the concentration of the chemically unknown ripening substance, but the success attained fully justified the exertions. Concentrations can be expressed in terms of a standard with an accuracy which is practically always better than 10 percent.[1]

The plasma of all the animals examined contains a ripening substance acting upon rabbit reticulocytes. Some of the substance is present also within the red corpuscles and is responsible for the ripening taking place when the corpuscles are suspended in saline. Plasma from the ox, which proved exceptionally constant and strong, was taken as the unit for concentration and to it was assigned the "ripening index" 1.00, while in practice a "Hepsol standard" of the same strength was used.

The rate of ripening brought about by a certain amount of ripening substance depends upon the temperature, according to a characteristic curve. Putting the rate at 40°C (the highest temperature examined) at 100, it is found to be 79-81 at 37°, 49-51 at 30°, and only 5-6 at 10°. As usual in biological processes, the Q_{10}, calculated according to van't Hoff, decreases with rising temperature.

A very interesting relationship was brought to light between the number of reticulocytes present in the blood of an animal and the quantity of ripening substance found in the plasma. In mice the lower the number of reticulocytes, the higher was the ripening index. In the ox no reticulocytes were ever found. The relationship found for the single species holds approximately even when average values for a number of species are compared, but only for adults having suffered no loss of blood. It does not hold in young growing animals, where both the reticulocyte number and the ripening index are increased, and loss of red cells, whether brought about by bloodletting or in any other way, is constantly followed by increases, usually both in the ripening index and in the number of reticulocytes in the circulating blood. When the function of the bone marrow is damaged, the consequent anemia may present a reduced number of reticulocytes, but even then an increased ripening index, indicating that the bone marrow is probably not the seat of formation of the ripening substance.

It is a very significant and interesting fact, worked out in detail by Ruth Plum, that the concentration of ripening substance present in the blood of women begins to rise *before* menstruation sets in and is further increased along with the actual small loss of blood.

Experiments on liver extracts, carried out by E. Jacobsen and Plum to try to isolate the active substance, led to the separation of a thermostable fraction, itself possessing

[1] The very large number of countings made in connection with these studies led to important consequences for the statistical theory. When Ruth Plum's paper was submitted to the Medical Faculty as a thesis for the M.D. degree, it was strongly objected to on the ground that the accuracy obtained was several times better than could be accounted for by Bernoulli's binomial theorem concerning the random distribution of marked elements in samples of a homogeneous population. Exhaustive tests, in which subjective influences were carefully guarded against and which included countings on enlarged photomicrographs of the cells concerned, substantiated the empirical results and conclusively demonstrated the fallacy of the theorem for countings of the large numbers here involved which can, as a matter of fact, be carried out with the accuracy empirically determined.

slight activity only but able to increase considerably the activity of the thermolabile fraction. Further purification of the thermostable fraction led to the isolation of a substance, identified by Inger Gad as tyrosine, which, while itself showing very slight activity, definitely activates the principle present in liver extract, but shows no activating effect on plasma. As tyrosine is practically absent from normal plasma it was assumed that a coupling of the unknown thermolabile factor with tyrosine, or a tyrosine-like substance, had already taken place in the plasma principle. The bold hypothesis was formulated that such a coupling might take place in the "reticulo-endothelial system," and experiments were made on rabbits in which this system was blocked by splenectomy and injection of 10 ml. of 1 percent trypan blue. This had the effect expected; viz., the amount of active ripening substance became in 2 days greatly reduced. It recovered in the course of a fortnight, but during all this time the normal ripening index could be restored by addition to the plasma samples of 0.01 percent tyrosine; that is, the thermolabile factor remained present in normal concentration, but could not become activated until the function of the reticulo-endothelial system had been restored.

Similar conclusions could be drawn from experiments undertaken by C.M. and Ruth Plum, in which 5 ml. of liver extract were injected intramuscularly into a rabbit and plasma samples examined for their ripening power at suitable intervals. These tests were made simultaneously, with and without the addition of tyrosine, which had a tremendous effect at first, but after 24 hours very little.

Later studies by Inger Gad, Jacobsen, and Plum made it very probable that the tyrosine does not act as such, but is partly oxidized in the reticulocytes to dopa and hallachrome, of which especially hallachrome shows a hundred times stronger activating effect than native tyrosine.

Ruth Plum, studying the concentration of ripening substance in a number of patients, found it definitely reduced in cases of pernicious anemia, in exophthalmic goiter, and in several patients with stomach complaints.

C. M. Plum examined a large number of organs by extraction of dried samples and also by pressing out sap from the fresh tissue to find the source of the thermolabile fraction. Some activity was found in all organs, and in most it could be increased by addition of tyrosine; but only in the stomach was the difference very marked, and the pylorus of pigs, especially, turned out to contain a maximal amount of the substance which can be activated by tyrosine, while the content of the fully formed substance is small only. This points definitely to the pyloric mucosa as the seat of formation of the thermolabile substance in the pig and to an interesting relationship with Castle's "intrinsic" factor against pernicious anemia, produced in the same tissue.

The war situation opened up the possibility of obtaining from the forensic postmortems fresh human stomachs, and it turned out that in man both the intrinsic factor and the ripening substance are produced mainly in the *fundus* of the stomach.

The transformation of reticulocytes into mature erythrocytes is the final stage in the formation of red cells and takes place to some extent in the circulating blood as explained on the preceding pages. In adult mammals the earlier stages are found only in the red bone marrow, and the main problem is the formation of nonnucleated reticulocytes from the nucleated "erythroblasts." Plum began to study this in 1939 and published in 1941 a paper dealing, mainly from a histological point of view,

with the cell types found in the marrow, their mutual relations and numbers.

In 1942 a very valuable study of the production of red cells in the rat fetus was published by E. Jacobsen and Plum. It is well known that the initial mechanism of blood formation differs fundamentally from that obtaining later, and distinction is made between primitive nucleated erythrocytes (sometimes called megalocytes) with their parent cells "megaloblasts" and the final nonnucleated erythrocytes, produced from erythroblasts. The formation of primitive blood cells begins from the endothelium of the first fetal blood vessels and takes place somewhat later in several organs, especially the liver. The formation of final erythrocytes is, some time after birth, taken over exclusively by the bone marrow.

The chief merit of the paper by Jacobsen and Plum lies in the quantitative determinations, which allow an analysis of the formation, development, and retrogression of the different types of red cells.

Beginning on the eleventh day after impregnation, when the fetus weighs about 1 centigram, they find a total of 75,000 megaloblasts, but not yet any primitive erythrocytes. On the fourteenth day the number is 200,000, but they have produced 2.25 million nucleated erythrocytes, and at the same time the nonnucleated final erythrocytes begin to appear in small numbers. These increase rapidly, 10 being formed from each erythroblast between the fourteenth and fifteenth days, but 260 between the nineteenth and twentieth days, when the total number increases to 300 million, and the fetus, just before term, weighs about 2 grams. The megaloblasts disappear about the eighteenth day, and the number of nucleated red cells, which reached a maximum of 7 million on the sixteenth day, is on the twentieth reduced to 3 million.

In this paper there is further calculated for the adult rat a total production of about 9×10^9 erythrocytes per day, corresponding to 35–100 from each erythroblast in the bone marrow.

During the next two years all work was concentrated on the reticulocyte ripening principle, but Plum did not forget the problem of erythrocyte formation and the difficulty arising from the fact that there could scarcely be enough mitoses taking place to account for it. He took up the matter again in 1944 and constructed an apparatus for artificial cultivation of bone marrow modified from one made by Osgood. In this apparatus two concentric collodion sacs are suspended within a wider glass tube. In the innermost a flow of nutrition fluid is maintained to supply by diffusion through the collodion the cells outside. Between the two sacs bone marrow, obtained by sternal puncture from dogs, cats, rabbits, or man, is suspended in Ringer's solution and kept aerated to provide oxygen and remove CO_2. The outside space is filled with Ringer's solution which, serving as a kind of kidney to remove waste products, is changed from time to time. In these conditions at body temperature there will be a vigorous production of new erythrocytes, which can be followed by counting samples at suitable intervals of time. Such countings show that the mitoses of erythroblasts taking place, and also counted, could not account for more than 1 percent of the erythrocytes produced.

The mitotic activity could be stopped completely by the addition of colchicine to the suspension, but this had practically no effect on the rate of the red-cell production. It does not appear possible, therefore, that the generally accepted notion, that red corpuscles are formed by cell division with subsequent removal of the nucleus, can be correct.

In 1940 the Swedish nurse Lisa Boström suggested a formation of erythrocytes by

disunion of pseudopodia from the erythroblasts, and, since such a mechanism might explain Plum's result, he endeavored to observe it directly. A simplified small culture chamber was constructed on the same principles as the large one. Under the microscope one can see how a spherical erythroblast becomes oval with the nucleus at one end and how protoplasm flows toward the opposite end where suddenly a drop of it is detached and forms a small erythrocyte, which in the course of about an hour becomes a reticulocyte.

Further studies by means of the large cultivating apparatus, which are still in progress, go to show that the reticulocyte ripening principle also has an accelerating effect on the earlier stage of erythrocyte formation, and the tests made in this instrument on the blood and bone marrow of patients are coming into use for diagnosis.

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ABSORPTION SPECTRA

A shaft of gleaming marble has at best
But surface sheen. Its atoms scatter rays
With scant absorption. Pallor here betrays
An inner lack externally expressed.
But rosy quartz, a stone that is a nest
Of lovely hues, is one within whose maze
Electrons vibrate, capture light and blaze
Like clouds that sunset fires along the west.

Perhaps this truth may have for us a cue
If blind resistance makes us always spurn
What life would give, our souls are pale and drawn;
But if, with shutters wide, we face the new
And let experience deep within us burn,
Our lives will be as colorful as dawn.

F. Marion Lougee

OKINAWA AND ITS PEOPLE—II*

By PAUL E. STEINER

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(On leave of absence from the Department of Pathology, The University of Chicago)

Agriculture. Okinawa had an agricultural economy. Farming, perhaps more accurately described as gardening, was by far the most important occupation. Because the population was great, agriculture was so intensive that many calories of food were produced per acre. Unfavorable terrain was brought under cultivation by ancient but highly developed methods for preventing soil erosion, by irrigation, terracing, and devices for reclaiming land from the sea. The climate, fortunately, was favorable so that there were crops maturing at all seasons, and multiple crops per year were grown on some soil. The range of crops was further extended by water-impounding devices and irrigation. By these methods a country whose maximum productivity had been estimated at one time as capable of feeding a population of 300,000 had become nearly able to support 460,000.

The land in general appeared fertile, although there was some evidence of depletion from long use. Despite the heavy annual rainfall, little water ever flowed back to the sea. Crop failures sometimes occurred because of too little or too much rain at crucial times or because of typhoons which caused the coastal flats to be inundated with salt water, thus rendering the soil unusable for several years, or until sufficient salt had leached away. When rain fell after a drought at planting time, school was dismissed and everyone worked in the fields. The soil had a peculiar texture so that its angle of repose when very wet was low, as our engineers soon discovered. Terracing of entire steep hillsides was extensively practiced in the northern part of the island despite this handicap.

The farms consisted of only a few acres each and were subdivided into small plots according to surface contour. Each plot was devoted to a different crop, giving the countryside a garden-like appearance. Retaining walls were composed of rocks, or of soil covered by sod or by a dense hedge of shrubs, cycads, and large grasses. All excess water was carefully collected and conducted from one level to another as needed. Rice paddies were consequently found on hillsides as well as in the valleys.

Although differences were found from one part of the island to another, the acreage devoted to the different crops was estimated to be in the following descending order: sweet potatoes, beans, sugar cane, and rice. Other crops fell far below these four.

Because of the favorable climate, with no frost and with rainfall at all seasons, crops of one type or another were growing the year around. Two to three crops of sweet potatoes could be harvested each year from a plot of ground. Likewise, two crops of rice could be gotten from some land. Soybeans, however, yielded only one crop each year, and sugar cane required from one to two years before it was ready to be cut.

Although nearly every farmer had a few

* Continued from page 241 of preceding issue.

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pigs, goats, and chickens, horses were found only on the larger farms, and most of the labor was done by hand. All the horses were small, some of them little larger than ponies. Cattle also were found only on the larger farms. They were large, black-and-white, and resembled Holsteins, although Durham cattle were formerly also popular. The goats were small and white. Hogs were of several standard breeds, both lard- and bacon-producing types. Both hogs and cattle were always confined to sty or stable. All offal from man and animals was carefully saved and together with ashes was used for fertilizer. Commercial fertilizers were introduced about 1900 to supplement the local supply, but because they were expensive they were not used in great amounts.

Sugar cane was the chief cash crop. It was pressed and processed either in small horse- or ox-powered presses or in larger community or commercial refineries. Other sources of cash income were surplus rice, soybeans, sweet potatoes, or animals.

Most of the farming was done by hand with crude tools, most important of which was an adz resembling a combination hoe, shovel, spade, and mattock. The more affluent farmers who owned an ox or small horse had a small two-wheeled cart and a crude plow. The cereals were threshed by flail or by a hand-powered thresher. "Weeds" were not tolerated, and the soil seemed free of weed seeds, as was indicated by the fact that few sprang up on deserted land or on freshly turned soil along roads and about military installations. On the other hand, sweet potatoes, tomatoes, and squash grew profusely and wildly as weeds in the most unexpected places, such as in the ashes of burned buildings and in the margins of shell holes.

Tea and coffee for local island use were grown in the northern hills at Tiara; they were said to be of mediocre quality. Dried soybean leaves were also used for brewing tea. Cotton was grown by some families for their own use. This was formerly an important crop until it became cheaper to buy Japanese machine-made cotton cloth. Tapioca, taro, lilies, and other root plants were grown on a small scale.

Nearly every farmer grew bamboo, cycads, shrubs, and trees in the hedge about the house, on boundary lines, along the larger terraces and irrigation ditches, and around outcropping rocks. The varieties of trees included conifers and numerous slow- and fast-growing, hard- and soft-wood species, some of which were scented or colored. They were selected for their usefulness as protection against typhoons and soil erosion, producing replacements of lumber for repairing buildings, and for making furniture. Banyan trees were trained to form an important part of many windbreaks. The bamboo was very important to these people: It was used in the construction of buildings, furniture, farm and kitchen utensils, fences, baskets, and screens. Another important plant was the cycad (*Cycas revoluta*) known as *sutichi* (Japanese, *sotetsu*). It was planted to mark boundary lines and to prevent erosion of soil on terraces. The stem was dried and used for food and to make sago starch, the "nuts" were used for food, and the leaves were used to make brooms and for fuel. The coconut palm was rarely grown on this island because, according to the natives, the typhoons are too severe and the temperature in winter is too low. The pandanus, growing along the seacoast, was used to prevent soil erosion, to protect against typhoons, and to make rope, mats, and hats.

Commerce and Industry. The Okinawans claim that centuries ago they were an important commercial nation whose sailing vessels roamed the seas from Japan to the islands of the South Seas and intermediate places such as Korea, China, the Philippines, Indo-China, and Siam. Having

come under the domination of Japan, Okinawa became isolated when Japan adopted the closed-door policy in 1638. An agricultural economy, which has persisted to date, was then forced on the country, and commerce on a large scale was not resumed even after Commodore Perry visited Naha in 1853–54 on his way to and from Japan. Fortunately, sweet potatoes and sugar cane, two of the highest-yielding crops, were introduced in 1623, just before outside commerce ceased. The importance of this event is indicated by the shrine Yomochi-jinja (*jinja* = shrine) dedicated to the importer of these plants, one Gima Singo. Yomochi-jinja is located on Ono-yama, a small island in Naha Bay, which it shares with the Gokoky-jinja, a shrine dedicated to Okinawese soldiers.

By a combination of circumstances, including a favorable climate, fertile soil, fortunate choice of crops, and an industrious, frugal people highly trained in crafts, the Okinawans became self-sufficient at a fair standard of living. There are few known important mineral resources, little potential water power, and not enough forests to supply local demands. Commerce, then, centered about the products of the soil and the sea.

The foremost industry, that of making and exporting sugar, had sprung up in connection with agriculture. Though much of the sugar was made in small plants on the farms, a few large refineries were in operation, using coal imported from several small islands near Formosa. They operated from December to May. The largest plant, said to have been financed by capital from Formosan sugar interests, was located at Bisha-gawa near Kadena. The difficulties of transportation between the farms and the factories tended to limit the size of the latter.

The center of commerce, industry, shipping, and finance was the largest city and main seaport, Naha, the capital. Exports from this port were said to be approximately in the following order: (1) sugar; (2) fresh vegetables such as cabbage, tomatoes, cauliflower, etc.; (3) sea foods, including dried tuna fish, shark fin (to China), and cuttlefish; (4) Okinawan-style cloth made from *maou* fiber, and also a little silk and banana fiber cloth; (5) sake; and (6) cattle, of which 50 to 100 were shipped to Kobe or Osaka several times a month. Small amounts of silkworms, kitchen utensils, and lacquered furniture were also exported.

Imports were chiefly food, cloth (cotton and woolen), and machinery. Imported food included rice, canned crab meat and salmon, soybeans, and fresh fruit such as oranges, persimmons, plums, and apples.

Besides Naha, other cities having commercial interests were Shuri, Nago, Toguchi, and Itoman. The fishing industry was conducted from numerous small seaports, foremost of which were Itoman and Toguchi. Salt for local island use was made from sea water at Awasi, Baten Ko, and Naha. The best quality coral stone came from the Machinato, Minatoga, and Yontan regions. That obtained from the first two locations was fine like sandstone; the Yontan stone was coarse but tough and hard. Volcanic stone, used for millstones, came from the Kuba-saki region. Sake, of a special Okinawan type, was made at Naha, Shuri, and Nago. Vinegar for local use was made from the rice residue. Tile, used principally as roofing material, was manufactured by numerous small factories; pottery, including urns, was made at Tschoboya.

Electric current was generated at Nago and at Naha. From the latter a power line led through Shuri and Yonabaru to Sashiki. Sugar refineries used steam; a few small industries had gasoline engines.

Small shops and stores were found in all urban centers. Shopkeeping probably ranked in importance after agriculture and fishing. Shops were strictly licensed.

Transportation was by boat, foot, or horseback until about 1900, when two-wheeled carts were introduced. Later, narrow-gauge railroads were built from Naha to Kadena, Yonabaru, and Itoman. An electric railroad from Naha to Shuri lost money and was abandoned. Recently bus lines have been operated from Naha to many parts of the island, including the Motobu, Katchin, and Chinen peninsulas. There were said to have been only a few privately owned automobiles, but many people had bicycles. A few rickshas were in operation at Naha and Shuri.

The highway system was adequate to serve the needs of the people. Radiating in several directions from Naha were narrow concrete roads which extended to the adjacent towns. Beyond these was an extensive system of hard gravel roads from which branched the narrow cartroads already described. The principal hotel, at Naha, was said to have been modern and luxurious.

Economics. Capitalism replaced feudalism on Okinawa in comparatively recent times. Private ownership of the land was encouraged by the Japanese beginning about 1900. Trade was by cash rather than barter; the unit of currency was the yen, which was the same as that used in Japan. Taxes were said to be high.

Although, according to rumor, some of the old families were wealthy because of stored gold, the fortunes made in recent times were made mostly by Japanese. The main sources of cash were farm produce, fishing, wages, and relatives abroad. Unskilled labor brought 1.5 to 3 yen per day, and skilled workers such as carpenters earned from 6 to 10 yen. Most men were proficient at several kinds of work; thus, farmers commonly worked part time at fishing, lumbering, or in the sugar refineries.

The people were not sharply divided into social classes, but three general groups were recognized on the basis of wealth. In the third group were the farmers, in the second the professional men, storekeepers, and other villagers, and in the first the very wealthy. These included the brokers, especially those in rice, sugar, silkworms, and real estate, the large city merchants, and a few who returned with fortunes from other lands.

Economic pressure forced many to leave the island to seek their fortunes elsewhere. A large number of the young men and women went to Japan, where they worked in the industrial cities, especially Osaka. The doctors stated, incidentally, that many of these young people returned home with tuberculosis. Smaller numbers went to Formosa, to the Hawaiian Islands, to the Marianas, and to South America, notably to Peru and Argentina. Only a few came to the United States.

Arts and Crafts. The Okinawans were highly skilled in making most of the necessities of life and some of the luxuries. Consequently, although little was purchased, living was comfortable at a fair level. Their workmanship was utilitarian rather than decorative. In quality and artistry it lagged behind the best Chinese and Japanese work. No evidence was found that it had ever reached a higher level than that which prevailed just prior to the American invasion.

Engineering skill of a high order is exhibited by the soil-erosion, water-conservation, and irrigation systems. This was very soon discovered by anyone required to map and drain them for mosquito control. Although the tools such as saws and hammers appear crude, the skill and workmanship used in construction of the houses, with their elaborate system of panels and flexible, mortised frames fully able to survive typhoons and earthquakes, were quite remarkable. Skill in stonework was less impressive but adequate; coral and volcanic stone was dressed by hand and carefully fitted. Absence of freezing

and thawing made this type of construction very durable. The use of mortar was quite recent. Stone arches claimed to be 500 or 600 years old, built without a central keystone, survive today in Nakagusuku Castle. The furniture, usually simple and functional but sometimes elaborate and beautifully lacquered, also showed skilled workmanship. Cooking utensils were chiefly plain pottery or iron, but every family had chinaware of good quality and pleasing design for ceremonial and formal occasions. Pottery jars of all sizes and designs were very important in the kitchen and for storage of foodstuffs inasmuch as they could be effectively closed for protection against the prevalent dampness. Chibana pottery, named after the town where it was made some centuries ago, was especially prized. Its manufacture antedates the use of the potter's wheel; it is further characterized by a hard texture and a dull glaze which has a greenish sheen.

The Okinawans were excellent weavers. They made baskets, large hats, and trays from pandanus, bamboo, and other grasses and reeds; they wove ropes and baskets from rice straw and grass. Nearly every family produced silk cloth for home use. A distinctive, highly valued cloth known as *maou* was made from the fiber of a plant called *oube*, which resembles the new shoots of blackberry bushes. From banana fiber was constructed, among other things, a fiber cloth, some of which was exported. Rainproof capes were made from the fiber of the *shuro* palm. The women were skilled at making clothing from silk, plant fibers, cotton, and wool. Their creations from castoff military garments were wonders to behold.

The native dances were said to show evidence of Chinese and Japanese derivation with native additions. Professional entertainers sang native songs, recited poems, and produced plays and vaudeville. The principal musical instrument resembled an undersized banjo; it had five strings, and the sound box was covered by snakeskin. Organs were found in a few homes.

Education and Religion. The educational system on Okinawa consisted of grammar, intermediate, high, and special schools, all operated at government expense. The courses of instruction were six, two, and five years, respectively. Children of both sexes attended the same lower schools, but they were taught, if possible, in separate rooms. Most children went to school for six or eight years. To attend high or special schools it was necessary for most students to leave home. The special schools offered courses in agriculture at Kadena, fishing at Itoman, business at Naha, carpentry at Shuri, and teaching at Naha. For education at the college or university level, it was necessary to leave the island, and the number who did so was restricted by financial considerations. Most of them went to the Japanese universities, though a few went to Formosa.

English was taught in some of the schools, but only a few children were found who could speak it. The school building was the most impressive edifice in the villages; it was a long, one-story, wooden building, the width of one room, and usually built around two or three sides of a schoolyard. In the cities, also, the school was the outstanding building. Here the construction was of reinforced concrete.

The religion was an ancient form of family worship overlaid by a veneer of Buddhism and a lesser amount of Shintoism, all inextricably mixed. Man had a God-like origin, and each generation by inheritance partakes of this spirit. Man's spirit, after death, because of its God-like origin and nature, is worshiped by his descendants. Man is composed of body and spirit; after death the former disintegrates, but the remains are held in reverence. The spirit leaves the body and enters the hereafter (*Gusho*) where later it divides. Part of it

becomes, or merges with, God. The remainder is reincarnated within seven generations in some descendant.

These religious beliefs determined and found expression in two customs which were almost universally practiced among the Okinawans; namely, their burial ceremonies and the those centering about the family shrine.

Each family had a private tomb, or crypt. It was located on or near the house in the side of a hill or outcropping of rock. It was constructed of fitted hewn stone or, more recently, of concrete. The exterior exhibited a large, sloping, smooth, horseshoe-shaped top which was convex. In front this top was continuous with a vertical face, in the center bottom of which was a small entrance less than 3 feet square leading into the vault. The front face also had bilateral abutments from which low lateral and forward extensions formed the sides of a small square yard in front of the tomb [See cover, SM, January 1947]. The interior was small, dark, and damp. Across the rear was a series of ascending stone steps, or shelves, on which were located urns containing the ancestral bones.

The symbolism behind this form of tomb is that it represents the uterus, or abdomen, legs, and genitalia of Woman. Man comes from Woman (his ancestors), and after death he returns to them. This style of tomb, known as Kame-No-Ko ("Shell of the Turtle"), was imported from South China many centuries ago. While its present purpose is that of observation of traditional religious forms, it has a very definite practical advantage in an overpopulated country centuries old, where land and food are at a premium, in that it does not use arable soil. The accumulated graves of centuries as in Occidental cemeteries would by now deprive the people of a considerable percent of their land.

Modifications of this typical tomb took several directions. Poor families might use merely a simple cave with stone entrance. Sometimes the curves of the top were replaced by straight lines and flat surfaces. North of Nago the tombs are often small vaults standing apart out in the open. Here, also, a separate, slender, upright stone or concrete marker is found beside the low entrance of the tomb.

After death the body was placed in the tomb in a wooden coffin together with sake, tea, tobacco, slippers, needle and thread, and a paper hat. About three years later the tomb was unsealed and the bones were cleaned by women relatives and placed in an urn, which was labeled and placed on one of the rear shelves. Bones of husband and wife occupied one urn. All ancestors were thus filed in an orderly fashion. The only exception to this procedure was in the case of those who had leprosy or any other disease considered to be dangerous. These bodies were placed in separate crypts and remained untouched.

It was these tombs, together with new caves dug by the military especially for defense, which the natives and Japanese soldiers occupied during the campaign and which proved so formidable and costly to capture.

The family shrine was located in the main room of the house as already described. A cabinet with sliding panel front, 4 to 6 feet long, 3 feet high, and 2 feet deep, was surmounted by a smaller cabinet which contained in its center the family spirit name plates. The name plates were lacquered pieces of wood about 4 inches long, 1 inch wide, and .25 inch thick. They were mounted in two rows, upper and lower, within a small lacquered frame, often of exquisite workmanship, sometimes bejeweled, about a foot square, and mounted upright on a base. On each plate was painted the biographical data of one ancestor. Males were placed in the upper row and females below. These

family shrines are called in Okinawese, *eafay;* in Japanese, *eihaii.* After death a temporary name plate was painted in front with a false name provided by a Buddhist priest and behind with the true name. Prayers were offered once a week for seven weeks, after which this name plate was burned and a permanent, true name plate placed in the frame.

The shrine with its spirit name plates preserved in the minds of the people in a fresh and real way the lives and achievements of their ancestors. They could recite in great detail biographical data about their ancestors, remote as well as recent. Ceremonials, attended by the entire family, were frequently—sometimes twice each month or at anniversaries of birth, marriage, or death—held at the family shrine. At this time the shrine was decorated with vases of flowers and food and drink.

Because of the events believed to occur after death and the reverence in which ancestors were held, death was not dreaded. It was considered not a termination but a promotion. For many the hereafter promised more than they got from this life.

The homes occasionally contained a second type of religious symbol. This consisted of a Buddha enclosed in a small lacquered cabinet with hinged doors.

The larger towns and cities were said to have had Buddhist priests in attendance at temples, most of which were destroyed during the war. The largest and most famous was the Yenkaku-ji (*ji* = temple) at Shuri. Being located near the heavily defended castle, it was entirely destroyed. At Naha were located the Gokoku-ji and Tenlikyo-ji, both of which were near the famous Shinto shrine, Mami-no-ue ("Above the Waves"), the Hongwan-ji, and others. In Tomari, a suburb of Naha on the Shuri Road, were two famous temples, the Sogen-ji and the Amiku-ji. Still others were found at Futema and other towns. Some of these temples show Shintoist architectural features, just as some of the Shinto shrines were constructed in the Buddhist manner.

The smaller towns and villages had simpler Buddhist temples. They consisted of small concrete or stone structures located at sacred sites on some rocky eminence above the town, in a grotto or cave, or in a grove of ancient, stately trees. The temples were sometimes so small that the people did not enter but worshiped from without. They were built in the Chinese style, although some of them showed Shinto influences. An excellent example is found at Kashaba.

Shintoism was introduced from Japan in the seventeenth century. Architecturally, it was recognized by Shintoist additions or modifications on older religious structures and by numerous comparatively new, purely Shinto, shrines. These shrines were marked by *torii*, the peculiar double-barred wooden arch. They were erected on sites which from ancient times had been held sacred by the Okinawese, and honored various historical persons and benefactors.

In actual practice the people did not distinguish sharply between their ancient Animism, Chinese Buddhism, or Japanese Shintoism. Elements of each were intermingled in architecture and religious practice. The core of religious observation was in the home and in the family worship. The people attended the Buddhist temple several times each month or perhaps only once a year. The burial ceremony was usually according to Buddhist rites. Shintoism, the religion adapted to Japanese nationalism and militarism, was superimposed on, but less influential than, ancient Animism and Buddhism. It has been said that the people never accepted it as anything more than a recognition of political overlordship and that attendance at ceremonies by school children and others was sometimes compulsory.

THE STORY OF TWO ATOMS

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IT IS now a matter of common knowledge that the nuclear model of the atom first proposed by Rutherford in 1911 is basic for all the natural sciences. On this model an atom is pictured as a minute positively charged nucleus, in which the greater part of its mass is concentrated, surrounded by a number of electrons. Under normal conditions, the number of electrons surrounding the nucleus is such as to make the whole atom electrically neutral. However, atoms with a net positive or negative charge are also known to exist. Thus, atoms with a positive charge can be obtained by simply removing one or more of the outer electrons of a neutral atom. Such *positive ions*—as they are sometimes called—can be produced in the laboratory under suitable conditions (usually in a spark discharge); they are also spectroscopically observed in the atmospheres of the sun and the stars where the high temperatures and low pressures prevailing are appropriate for their occurrence in the free state. On the other hand, *negative ions*, which in all cases known consist of one additional electron attached to a neutral atom, are of less common occurrence: indeed, whether a particular neutral atom can or cannot form a stable atomic configuration with an additional electron is a question not always easily answered. When the stability of a negative ion can be established, the corresponding neutral atom is said to have a positive *electron affinity*.

It is evident that on the Rutherford model, the simplest type of atomic structure will be represented by atoms (or ions) with one electron. The prototype of these is of course the hydrogen atom, which consists of an electron in the field of a proton. Singly ionized helium,[1] doubly ionized lithium, or triply ionized beryllium are also examples of one-electron systems. The next simplest type of atomic architecture will be represented by atoms (or ions) with two electrons: and the prototype of these is helium (named by Norman Lockyer after ἥλιος, "the sun"). Although other atomic configurations with two electrons (such as lithium, once ionized, or beryllium, twice ionized) are well known and have been produced in the laboratory, the case of the *negative hydrogen ion* consisting of an electron stably bound to a hydrogen atom is unique in that its stability has been established on purely theoretical grounds and it has been positively detected only in the sun's atmosphere. Indeed, it is remarkable that the story of the discovery and the identification of the two atoms, helium and negative hydrogen ion, should be so intimately connected with astrophysical studies relating to the constitution of the sun's atmosphere. And the story is worth telling since it illustrates once again the basic unity of all the sciences—in this instance, the unity of astronomical and laboratory physics.

IN THE year 1706 there was an eclipse of the sun visible in Switzerland, and there was one Stannyan who gave an account of what he saw at Bern. After describing the phenomena of the eclipse, he said, referring to the sun, "His getting out of eclipse was preceded by a blood-red streak of light." There are no photographic rec-

[1] An atom is said to be once ionized if one electron from the neutral atom has been removed, twice ionized if two electrons have been removed, and so on.

ords of the eclipse of 1706 (it was before the days of photography and astronomical spectroscopy), but we do have records of similar later eclipses, and in all of them Stannyan's observation has been confirmed. Indeed, it is now known that the origin of the blood-red streak which Stannyan first observed is to be found in one of the solar envelopes to which the name *chromosphere* was given by the English astronomer Norman Lockyer.

In the year 1868, during an eclipse of the sun visible in India, the spectroscope was first put into effective use for the study of the chromosphere by Lockyer, Pogson, and Janssen. In Figure 1 we have illustrated Pogson's original diagram of the spectrum of the chromosphere. It will be seen that in the spectrum there is a *bright line* appearing in the position of the dark (Fraunhofer) D line of the normal solar spectrum. Referring to this yellow line, Pogson said that it was "at D or near D." Almost the whole of the story of helium depends on this distinction.

After the eclipse of 1868 a method suggested much earlier for studying the chromospheric prominences without waiting for an eclipse was put into operation and has been extensively used ever since. The method consists in forming a telescopic image of the sun on the slit of a spectroscope so that the spectrum of the sun's edge and of the sun's surrounding envelope could be seen simultaneously. Consequently, exact coincidence or want of coincidence between the bright lines of the chromospheric prominences and the dark Fraunhofer lines of the normal solar spectrum could be ascertained at once. By using this method, it was shown that there was exact coincidence between the lines C and F ($H\alpha$ and $H\beta$, respectively) appearing as dark lines in the solar spectrum and the strong bright lines in the red and the blue-green regions of the spectrum of the "blood-red streak" (Figs. 2 and 3). On the other hand, no such coincidence was found between the D lines originating from the sodium vapor present in the solar atmosphere and the bright yellow line of the prominences (Fig. 4). The prominence line in this position has no connection whatever with the dark lines of the ordinary solar spectrum. The considerable significance attached to this divergence by Lockyer, Janssen, and others who had examined the matter is therefore understandable. The new line was accordingly called D_3 to distinguish it from the sodium lines D_1 and D_2, and the presence in the solar atmosphere of a new element, "helium," not terrestrially known at the time, was concluded.

A considerable amount of laboratory work was done with regard to the D_3 line.

> But the substance emitting the yellow ray lay outside the range of our acquaintanceship, and seemed unlikely to be brought within it. That contingency, nevertheless, came to pass. In the

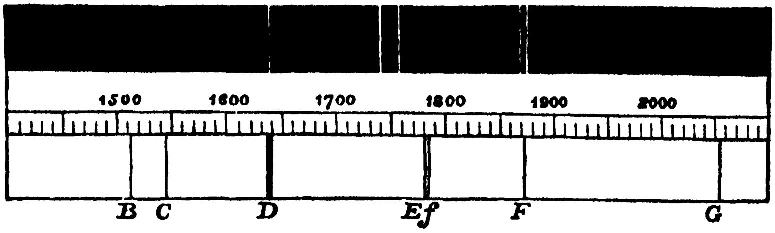

Fig. 1. Pogson's diagram of the spectrum of the sun's envelope during the eclipse of 1868. The bright lines which were seen are shown in the upper part of the diagram and the principal dark Fraunhofer lines in the normal solar spectrum are shown in the lower part.

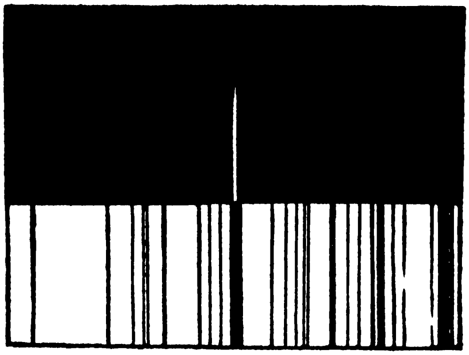

Fig. 2. The coincidence of the red chromospheric-prominence line with the dark line $C(H_\alpha)$.

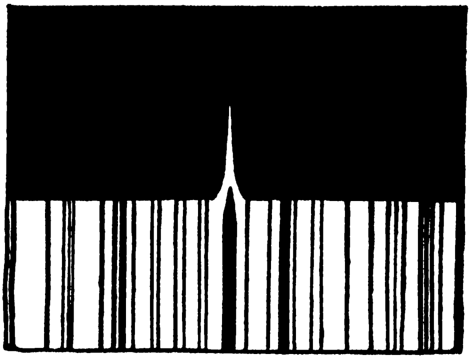

Fig. 3. The coincidence of the blue-green chromospheric-prominence line with the dark line $F(H_\beta)$.

course of a search for compounds of argon, Professor Ramsay, at the suggestion of Professor Miers,[2] fortunately examined the reputed nitrogen occluded by the Scandinavian mineral 'clevite." This velvety-black stone, remarked as peculiar by Nordenskiöld and analyzed by Cleve, is a kind of pitchblende, composed of uranate of lead mixed with rare earths. The gas evolved from it at University College [London] gave a brilliant spectrum in which the prominence-line D_3 shone conspicuous. Helium was indeed captured! A beautiful confirmation of the identity was soon afterwards afforded. The golden line seen in the laboratory was perceived by Runge to have a faint close companion, and he declared that, unless the solar D_3 were also double, clevite-gas should be regarded as different from helium. The challenge was taken up on both sides of the Atlantic. Professor Hale on the 20th of June, 1895, and Sir William Huggins independently on July 10th, succeeded in resolving the prominence-ray into a delicate, unequal pair, and our possession of helium as a truly indigenous element was rendered incontrovertible.[3]

THAT a neutral hydrogen atom, together with an electron, can form a stable atomic configuration was first conclusively proved by H. A. Bethe and E. A. Hylleraas, independently, in 1930. Since this is a unique instance in which the stability of an atom had been first established from theoretical considerations, it is of interest to see how Bethe and Hylleraas arrived at their conclusion.

On the quantum theory any arbitrary function of the coordinates of the electrons of an atomic system represents a possible state of the system; and the *probable*

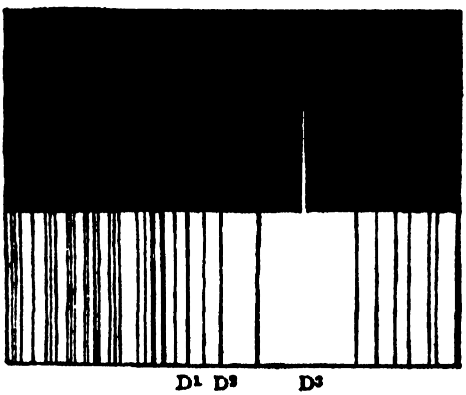

Fig. 4. The want of coincidence of yellow D_3 line with the dark lines D_1 and D_2 of sodium vapor in the sun's atmosphere.

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[2] Professor Miers, of the British Museum, recalled to Ramsay that W. F. Hildebrand, of the U. S. Geological Survey, had observed in 1889 that when certain uranium-containing minerals were boiled with sulphuric acid, a quantity of gas was evolved. The gas was, however, supposed to be nitrogen as its spectrum showed the characteristic fluting, and, consequently, it was not further investigated.

[3] Clerke, Agnes M. *Problems in Astrophysics.* London: Adam and Charles Black, 1903, 56.

energy (i.e., the energy which would be obtained as the average of many independent determinations on the system in the state considered) appropriate to the state in question can be found in a unique manner according to established rules of the quantum theory. If the atomic system considered has a stable ground (or normal) state then the function (*wave function*, as it is then called) describing such a state will lead to an energy which will be *less* than that given by *any* other function. This is one of the fundamental principles of the quantum theory. And we shall now indicate how this principle can be used to decide whether a given atomic system has or has not a stable ground state. We shall illustrate this by considering specifically the case of the negative hydrogen ion. The question is: Can an electron attach itself stably to a hydrogen atom?

We know that a proton and an electron can be stably bound together to form a hydrogen atom. And the energy of binding is 13.54 electron volts, meaning that an amount of work necessary for displacing an electron against a potential difference of

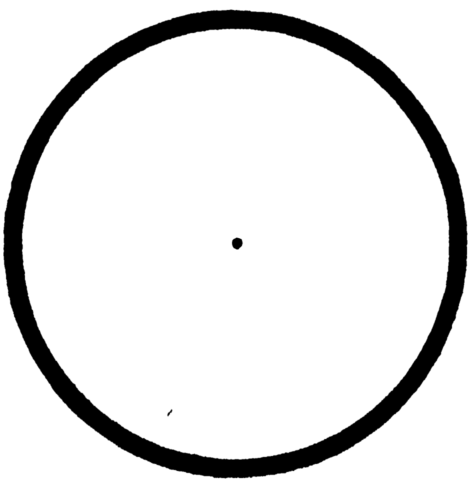

Fig. 5. A model of the hydrogen atom in its ground state. The electron will be found on the average at a distance of 0.79×10^{-8} cm. from the proton.

13.54 volts will be required for separating the constituents of a hydrogen atom, to rest, at infinite separation. We may accordingly say that the energy of the normal state of the hydrogen atom is −13.54 electron volts if we take as our zero of energy the state in which the charged particles constituting an atom are at rest at infinite separation. If the hydrogen atom together with an electron can form a stable atom, then its energy should be *less* than −13.54 electron volts: for, if a stable negative hydrogen ion exists, a finite amount of work should be necessary to separate the additional electron to infinity and leave a normal hydrogen atom behind with an energy of −13.54 electron volts. Consequently, *if* we can find *some* function which, according to the rules of the quantum theory, will lead to an energy less than −13.54 electron volts, we shall have proved the stability of the negative hydrogen ion: for the normal state must have an energy *lower* than any that we can find—unless we have been lucky enough to guess the correct wave function itself!

Bethe and Hylleraas were able to do precisely this: they were, in fact, able to isolate a function of the coordinates of the two electrons in the field of a proton which gave an energy of −14.24 electron volts indicating that the electron affinity of hydrogen is positive and must *exceed* 0.70 electron volts. A more recent theoretical determination has increased the value of the electron affinity of hydrogen to 0.75 electron volts; there are reasons to believe that this last value must be very near the true value.

Having seen that the negative hydrogen ion as a stable atom exists, we may ask, "What does it look like?" In one sense, it is not reasonable to ask such a question since on the quantum theory we cannot locate or individually identify the different electrons in an atom: we can only give the *probability*

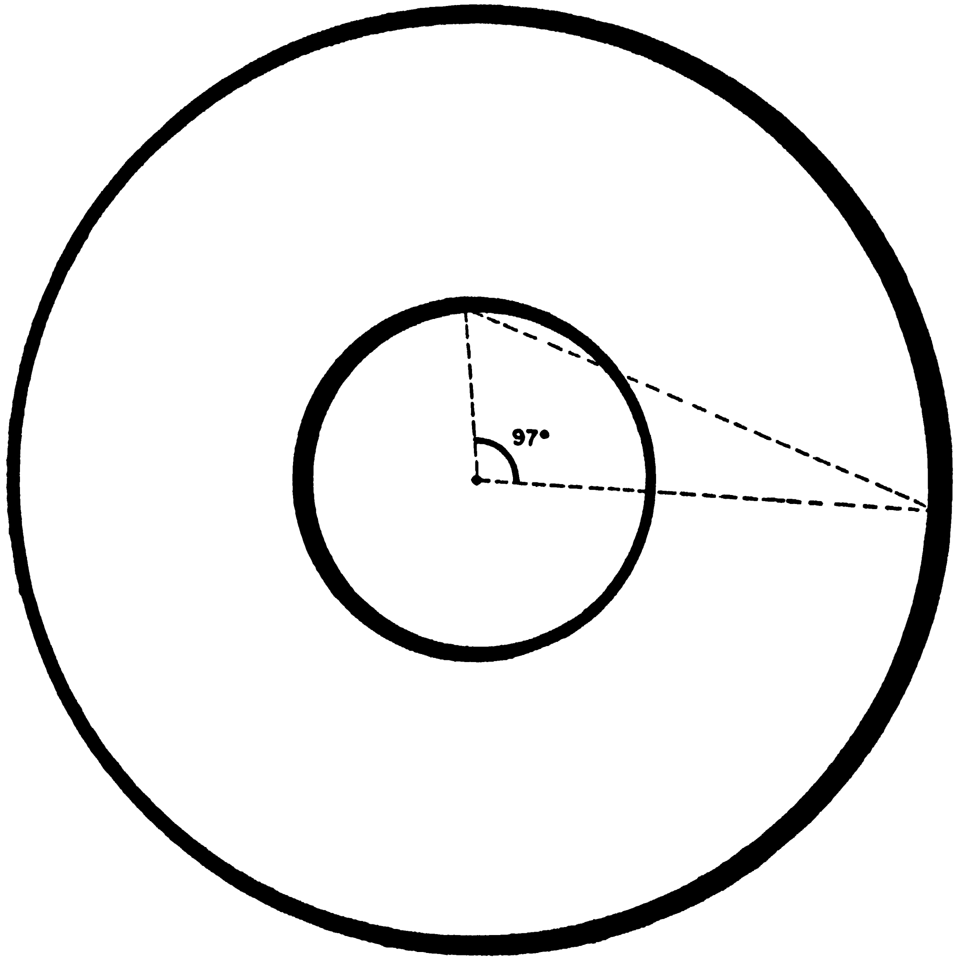

Fig. 6. A model of the negative hydrogen ion. The inner electron will be found on the average at a distance of 0.75×10^{-8} cm. from the proton while the outer electron will be found on the average at a distance of 2.02×10^{-8} cm. from the proton. The fact that the two electrons will be more often found on the opposite sides of the proton than on the same side is indicated by the unsymmetrical shading of the orbits. Calculations show that on the average the angle between the lines joining the proton to the two electrons will be about 97°.

that the electrons will be found in assigned positions. Nevertheless, we may ask where the electrons will be found on the *average*. In a hydrogen atom in its normal state, for example, the electron will be found at an average distance of 0.79×10^{-8} cm. from the proton (Fig. 5). With the same reservations, we may picture the negative hydrogen ion as in Figure 6, with the inner electron at a distance of 0.75×70^{-8}cm. from the proton—this is only slightly less than it is in the hydrogen atom—and the outer electron at a distance 2.02×10^{-8} cm. from the proton. However, the positions of the two electrons are correlated in the sense that the two electrons will be found more often on the opposite sides of the proton than on the same side: it is as though

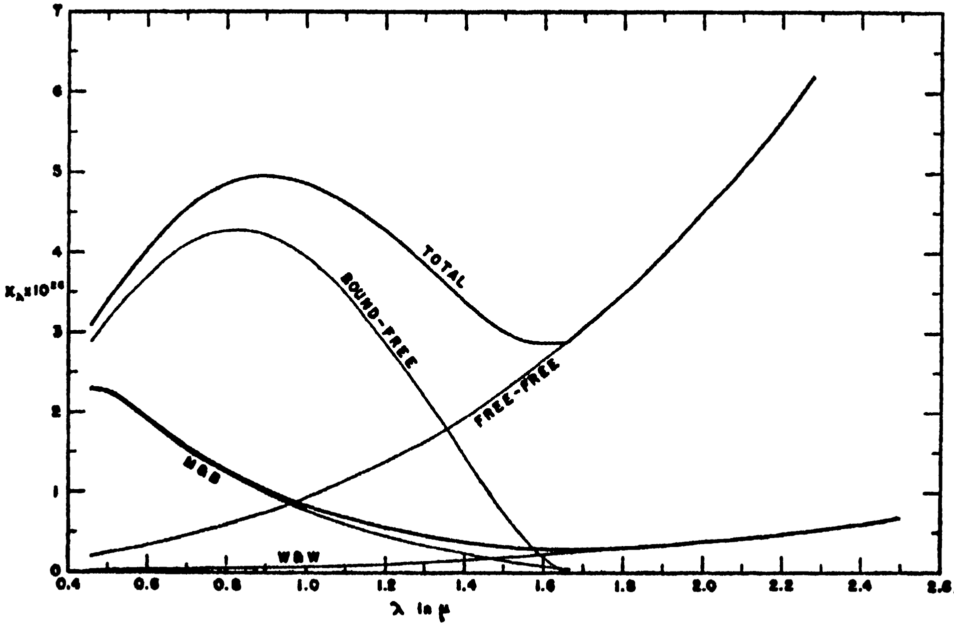

Fig. 7. The absorptive power of the negative hydrogen ion for light of various colors derived from physical theory. The curves are drawn appropriately for an atmosphere at a temperature of 6300° Kelvin and an electron pressure of one dyne/cm.^{2} and per neutral hydrogen atom. The curves *M* and *B*, *W* and *W*, and their sum are the results of earlier determinations by Massey and Bates, and Wheeler and Wildt. The results of more recent determination by Chandrasekhar and Breen are illustrated by the remaining curves.

the inner electron is accommodating enough to let the outer electron have an occasional glimpse of the proton to strengthen its binding!

We have indicated how the principles of the quantum theory enable us to determine the stability and structure of the negative hydrogen ion. But the atom has not so far been isolated with definiteness in the laboratory. We shall now show how its presence in the solar atmosphere has recently been inferred.

It is known from careful measurements of the *solar constant* (which gives the amount of radiant energy received from the sun, at the earth's distance, by a unit area normal to the line joining it to the sun) that the sun radiates 3.78×10^{33} ergs per second to the space outside. Since the radius of the sun is 6.95×10^{10} cm., it follows that each square centimeter of the sun radiates 6.23×10^{10} ergs per second. This *flux* of emergent radiant energy is not emitted in any particular wave length or color. The energy is actually emitted in all colors with a definite determinable distribution of intensity. This distribution of the emitted solar energy over the different colors has been determined with great precision by C. G. Abbot and his collaborators at the Smithsonian Institution. The experimentally derived distribution is illustrated in Figure 10 (curve 2). It is evident that we should be able to infer from this energy distribution something significant about the constitution of the solar atmosphere, particularly about the relative effectiveness with which the material composing the solar atmosphere absorbs light of various colors. For the radiation we receive at any partic-

ular wave length (or color) is the resultant of the radiation of this color emitted by the layers of the solar atmosphere at *all* depths: only the weight with which the deeper layers contribute to the emergent intensity will be progressively reduced on account of absorption by the intervening material. The effectiveness with which the deeper layers affect the emergent radiation in any particular color will therefore depend on the absorptive power of the solar material for radiation of this color. Thus, if the absorptive power of the solar material is less in blue-green than it is in yellow-orange, then in the blue-green region of the spectrum we should effectively see to greater depths in the solar atmosphere than we do in the yellow-orange region; consequently, the radiation in the blue-green region must be more characteristic of the deeper layers at a higher temperature than the radiation in the yellow-orange region, which will be more characteristic of the higher layers at a lower temperature. It is clear that in this manner we should be able to infer the relative absorptive power of the solar atmosphere for radiation of different colors. This was first done by E. A. Milne in 1922, and his results have been generally confirmed and extended by later investigators. These investigations show that the absorptive power of the solar atmosphere increases by a factor of the order of two as the wave length increases from 4,000 to 9,000A; beyond λ 9,000A the absorptive power decreases by about the same amount until we reach λ 16,000A in the infrared; and the indications are that, as we go further into the far infrared, the absorptive power again increases.

For many years one of the principal problems of astrophysics was to determine the source of continuous absorption in the solar atmosphere which will show the behavior we have described. All efforts in this direction failed until R. Wildt suggested in 1938 that we should perhaps look for the source of continuous absorption in the solar atmosphere in the presence of negative hydrogen ions in the atmosphere. The

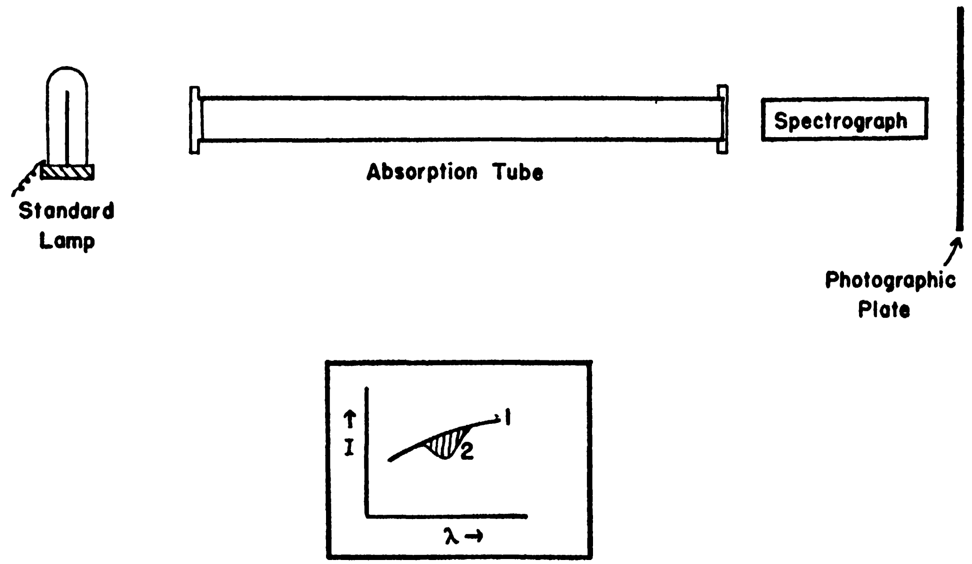

Fig. 8. The schematic experimental arrangement used in laboratory investigations for the spectroscopic identification of an unknown gas.

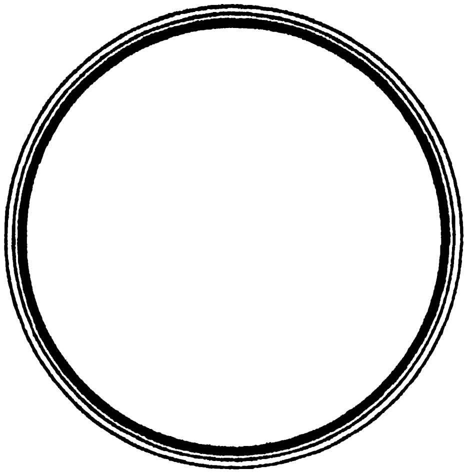

Fig. 9. A schematic drawing of the sun's photospheric layers.

grounds for the expectation were simply that in the solar atmosphere there is an abundance of neutral hydrogen atoms and there is also a supply of free electrons from easily ionized atoms such as sodium, calcium, magnesium, silicon, and the rest; and, in view of the positive electron affinity of hydrogen, some of the electrons will attach themselves to the neutral hydrogen atoms present. This suggestion of Wildt was taken up with great enthusiasm by other astrophysicists. But, before definite conclusions could be drawn, the absorptive power of negative hydrogen ions for radiation of different colors had to be derived, again from physical theory. The first determinations of the absorptive power of the negative hydrogen ion by Massey and Bates and others were disappointing. As will be seen from Figure 7, these early determinations were at complete variance with the astrophysical demands. However, it was soon realized that a reliable determination of the absorptive power of the negative hydrogen ion is an exceptionally difficult problem on account of its relatively very large size. The difficulties have now been overcome satisfactorily, and the new determinations are entirely in accord with the solar data. Indeed, we shall now show how, with the help of the newer determination of the absorptive power of the negative hydrogen ion, we can actually *see* the presences of these atoms in the solar atmosphere.

Let us recall how we normally identify any particular gaseous substance by its absorption spectrum in the laboratory (Fig. 8). First, we have a standard lamp which emits radiation in a known manner in all wave lengths (curve 1, Fig. 8). Next, we introduce the unknown gas in an absorption tube and we let the light from the standard lamp pass through the absorption tube and analyze the transmitted light spectroscopically. By comparing the intensity distribution in the transmitted light (curve 2, Fig. 8) with that emitted by the standard lamp (curve 1) we get the *absorption curve* of the gas. It is in terms of this *absorption* that the identification of the gas is made. Similarly, the radiation from the deeper layers of the sun passing through the outer (photospheric) layers gives rise to the observed emergent continuous spectrum of the sun (Fig. 9 and curve 2,

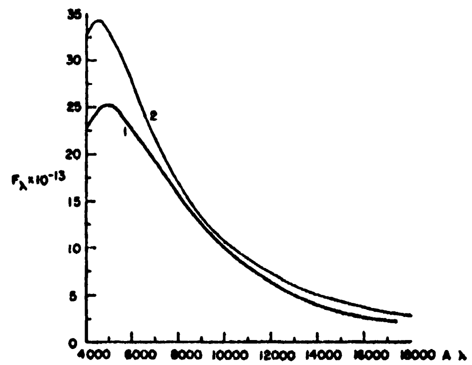

Fig. 10. The intensity distribution in the emergent continuous spectrum of the sun (curve 2) and the expected distribution if the absorptive power of its atmosphere is independent of color (curve 1).

Fig. 10). It would seem that the analogy with the laboratory experiment breaks down at this point since we do not seem to have the analogue of the standard lamp. However, we can construct a theoretical standard of reference in the following way: Suppose that the absorptive power of the solar atmosphere is the same in all colors; in other words, suppose it is *gray*. We can then predict in a relatively straightforward way what the intensity distribution in the emergent radiation should be. This is indicated by curve 1 in Figure 10. We use this as our standard of reference. The difference in curves 1 and 2 in Figure 10 must be due then to the *variation* of the absorptive power of the solar atmosphere with color. We may, if we like, call this difference the *solar absorption curve*. This is shown as a series of points in Figure 11. Now, we can compute what the solar absorption curve should be if the absorbing mechanism is provided by the negative hydrogen ions. This is shown as the full-line curve in Figure 11. It is seen that the agreement between the observed absorption curve and that to be expected from the negative hydrogen ion is very satisfactory; indeed, so satisfactory that we are justified in saying that this atom has been detected spectrophotometrically in the sun's atmosphere.

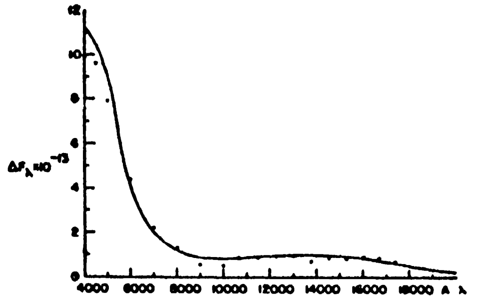

Fig. 11. A comparison of the observed "solar ab sorption curve"—the difference of curves 1 and 2 o Figure 10—and that to be expected on account o the presence of negative hydrogen ions in the solar atmosphere. The former is indicated by dots while the latter (derived from a recent investigation of Chandrasekhar and Münch) is shown by the full-line curve.

The story of the negative hydrogen ion we have just concluded has, in some respects, a plot which is strangely the reverse of that of helium. In the case of helium, the presence of a new element in the sun was first concluded on the strength of spectroscopic evidence, and it was only a quarter of a centry later that its terrestrial existence was established. In the case of the negative hydrogen ion, on the other hand, its stable existence was first established on theoretical grounds, and it was only some 15 years later that its presence in the solar atmosphere was concluded, on the strength of spectrophotometric evidence.

The stories of helium and negative hydrogen ion we have told have a moral. It is, however, the same as it was 50 years ago when Norman Lockyer, concluding the story of helium, wrote, "The more we can study the different branches of science in their relation to each other, the better for the progress of all sciences".

THE WEAPONS OF PANACEA

By H. B. VAN DYKE

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THE title of this article will mystify some readers. Panacea was a goddess whose name has fallen into serious disrepute owing to the common practice of making it synonymous with the impossible—a remedy for all diseases and, hence, a quack medicine. In the hope that I can contribute to the abatement of her unjust treatment, I wish briefly to give one version of her origin, to describe some of the methods by which her armory has been equipped, and to emphasize how carefully her fascinating new weapons must be used by her votaries. According to some accounts, Aesculapius, the god of medicine, had several daughters, of whom the most famous was the goddess of health, Hygeia. My title refers to a less well-recognized daughter, Panacea, the goddess of healing, whom we may regard as the patroness of therapeutics, just as Hygeia can be considered the patroness of preventive medicine. I cannot discuss all the varied weapons from which Panacea may choose if she is to be a healer of all ills. My chief interest is in the action of drugs, and I shall direct attention principally to these therapeutic weapons.

Attempts to cure or to prevent disease by plants or by animal tissues unquestionably were undertaken for ages before any records are known to us. Probably some guesses as to prehistoric methods can be furnished by the practices of primitive peoples today who rely on magical rites and peculiar remedies for recovery from disease. Moreover, as MacKinney has pointed out, even highly developed civilizations such as those of Egypt, Greece, and Rome were content to accept drugs prepared from plants, animals, or minerals although their therapeutic value was determined only by superstition and crude empiricism. Some remedies, not truly different from these, occasionally survive even today among isolated ignorant groups in countries, including our own, which regard themselves as most civilized. I can illustrate how superstition and irrational empiricism were practiced by quoting MacKinney's description of the manner in which the royal physicians treated King Charles II during his last illness:

> . . . a pint of blood was extracted from his right arm, and a half-pint from his left shoulder, followed by an emetic, two physics, and an enema comprising 15 substances; the royal head was then shaved and a blister raised; then a sneezing powder, more emetics and bleeding, soothing potions, a plaster of pitch and pigeon dung on his feet, potions containing 10 different substances, chiefly herbs, finally 40 drops of extract of human skull, and the application of bezoar stone; after which his majesty died.

If we are better off today, we owe this change to the scientific revolution of the past century and a half. It is true that important drugs were first discovered by primitive people or by careful appraisal of medicinal folklore. Cinchona bark containing quinine is an example of one, and digitalis is an example of the other. The use of cinchona bark enabled the seventeenth-century English physician Sydenham to distinguish malaria from other fevers, although its rational use had to follow Laveran's discovery of the malarial parasites in 1880. Another English physician, Withering, in 1775, identified digitalis, or foxglove, as the active therapeutic agent in the secret complex prescription of medicinal herbs of an old woman in Shropshire. Yet Withering used the drug for treating

dropsy (edema), and its rational use, still not completely understood as a remedy for the diseased and failing heart, has been a matter of gradual development since late in the nineteenth century. Thus we can conclude that the growth of all the sciences, occurring as it has largely since 1800, has determined the growth of scientific medicine which, in turn, alone has enabled us to employ drugs with an understanding of their action.

Until the middle of the nineteenth century, therapy by drugs could add little to the self-respect of the conscientious and critical physician. It is true that much earlier a beginning had been made in attempts to describe without equivocation the remedies which physicians use. At the end of the fifteenth century, the city of Florence introduced a *Nuovo Receptario*. As Urdang points out, this may be regarded as the first-known European ancestor of the modern pharmacopoeia in the sense that it contained standard descriptions of pharmaceutical preparations which had to be used by all physicians and pharmacists in the political unit in which it was promulgated. However, official or unofficial descriptions of the complex mixtures commonly prescribed, although a step forward, contributed little to therapy. A popular account in *Fortune* mentions that Oliver Wendell Holmes could proclaim in 1860 that nearly all the drugs then in use should be thrown "into the sea where it would be all the better for mankind, and all the worse for fishes." The successor of the ancient apothecary became the vendor of patent medicines who "put more time, money and imagination into the package than he ever put into the contents, which might be and often were thrown together out of the cheapest nondescript materials." Leaders like Cushny recognized that not a few respected drugs had no more therapeutic virtue than the nostrums and demanded scientific evidence to support their continued use. As a result, deletions from official lists were made at an accelerated pace. Only recently has recognition of the need to subject every important new remedy to rigorously controlled therapeutic tests begun to be general.

Before the nineteenth century was half completed, ether had been introduced as a general anesthetic, and the first laboratory of pharmacology formally dedicated to the scientific study of the action of drugs had been founded. As physics and chemistry flourished, a whole group of biological sciences was established, offering concepts and methods with which drugs could be scientifically evaluated. In this regard physiology, biochemistry, and bacteriology are of special importance. In their development and in their relation to clinical medicine, pharmacology and another newly founded biological science, pathology, have several attributes in common. The early growth of both followed and depended upon the founding and development of sister preclinical sciences, such as physiology and biochemistry for pharmacology, and anatomy and bacteriology for pathology. Both have close immediate ties with clinical medicine—pharmacology with therapeutics, and pathology with diagnosis. Although both are studied in the spirit of "pure" science, they are conscious of inescapable obligations to clinical medicine. Both sciences are interested in deviations from the normal caused in one case by drugs, and in the other by disease. In the pursuit of this interest, both sciences have repaid a measure of their continuing debt to sister sciences. For example, pharmacology not only has furnished chemical tools for the use of the physiologist or biochemist but also has itself discovered substances of great physiological importance.

Shortly after the first medicinal use of a synthetic organic chemical, ether, Perkin discovered that dyestuffs can be made from aniline (1856) and the foundation of the

great scientific expansion and industrial exploitation of organic chemistry was laid. However, only during the sixty years just past has it been realized what immense contributions synthetic organic chemistry can make to therapeutics. This new source of drugs is limited only by the scientific insight, the ingenuity, and the industry of the organic chemist and the pharmacologist. Within a century, many of Panacea's most powerful weapons unknown throughout history in terms of the traditional *materia medica* of drugs of plant, animal, and mineral origin have been devised and made in the laboratory. A parallel development has been the isolation and structural characterization of naturally occurring therapeutic agents such as alkaloids, glycosides, and hormones. Lastly, metals such as arsenic and antimony have been shown to confer unique and specific chemotherapeutic properties when incorporated into larger molecules by the organic chemist. Recently, new methods of radiation therapy have been made possible by the use of radioactive isotopes furnished by the physicist. Three of these, radioactive iodine, radioactive phosphorus, and radioactive sodium, are already on trial therapeutically. Also, it is apparent that tracer elements like heavy nitrogen (N^{15}) or radioactive carbon (C^{14}) can be incorporated into drugs so that these drugs or their degradation products can be specifically identified.

THE foregoing is largely historical prologue. It may be of interest to illustrate by a few modern examples how new drugs are discovered and the great care which must be exercised in demonstrating their value in human disease. However, we should first give brief consideration to a few principles and methods to be regarded as essential in the investigation and introduction of drugs. Were there not efforts to prevent animal experimentation, I would feel it a reflection on the reader's intelligence to mention that a new drug usually cannot be discovered or safely used in human disease without humane and properly controlled experiments on organisms ranging from bacteria to primates. In the final clinical trials, man is the experimental animal; under these circumstances control groups often can be set up only incompletely or with great difficulty. In contrast with the therapeutic lore preceding our more scientific era, analysis of the therapeutic effect of a drug is simplified but not made simple by using so far as possible single drugs of known composition. Obvious as this practice will appear, it is well to remember that it had little recognition for centuries. An extreme example of a complex prescription not worth the labor of evaluation is the antidote of Mattioli containing 230 ingredients. Specificity of action is a highly desirable attribute of a drug since it leads to a corresponding increase in precision of therapy and may immensely facilitate diagnosis. The number of drugs with high specificity is small, and we often describe unwanted or harmful actions as "side effects." Penicillins may be described as highly specific antibacterial drugs in the sense that toxic or undesirable effects are rarely associated with their proper use. On the other hand, all general anesthetics have limitations of one kind or another which reduce their specificity. The harm that drugs can do, even though often perceived early, may be subtle and lead to consequences which some physicians are loath to recognize. For example, the use of sulfonamides for trivial complaints may sensitize the patient so that the later use of a sulfonamide leads to serious toxic manifestations when the drug may be required for a serious bacterial infection. Inadequate dosing may be worse than no therapy if a strain of bacteria or parasites resistant to the drug survives and is allowed to propagate itself.

I mentioned earlier that the scientific study of drugs may lead to discoveries of great physiological interest or furnish chemical tools for the physiologist. Hunt and Taveau discovered the truly remarkable effect of acetylcholine in lowering the blood pressure following attempts to identify the substance in adrenal and brain extracts which had a depressor action. Now, years later, it is universally agreed that this substance plays an important part in the physiology of the nervous system. Alloxan is an example of a compound used as a physiological tool: it can specifically destroy the insulin-secreting cells of the pancreas and thus cause experimental *diabetes mellitus* without surgical intervention.

Also of great interest are unsuspected properties of drugs leading to new discoveries of real practical importance. If the MacKenzies and McCollum had not noticed the goiters in their animals receiving repeated doses of sulfonamides, the discovery by Astwood of the much more useful antithyroid drug, thiouracil, probably would not have been made. Dale described how a wrong hypothesis can also enable the alert investigator to forge ahead. The diguanidine, Synthalin, is a drug which causes the blood sugar to fall and was once thought to be a possible substitute for insulin. V. Jancso, knowing that trypanosomes utilize much sugar when living outside of the body, administered Synthalin to trypanosome-infected animals. Since the parasites disappeared, he concluded that lack of sugar in the blood was responsible for their death. However, Lourie and Yorke satisfied themselves that insulin in vitro does not have a significant lethal effect but that Synthalin can kill trypanosomes in very great dilution. King, Lourie, and Yorke were thus led to the discovery of other diamidines which are used for the treatment of African sleeping sickness and kala azar. Some drugs of extreme importance have been discovered by laborious systematic pharmacological examination of a host of organic compounds. Atabrine and the newer antimalarials typify this method of research, which requires the immense funds of large industrial establishments or government agencies.

Today we believe ourselves to be more critical of the claims made for new drugs than were our predecessors and we are attempting to set up machinery for clinical tests under scientific control. However, the uncritical still furnish material for a history of useless drugs which now appear to be more respectable because of a more or less accurate chemical description. The critical physician of a generation or more ago has been called a therapeutic nihilist. Like Trousseau, who remarked, "Always use the new drugs while they still have power to heal" (Brown), he could also be a therapeutic cynic. Brown also cites examples of the manner in which new drugs are found to be beneficial to the extent that the physician believes in their merit. During the period of medical enthusiasm, results can be highly successful; as doubts assail the therapist, only an occasional patient is benefited; finally, the physician is frankly skeptical, and the previously valuable drug becomes virtually useless. Suggestion can give temporary life to a valueless remedy. Only controlled critical repeated observations can insure a respectable longevity for a drug. A new drug developed in the laboratory must be supported by pharmacological and toxicological data; its early clinical acceptance likewise requires similar data from human trials. Some truly useful drugs, such as colchicine for the treatment of the acute pain of gout, lack what some would call "laboratory" support. However, the number of such remedies is unquestionably declining as the bulk of new drugs now flows from the laboratory.

On the basis of the production data of

insulin, Clark estimated that about a generation is required for the full utilization of a genuinely useful drug. Although the decade just ending is manifestly abnormal, it appears that the tempo of the rise, decline, and ultimate stabilization of an important therapeutic agent has been greatly accelerated. This can even furnish amusement to the laity when the cartoonist depicts a druggist holding a vial before a prospective customer and remarking, "It has been a wonder drug for over a week now." Under the spur of government-sponsored research and production, penicillins within a few years have displaced sulfonamides in both medical and lay favor, although sulfonamides first received favorable recognition only in 1935 and the peak of production in this country, 9,860,000 pounds, was reached in 1943. A chapter could also be written on the influence of popular articles concerning drugs on the therapeutic practices of the average physician. For example, it appears today that only a strong and honest physician can resist the patient's demand that penicillins be used for every ailment: he risks ostracism by his patients and foregoes easy profit to himself.

The modern physician has received an elaborate training in the basic preclinical medical sciences. On this foundation he builds during the remainder of his career in clinical medicine. With this background of training and experience, the alert clinician knows that if he is to use one of the many weapons of Panacea to prevent, alleviate, or cure disease, he must choose his weapon wisely and use it to its full advantage.

PLUTONIUM—NAMED FOR THE GOD

Out of the void, the fear, the terror's breath
He rose titanic from those regions dank
Where darkness reigns—darkness and grief and death
Pluto, grey guardian of the lifeless rank.
Yet men conceived him in another guise
His not alone the grim and bleak design,
Wearing an aspect gracious and benign.
And now at world's end, Man, the child, the fool,
Sudden in strength, cleaving the very sod,
Has forged himself a wonder-working tool
Plutonium—and named it for the God.
O Man, creator turned, lift up your voice
Abundant life or death—and yours the choice.

MARTHA E. MUNZER

HOW THE JUNIOR ACADEMIES OF SCIENCE OPERATE

By JOHN W. THOMSON, JR.
Department of Botany, The University of Wisconsin

EVERYWHERE today one reads of the part the young scientists must play in the world of tomorrow. The need for them is great and a place is assured for them, but what is being done to find them?

Back in 1919 a movement began in Illinois, flowered briefly, was almost submerged, and then in 1928 with great vigor began to show its real importance. Always depending upon volunteer enthusiasm and interest, the junior academy movement is rapidly becoming a force to be reckoned with in the production of young scientists. In the junior academies the senior membership is finding the wellsprings of continued growth and leadership.

When the Wisconsin Junior Academy was in process of being organized, information on the operation of junior academies was needed. In order to obtain up-to-date information, a questionnaire was sent to all Science Clubs of America Cooperators and junior academy sponsors. Through the generous help of many people, the information in this article is made available.

Probably the real start of the junior academy movement came with the reorganization in 1928 of the Illinois Junior Academy to include the participation of the high-school students in an academy meeting. All the active junior academies have since successfully followed this pattern of having at least one meeting a year with the senior academy. Fairly soon after the reorganization of the Illinois group, other junior academies were founded: Indiana and Kansas in 1930, Iowa in 1931, Kentucky and Pennsylvania in 1933, Alabama and West Virginia in 1934, Oklahoma and Texas in 1935, Missouri and St. Louis, Mo., in 1936, Minnesota in 1937, Florida in 1939, Virginia and Ohio in 1940, Tennessee in 1943, Wisconsin in 1944, and Georgia and Michigan in 1946. With this the roll cannot really be called complete, for the list should include organizations whose activities embrace the same field and philosophies as the junior academies. In North Carolina there is the High School Committee of the North Carolina Academy of Science; in Maine the Maine Principals' Association has sponsored a Statewide Science Fair and Congress; in New Jersey the Newark Museum sponsors a Science Congress; in Rochester, N. Y., the Junior Science Club Committee of the Rochester Academy of Science is making arrangements to sponsor opportunities for the science-minded youngsters of that area; in western Pennsylvania the *Pittsburgh Press* sponsors a year-round program for young scientists with a Science Fair; in Rhode Island the *Providence Journal* supports a similar Science Fair; and in New York the American Institute of the City of New York has sponsored Science Fairs at which projects were exhibited.

Two main sources of leadership have helped foster this rapid development. The Academies Conference of the American Association for the Advancement of Science under the leadership of Dr. Otis W. Caldwell has carried the burden of helping prospective and active junior academies since the Cleveland meeting of 1930. More recently, Science Clubs of America, organizing science clubs on a national basis, has been of additional great help to the junior academies through its "Cooperators" arrangement. By furnishing a central of-

fice through which exchanges of information and materials can be made, Science Clubs of America has been extremely helpful. Cooperators with this organization are kept in touch with the activities of the other junior academies by the preparation of digests of their communications and publications. Sets of the literature, publicity releases, "Things-of-Science" Kits, *Science News Letter*, and *Chemistry* are all sent to cooperators. Virginia and Tennessee have participated in state-wide Science Talent Searches coordinated with the Westinghouse Science Talent Search administered by Science Service and Science Clubs of America. As nearly as can be ascertained, all the active Junior Academies of Science have representatives appointed by the state academy to act as cooperators with this national organization.

Information on the early years of most of the junior academies is difficult to obtain. Records of few have been published. From such materials as were available the number of science clubs joining the junior academy in its first year of existence has been ascertained to be as follows: Indiana 8, Kansas 6, Iowa 13, Pennsylvania 14, Alabama 17, West Virginia 16, Oklahoma 16, Minnesota 7, and Wisconsin 15. Figures for the first year of operation of the other junior academies were unobtainable.

The boys and girls in the science clubs join the junior academies as club members rather than as individuals; that is, the clubs to which they belong join as a chapter of the junior academy, and all club members automatically become junior academy members. The most recent membership figures available, those for the 1945–46 school year, are shown in Table 1.

It is obvious, of course, that not all science clubs within a given state have become chapters of the junior academies. For example, in Wisconsin a careful survey of the high schools showed sixty-nine clubs to be active, when twenty-nine chapters were in the junior academy. The reasons for the incomplete membership vary from state to state; perhaps the newness of the movement in Wisconsin is one of the most important reasons there. Virginia and Florida very probably have enrolled almost all the active science clubs of their respective states. In the high schools the turnover of teacher personnel is great, and, as the sponsoring of science-club work is largely a reflection of the personal interests of the teachers, this bulks large as a factor in the continuation of science clubs in any given school. The same factor is also operative in junior academy membership. Active, enthusiastic sponsors may keep the club in the junior academy year after year.

One problem which has not yet been satisfactorily solved by all the junior academies is the problem of getting in touch with the lone science enthusiast in a school which is too small to boast of a science club. Steps have been taken by some of the state junior academies to remedy this situation. Both Virginia and Tennessee have initiated state wide-Science

TABLE 1

MEMBERSHIP IN THE JUNIOR ACADEMIES, 1945–46

	Chapters or Clubs	Student Members
Illinois	46	1,400
Indiana	40	1,200
Kansas	25	500
Iowa	?	?
Kentucky	28	675
Pennsylvania	23	500
Alabama	8	125
West Virginia	35	300
Oklahoma	10	300
Texas	14	?
Missouri	30	500
Missouri, St. Louis	?	?
Minnesota	12	120
Florida	124	2,000
Virginia	115	3,000
Ohio	0 (inactive)	0
Tennessee	40	400
Wisconsin	29	1,100
North Carolina	2	50

Talent Searches which run concurrently with the Westinghouse Science Talent Search. Based on state-wide interest, and with the awarding of scholarships to institutions within the state, these talent searches will materially aid in ferreting out the capable boys and girls within these states. The wide publicity given by the state organizations as well as by the national organization make for a greater likelihood of reaching the attention of the youngsters. The additional scholarships available also make the students and the teachers feel the effort of entering the contests to be more worth while.

THE financial support of the junior academies is exceedingly diverse. In Virginia, Tennessee, Iowa, and Kansas no dues are charged for membership, and the entire cost of operation is defrayed by the senior academy. Illinois charges 10 cents per student member, Indiana charges $1.00 for admission of the club to the junior academy and $1.00 per year dues for each club. In Kentucky the charge is 15 cents per student; in Oklahoma, 10 cents per student, with a minimum of $1.50 for the club, in Minnesota, 25 cents per student, plus $1.00 per school. When operating, Ohio charges were $1.00 for the adult sponsor and club, plus 15 cents for each student member. Pennsylvania and Florida charge $1.00 annual fee per club; Missouri charges $1.50 per chapter; Texas, Alabama, Wisconsin, and North Carolina charge an annual fee of $2.00 per club; and St. Louis, Mo., charges $5.00 per year per club.

Although these dues and fees help to support the junior academies, the senior academies in almost all states, realizing the worthiness of these projects, help to subsidize the junior academy work. In making this survey special attention was paid to the support and operation of the junior academies. Practically every junior academy presents a different situation. Thanks to the candor with which the questionnaire was answered, the following summary of support may be presented.

In Illinois, where the cost of operation has averaged about $150.00, the support comes about one-third from the dues and two-thirds from the senior academy and the High School Principals' Association; some additional support is obtained from commercial sources. This last consists of books and equipment and cash prizes to be awarded at the annual meeting.

In Indiana no figure was given for the annual cost of operation, but the cost of stationery, postage, etc., is assumed by the office of the state sponsor, Dr. Howard H. Michaud, Purdue University. When *Science Aids Service*, a now defunct joint publication of the Illinois and Indiana Junior Academies was being issued, the Indiana Senior Academy contributed toward its cost.

In Kansas and Iowa the annual cost of operation was placed at $100.00, and the entire support was given by the senior academy. Kentucky presents a more complicated situation. The cost of operation runs to about $275.00, with the support coming one-half from dues, one-seventh from the senior academy, and the rest from the Kentucky Ornithological Society and special financial contributions from affiliated clubs and from friends.

In Pennsylvania the annual cost runs to about $20.00, half coming from dues and half contributed by the senior academy. Printing is done free by high-school students so that this cost does not appear in the budget. In Alabama $40.00 comes from dues and $20.00 from the senior academy. In West Virginia the junior academy was in the process of reorganization so that no figures were available on these items.

The Oklahoma budget runs to about $50.00, with about 95 percent coming

from the dues and occasional help from the senior academy and scientific apparatus firms. In Texas the budget of $25.00 to $35.00 and in Missouri the cost of $50.00 are met by grants from the senior society. The annual cost reported from Minnesota was $25.00 to $30.00, with most income coming from dues, augmented by a grant of $10.00 from the senior academy. In Ohio the situation reported was a cost varying from $15.00 to $50.00, with the support coming entirely from the junior academy dues. Tennessee reported a budget of $12.00, with the support entirely from the senior group. The cost has probably risen in this state since the beginning of their Science Talent Search.

Wisconsin presents a unique cooperative situation. The state academy sponsors the junior academy and pays for part of the operational expenses, printing and the cost of the annual dinner for the state-wide district meeting winners being the major items in the academy budget for junior academy support. The University of Wisconsin, as part of its program of greater service to the people throughout the state, makes available the time of one of its staff members. The Chairman of the Junior Academy Committee is released from teaching duties in the Department of Botany in order to devote this time to the junior academy work. Office expenses, postage, and travel have also been assumed by the university in supporting the junior academy. The total cost is thus greater than for the other junior academies, especially if the time of the state sponsor is taken into account. Apart from this, the cost has run to about $350.00 to $400.00 per year during the years of the founding of this junior academy. Additional support has been given by a friend of the academy and by the local chapter of Gamma Alpha for prizes at the annual meeting.

North Carolina reported a budget of about $150.00, with $50.00 being contributed by the senior academy, $2.00 coming from dues, and the balance contributed by the State Department of Public Instruction, State Forestry Association, the North Carolina Education Association, Bird Clubs, and the Carolina Biological Supply Company.

It should of course be realized that the budgetary figures do not show the entire cost of operation in most of the states. The desire to further the careers of the youthful scientists has led many people to contribute their time and efforts. It is this enthusiasm and interest which have not only kept the junior academy movement going but also expanding.

The meetings of the junior academies are more uniform than the finances or the government. All have at least one meeting a year with the senior academy. This contact with the sponsoring group is invaluable. The senior members are always much impressed by the enthusiasm and the excellent presentations made by the junior members, and the latter are delighted to meet the men of science of their state. Those states which have sectional as well as state-wide meetings are Pennsylvania, Oklahoma, Ohio, and Wisconsin. In this last state there are two district meetings, one in the central part of the state and one in the eastern. Eight demonstrations are chosen from these district meetings to be presented at the state-wide meeting with the senior academy. In Oklahoma there are spring and fall meetings, the latter with the senior academy.

The most important feature of junior academy meetings is the opportunity given the boys and girls to present the results of their own researches. To them the field of science is new and full of interesting material. The professional scientist might have expected that the opening of the atomic age would lead to a preponderance of papers and demonstrations on such

things as atomic power, radioactivity, and rocket propulsion. Examine the programs of any of the junior academy meetings last year—the healthy spread of interests is reassuring. The range of interests can be shown by the topics chosen by speakers on the program in Maine: The Scientific Alarm Clock, The Manometric Flame, Wheels—Humble and Highbrow, Comparative Study of the Digestive System, Fingerprinting, Widal Test, Purification of the Bangor Water Supply, Testing Milk for Butterfat Content, Solubility and Specific Gravity of Gases, A Diving Helmet, A Homemade Six-inch Telescope and the Stars It Will Bring Closer, The Tesla Coil, Effect of Various Light Rays on Plants, Reproduction in Plants, Evolution in the Brain of Vertebrates, The Frasch Process for Mining Sulphur, Glass Etching, The Preparation of Sulphuric Acid, The Slide Rule, "B" Battery Elimination, Seeing Eye, Development of the Chick, Homemade Record-Changer, Diorama on Conservation, Diorama on Shore Birds, Working Model of Panama Canal Locks, Arteries of the Head, Unusual Properties of Minerals, and Uranium Minerals.

The eight papers of the state-wide meeting of the Wisconsin Junior Academy may also be taken as a sample. The topics ranged from Back-yard Insect Collecting, through The Importance of Soil Analysis, Cold Light, Applications of Atomic Energy, Blood Will Tell, Astronomy Hobby, and Hydroponics to Lift and Drag Coefficients of Airfoil Sections.

The gamut of the sciences is covered by the youngsters, and the depth of understanding of their topics sometimes startles the older members who stop in to visit the young academy members at their meetings. Naturally, with all the *élan* of youth they have no hesitation in presenting demonstrations covering tremendous fields in fifteen minutes. But how much more lively and interesting a session with these young people is than many a more academic discussion that we have all attended!

Besides these student demonstrations and papers, exhibits of projects prepared by the members are universal at the junior academy congresses. A frequent addition to the program is a speech by some distinguished state scientist. Usually this is limited to an address of welcome or to a speech at a banquet, but North Carolina is an exception. There the projects of students are exhibited and the only talks given are those by professional scientists. Most of the junior academies add motion pictures on science to their programs, only Kansas, Ohio, Wisconsin, and North Carolina omitting this phase of the annual program. Some few add field trips to the program. Trips to visit the campus of the college at which the meeting was held, to museums, industrial plants, or other places of interest were mentioned by Illinois, Pennsylvania, Alabama, West Virginia, Oklahoma, Missouri, Minnesota, Florida, Virginia, Ohio, and Tennessee as being at least occasional features of their annual meetings.

Although the stimulus of attending the meetings is usually considered a sufficient incentive among the junior academies, cash prizes, certificates, pins, and other awards are given to participants in many of the states. Indiana, Missouri, Pennsylvania, and Tennessee deem the participation in the meeting enough of an incentive toward good work. These do offer membership in the A.A.A.S. as an award; more will be said of this later. In Illinois and Iowa certificates are given for excellent work. In Kansas pins, plaques, science books, ribbons, loving cups, and a $10.00 prize to the best club are all awarded. In Kentucky awards of $10.00 to two demonstrations and $5.00 to four demonstrations are given. Ad-

ditional awards include four magazine subscriptions, pins, and ribbons. Alabama awards four cash prizes of $5.00 and four certificates of excellence. In West Virginia awards of both cash and certificates are made, but the amount was not specified. In Oklahoma cash, certificates, plaques, and academy pins are all awarded. In Texas an award is made by an apparatus company, and a gold pin is presented. The Dwight Institute Human Genetics project in Minnesota awards a $25.00 prize and one or more $10.00 prizes for family history information. In Florida awards are made of $10.00 for the best club exhibit, $5.00 for the best student exhibit, and of books for essay winners. Virginia awards are especially generous. There is a $50.00 award to a club, the presentation of honorable mention certificates, and fifteen boys and girls win a trip to the state capital in the state Science Talent Search. In Ohio certificates and keys are presented. In Wisconsin a $25.00 war bond, a $7.00 prize, and an award by Gamma Alpha constituted the prizes offered in addition to honorary memberships at the 1946 meeting. The North Carolina awards are varied: for the best biological collections, awards of $20.00, $15.00, and $10.00 are made; for the best general exhibits, awards of $20.00, $10.00, and $5.00; for the best forestry essay, awards of $20.00, $10.00, and $5.00; and for an ornithological essay, $20.00 and two books on birds are awarded.

Much more important as incentives of permanent value are the awards of honorary membership. Possibly not all members of the American Association for the Advancement of Science are familiar with the fact that two honorary annual memberships in the A.A.A.S., one to a boy and one to a girl, are available per state junior academy. These memberships entitle the recipient to *Science News Letter* for a year as well as other privileges. Not all the junior academies have taken advantage of this award. Illinois, Indiana, Kansas, Kentucky, Pennsylvania, West Virginia, Oklahoma, Texas, Missouri, Minnesota, Virginia, Tennessee, Wisconsin, and North Carolina, a total of fourteen, have availed themselves of this opportunity. In Wisconsin the award is limited to students in their junior year. This is expected to make the award more effective in the high school instead of having the benefits accrue when the recipient has left. Only three states have offered honorary annual membership in the state academy to promising young high-school students: Pennsylvania; Virginia (four memberships); and Wisconsin (six memberships). It would certainly seem that more of the junior academies should take advantage of the A.A.A.S. memberships and that more of the state academies should offer memberships as an incentive.

Very few junior academies reported use of the radio as a vehicle for their scientific talent. In Illinois the state station, WILL, formerly carried a weekly junior academy program. Iowa reported that a weekly broadcast has been maintained for a year. Indiana, Pennsylvania, Missouri, and Wisconsin reported occasional broadcasts on junior academy work. There is much opportunity for expansion of this type of junior academy activity. Few of those queried reported that speakers for the club programs were available. Indiana, Iowa, and North Carolina reported that speakers were available for a nominal fee or for travel expenses. Free speakers were available in Kentucky, Oklahoma, and Wisconsin. As an encouragement to the young people this is one service which every state academy should make available. No club should have a program made up entirely of outside speakers; the burden of the club programs as well as its management should fall upon its members if it is to be really successful in its purpose.

But to be able to obtain an occasional speaker, just to know that the older academy members were interested in them, would give most clubs a considerable measure of encouragement.

A few junior academies can also offer other services to chapters. In Indiana two motion-picture reels on lenses and three exhibit kits on venom, neoprene, and plastics are available. Oklahoma offers a loan set of pamphlets on organization and operation of science clubs. In Texas clubs are given a subscription to *Texas Game and Fish*. Two project kits can be borrowed in Missouri. Motion pictures showing one of the meetings are available in Ohio. Wisconsin offers loan sets of lantern slides in biology, and North Carolina offers the loan of six sets of slides and a projector. More traveling exhibits could certainly be devised to circulate among the member clubs.

Another field of stimulation of endeavor now being explored by some of the junior academies is the possibility of some of the youths being invited to help professional scientists in "co-projects" and thus initiate them into really scientific work. The Virginia clubs have two such opportunities, a project on the distribution and economic importance of Virginia plants in cooperation with the Virginia Academy of Science Committee on Virginia Flora, and a second project on minerals in cooperation with the State Geologist. Other projects were noted as being planned for that state if a demand developed for them. The Pennsylvania Academy of Science sponsors a Wildflower Phenology project in which cooperation on a state-wide study of the blossoming and fruiting dates of flowers is obtained. The Minnesota Junior Academy of Science has available a remarkably fine list of sixty-two projects in which the junior members could make a contribution to science. This unusual list of suggestions was made with the help of many members of the state academy and is the kind of work which will help to develop the superior student of science. The list covers problems in the field of biology and conservation; no comparable series for the physical sciences has been seen. In Wisconsin the junior members may cooperate with a committee of the state academy on a study of the distribution of the native trees of the state. Another project is on the phenology of wild flowers, and a cooperative project with the U. S. Soil Conservation Service local area office is available.

For all the science clubs, whether a junior academy is operative in a state or not, there are a number of coprojects which Science Clubs of America sponsors with federal and national agencies. Thirteen co-projects were available during the past two years.

Most of the junior academies have reported some sort of mimeographed publication which serves as a medium for announcements and exchanges of ideas for club and individual programs and projects and which keeps the clubs informed of one another's activities. Such publications are irregular in issue, but usually several numbers appear during a year. In some states the responsibility for each issue is taken by a different club. Such is the case in Alabama, Texas, and Wisconsin. Articles on science subjects, poetry, news items, announcements of meetings, and other notes of interest to the member clubs appear in these bulletins. Formerly the Illinois and Indiana junior academies issued a printed publication, *Science Aids Service*, in which news of not only the clubs of these states but of other junior academies appeared. This publication appears to have been discontinued. The only junior academy now issuing a printed publication with science articles and other contributions by science-club members is the Kentucky Junior Academy of Science. They sponsor an

excellent eight-page bulletin issued five to six times a year. This paper is a model for the other junior academies. Most of the contributions are by student members, and the encouragement given by seeing their articles and names in print is no doubt invaluable. Another publication which should be mentioned in connection with junior academy work is *The Science Teacher*, published quarterly at 201 North School Street, Normal, Ill. A special section of this magazine is devoted to science clubs and junior academy activities, with many of the articles prepared by the superior science students. A very interesting publication is the *Yearbook of the Illinois Junior Academy of Science* issued during 1945. All the clubs were given the opportunity to contribute articles to this publication. The eighteen projects upon which they worked in various fields of science are described in some detail by the boys and girls. Many photographs and line drawings illustrate the bulletin and add to its attractiveness. This is another idea worth exploiting in the encouragement of the youths in other states.

Because the junior academies are so much the outgrowth and reflection of the personal interests of the sponsors, the government of each is highly individual. It is difficult to make any generalization as the management depends upon the history and development of each junior academy. The most effective of the junior academies are those in which an individual is responsible and is supported by an ardently interested committee. The inclusion of science-club teachers as members of that committee is most desirable. The different types of junior academy governments are briefly outlined below.

In Illinois the junior academy is supervised by a Junior Academy Representative. He nominates teacher helpers who are then elected by the state academy. These teachers include the chairman of exhibits and four area representatives. A different state sponsor may be selected each year.

The Indiana Academy of Science appoints an eight-member committee, with the chairman, Dr. H. H. Michaud, serving as the state sponsor of the junior academy. The immediate governing body, however, is a group of five outstanding club sponsors elected by the club sponsors at the annual meeting to serve for five years, with one being elected each year and not being eligible to succeed himself within one year.

In Kansas the president of the senior academy appoints a committee composed of a chairman with a three-year term and five other members representing different parts of the state. The chairman is also a member of the senior academy executive board.

In Iowa the High School Relations Committee sponsors the junior academy. Two high-school teachers called Senior and Junior Counsellors are recommended by the junior academy and elected by the Executive Committee of the Senior Academy. At the annual meeting, high-school students are elected as President, Vice-President, and Secretary-Treasurer. These officers, plus the secretary of the senior academy and the Chairman of the High School Relations Committee, comprise the Executive Committee of the Junior Academy.

The Kentucky Junior Academy is controlled by an Executive Board of the Junior Academy, which is made up of the four officers. An Executive Committee appointed by the senior academy is a standing committee.

The fifteen-member Committee on School Service of the senior academy controls the Pennsylvania Junior Academy, but there also appears to be a nine-member Advisory Committee of high-school teachers from different parts of the state which operates the junior academy.

In Alabama a Junior Academy Committee of Counsellors of three members is appointed by the senior academy Executive Committee to serve three years each. The chairmanship is rotated, and the comment was made that a permanent chairmanship is needed. West Virginia was in process of reorganizing its committee so that information beyond the fact that the senior academy appoints the committee was lacking.

In Oklahoma the junior academy is organized as an independent organization from the senior academy. The chairman is elected by vote at the annual meeting of the senior academy, however, and there is a High School Relations Committee of the senior academy. Actual management of the junior academy seems to depend upon a group of high-school teachers interested in the work.

In Texas the junior academy is governed by a committee consisting of the Secretary of the senior academy, a general chairman appointed by the Executive Council of the Senior Academy and seven high-school teachers engaged in school club work and representing various areas of the state.

In Missouri the Executive Committee of the senior academy appoints a chairman to be in charge of the junior academy activities. He in turn nominates a committee of eight which is approved by the Executive Committee.

In Minnesota the President of the junior academy is a science teacher elected by the junior academy Council; the Secretary-Treasurer is also an adult chosen in the same manner. The Vice-President is a high-school student elected at the annual meeting. The President is also a member of the Council of the senior academy. The junior Council is composed of these three officers, plus four student members elected by the junior academy, four adults elected by the junior academy for a term of two years, and the retiring President and Vice-President of the senior academy.

In Florida government is by a committee of three. The senior academy Executive Council appoints a Chairman, the juniors elect a second adult, and these two select a third committee member, who usually resides in the city where the next annual meeting is to be held and who chooses his own committee on arrangements.

In Virginia there is an Advisory Committee of four and a Science Club Committee of fourteen members. The chairman of the Science Club Committee is appointed by the President of the senior academy. In turn, the chairman appoints the other committee members, six adults and seven students. A Governing Council of the junior academy consists of the President, Vice-President, and Secretary of the junior academy elected by the delegates of the chapters, the Chairman of the Science Club Committee, and two other members of that committee. In addition, the sponsor of the host club for the next academy meeting also serves on the Governing Council.

In Ohio there is a governing council composed of five members representing different areas in the state and four advisors representing the senior academy. One council member is elected at the state meeting each year.

In Tennessee the Executive Committee of the senior academy appoints a chairman who in turn appoints six additional committee members.

Because the junior academy in Wisconsin is sponsored both by the senior academy and The University of Wisconsin, the situation as to government is a little more complex than in the other states. It has proved a workable arrangement. The university has released the Chairman of the Junior Academy Committee from his teaching duties in the Department of Botany in order that he may devote his

time to the junior academy affairs; so that, in a sense, both the university and the senior academy Executive Council appoint the chairman. A committee to advise the over-all activities of the junior academy is appointed jointly by the university and the senior academy. In addition to this general supervisory committee, there is a second committee composed of high-school teachers active in science-club work. These represent different areas of the state, are nominated by the Chairman of the Junior Academy Committee, and are appointed by the senior academy Executive Council. Except for the close cooperation between the university and the senior academy, the situation as far as government is concerned is quite similar to that in Indiana. The university in addition to contributing the time of one of its staff members also provides considerable financial and other support in Wisconsin.

In North Carolina there is a High School Relations Committee composed of a chairman and six other members representing colleges, high schools, and the State Department of Public Instruction.

No information has yet been obtained of the government of the two new junior academies, Georgia and Michigan.

The thread of similarity which seems to underlie all these different responses to local environments seems to be a small committee of working high-school teachers and a chairman appointed by the senior academy to be on the active group and also on the advisory committee of the senior academy, the second committee. Several of the sponsors who returned questionnaires voluntarily commented upon the desirability of at least a more or less permanent chairmanship to insure a definite and continued policy.

What is expected to accrue from these activities? Obviously the program is designed to appeal to the superior, not the mediocre, students and to guide them along the pathways of science. Perhaps the Junior Academy of Science movement is at least part of the answer to a letter written to the Office of Scientific Research and Development by the late President Roosevelt when he asked:

> Can an effective program be proposed for discovering and developing scientific talent in American youth so that the continuing future of scientific research in this country may be assured on a level comparable to what has been done during the war?
>
> New frontiers of the mind are before us, and if they are pioneered with the same vision, boldness, and drive with which we waged this war we can create a fuller and more fruitful employment and a fuller and more fruitful life (*Science*, December 15, 1944).

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WHY TERMITES?[1]

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ANTHROPOCENTRIC philosophers once assumed that all the creatures of the earth were placed here by the Creator for man's good or, if annoying or pestiferous, were the product of the Devil. This medieval concept still crops up in casual conversations. My fifteen-year-old nephew once asked me, "What good are mosquitoes?" In my less sophisticated moments, I give some credence to the hypothesis that mosquitoes were invented by the Devil. However, I do not put termites in the same category. I think termites are good—good for the biologist to study. This conviction sometimes shocks my friends, who seem to think that all my time should be devoted to eradicating these pests. If someone seeks my advice on how to exterminate termites from his house, my usual remark is, "Well, in the first place, you know that I am in favor of the termites."

I am not going to discuss the harmful effects of termites. In addition to the great damage to buildings, I have even heard of a case in which two chess players had to rush through their game because one of the bishops became infested with termites. Severe as the havoc is, I shall concentrate your attention on the more fundamental scientific values to be derived from the study of these insects.

Insects are too far removed from man—taxonomically, I mean—to draw forth much affectionate response or paternal solicitude. At least in my own case such affection has been relegated to the subconscious. A psychoanalyst friend once told me that biological inquiry is sublimated sexual curiosity, and that insects in some way are psychic symbols of children, possibly because there are too many of the little pests. Be that as it may, this is not the time to elaborate upon the individual psychology of either my entomological colleagues or myself.

Speaking of psychiatry, I was once telling a friend about the reactions of various peoples over the earth to my termite-collecting activities. Farmers and ranchers in the United States were worried; West Indian negroes thought I was crazy; South American Indians giggled and whispered among themselves; the Arabs of North Africa doubted by sanity; and the Italians discussed incarceration. At this point in my story, my "friend" remarked, "Well, a million people can't be wrong."

Enough of the reactions of the layman! Zoologists, I am sure, are able to appreciate the pearls cast before them, so I shall discourse upon why termites are good for scientific investigation.

The individual researcher is likely to approach biological problems along several paths. He may apply a certain technique to a variety of phenomena; he may choose a single principle and explore its manifestations in a variety of organisms; or he may choose a single organism or a restricted taxonomic group and attempt to study various correlated principles through the application of a variety of techniques.

I doubt if any approach is broader, narrower, or more productive than another. All have led to important results in the hands of skilled scientists. Occasionally a single investigator will apply one technique to one phenomenon in one organism. Rarely, a single individual may be able to

[1] From the address of the retiring Vice-president (Chairman of Section F), American Association for the Advancement of Science, Boston, December 29, 1946.

approach several problems with more than one method. Both depth and breadth have their values. Both analysis and synthesis are important aspects of science.

Termites are an order of insects with close to 1,800 described species, and many more await discovery. Through the early inspiration of a number of teachers and friends[2] I have found myself interested in all aspects of termite life. From among the many important principles exhibited by termite biology, I have chosen to place particular emphasis upon a phenomenon that is especially well illustrated by such social insects, namely, the integration of a population. Population integration is the result of the operation of many complex factors—factors that also produce other effects in other organisms—so that any understanding of termite society enables us to better comprehend other aspects of life and vice versa.

The adaptive evolution of the sterile castes of the social insects seemed to Darwin to be fatal to his theory of natural selection until it occurred to him that selection must operate upon the "family" as a unit. He subsequently found that the social insects provided some of the best examples of the influence of natural selection, at the same time affording an almost perfect proof that adaptive evolution could occur in the absence of any Lamarckian inheritance of acquired characters.

In the post-Darwinian decades, we may say that the accumulation of additional facts concerning the social insects has verified some of the essential aspects of the theory of natural selection of population groups and has assisted in the removal of some fallacious theories from modern biology.

[2] These include K. P. Schmidt, J. H. Comstock, Anna B. Comstock, William Beebe, J. G. Needham, O. A. Johannsen, J. C. Bradley, N. Banks, T. E. Snyder, W. M. Wheeler, F. E. Lutz, J. Bequaert, H. Lang, H. D. Fish, F. Silvestri, N. Holmgren, Y. Sjöstedt, N. A. Kemner, and S. F. Light.

A list of more modern generalizations may serve to focus our thoughts and give a base for discussion. Some of the following conclusions are considered invalid by leading contemporary biologists. By means of scientific controversy new facts and problems are discovered, new tests are applied, new lines of orderability become apparent, new applications are attempted, and both fundamental and applied science advances. Theodore Roosevelt once said, "Each scientist views the work of his colleagues with quarrelsome interest." I expect that this creative aspect of controversy will continue and that each fact and postulate will be critically scrutinized.

From the study of termites, a few generalizations may be made:

1. Population systems must be added to the several basic biological entities.
2. Population systems may be divided into two fundamental types, the species population with genetic continuity and the interspecies community with ecological continuity.
3. Populations have organismic attributes and may be appropriately called supraorganisms.
4. The more inclusive units incorporate the lower units together with their immediate environments, both physical and biotic. By this means, there is an organic evolution of the physical as well as the biotic environment.
5. The common characteristics of organisms and population systems are the result of natural selection of each unit through its internal and external relations.
6. Increasing stabilization and controlled periodicity of the internal and external environment (homeostasis) is a trend in the ontogeny and phylogeny of organisms and supraorganisms.
7. Significant parallels between insect and human societies have a common causation.

In order to clearly substantiate these statements, I should like to offer a clean-cut fact logically associated with each point. However, each series of related facts verifies several conclusions, so I am not attempting to adhere to the stated order of my conclusions in assembling the factual basis for these contentions.

In the first place, let us consider a most remarkable series of African termite nests described by Desneux, Stumper, and Sjöstedt. In my opinion, the nests I am about to describe afford the best example of the evolution of behavior yet discovered by biologists. Each of the nests is built by worker termites belonging to a colony of a species of the genus *Apicotermes*. Each is about the size of a basketball and is built in an excavated and lined subterranean chamber. Every nest possesses numerous tiny ventilation pores running through the wall. Three types of nests are known, constructed by five species of *Apicotermes*, and these may be arranged in what appears to be an evolutionary sequence. In the simplest type rows of uniformly spaced pimple-like projections occur on the outside surface. An arched pore extends from each projection through the nest wall into an interior chamber inhabited by the termites. The wall is probably formed from the claylike excrement of the termites, and the pores are molded as the wall is constructed by the cooperative activities of hundreds of workers.

The uniform repetition of these pores in the nest wall may be compared with the replication of structures in an organism. The best parallel is that of the pores in the wall of a sponge of the ascon type. Both the individual sponge and the termite nest are radially symmetrical with a geometric repetition of structures. One is a multicellular organism, whereas the other is the product of the behavior of a population of organisms. Obviously, the comparable similarity is pure analogy. From the functional viewpoint, however, both the organismic and social levels have evolved toward a more optimal internal balance of otherwise more variable environmental factors. This internal physicochemical constancy is called homeostasis by the physiologists (Cannon), and the same term applies to the relative constancy of the environment within the nest attained through social cooperation.

A second type of *Apicotermes* nest constructed by the workers of a different species possesses a series of enlarged openings on the outside of the wall, each leading into a tubular chamber before narrowing into the small pore homologous with the more primitive simple pore of the first type of nest.

A third type of *Apicotermes* nest, characteristic, with certain minor variations, of three different species, has circular galleries inside the walls that would seem to be modifications of the tubular chambers of the second type. Small outside holes lead into these circular galleries, and alternately arranged pores run from the circular galleries into the interior termite-inhabited chambers.

I am not sure that a word description of this series of termite nests can convey the precision of the geometric pattern, the modifications of homologous structures, and the obvious adaptive function of the architectural behavior of the termite workers. The evidence is clear, however, that we may apply certain concepts to the social behavior of an insect population that have heretofore been almost exclusively applied to the ontogeny and phylogeny of individual organisms. Such concepts as homology, polyisomerism, symmetry, and adaptive evolution, together with comparable basic physiologic and genetic induction mechanisms, may be exactly studied by the comparative social psychologists as well as by the comparative anatomist. The correlation between the psychic and the morphologic makes a dualistic philosophy increasingly untenable to biologists.

Nests constructed by other termites indicate that social behavior may exhibit regeneration, convergent evolution, and regressive evolution. Unrelated termites belonging to separate subfamilies construct similar chevron-shaped, rain-deflect-

ing ridges on the tree trunk above the nest. The similarity of the architecture is surely the result of convergent evolution in these instances, and the behavior is analogous. Hingston removed the rain-deflecting ridges from one nest of this type and witnessed the regeneration of the architectural pattern. Regression of the nest-building instinct is illustrated by a species belonging to a typically nest-building genus. In this particular species, which always lives on the stored food in a nest constructed by a very different termite, the ancient nest-building behavior has degenerated.

With such parallels between organisms and insect societies, if such terms as "supra-organism" or "epiorganism" (Gerard) are not used for the termite colony with its differentiated, sterile, "somatic" castes, then a new term must be invented to express the striking analogues common to the individual organism and the insect society.

Division of labor and structural orderability indicates antecedent operational physiological factors. What integrative mechanisms have been discovered that coordinate the insect society? Particularly through the recent work of Light and his associates, it is well established that the presence of mature males, females, or soldiers, respectively, inhibits the development of the same caste from undifferentiated nymphs. The best theory to account for the experimental results is that each of these castes gives off an exudate or "exohormone" that is passed through the colony by trophyllactic contact, thus inhibiting the development of either reproductives or soldiers unless the population increases beyond the minimum threshold amount of the inhibiting "exohormone." Theoretically, the worker does not inhibit the development of workers, but inhibition by reproductives and soldiers results in the development of workers in colonies of the higher termites in which this caste is present. Thus periodicities in the production of mature reproductive individuals in the colony of social insects has certain similarities to the hormonal mechanism periodically controlling the production of mature reproductive cells in the vertebrate organism. We have barely begun the study of social endocrinology, but we have some indication that the analysis of the social system may progress by applying methods similar to those that have proved effective in the analysis of the organism and its sexual adjustments.

Although the social insects are not suited to the experiments of classical genetics, they do indicate the existence of certain fundamental genetic mechanisms. For instance, it has been assumed by some that regressive evolution is the result of losses of the genes initiating the development of the character undergoing phylogenetic degeneration. Among the different genera of termites phylogenetic regression of the eyes of the sterile soldier caste is clearly indicated. Primitive soldier termites may have fairly large functional, pigmented, compound eyes, while an evolutionary reduction of pigment and of number and organization of the ommatidia is evident, ending in the total absence of the eye of the soldier in the most advanced termites. In this sequence of soldiers with reduced eyes, we have complete proof that the entire gene pattern for the development of functional eyes is present. The winged parents of each soldier possess functional compound eyes, and there is no genetic difference between the reproductive and soldier castes.

Modern physiological geneticists can cite organismic parallels to such a societal sequence. Sewall Wright has postulated that the abdominal appendages of an arthropod may undergo regressive evolution while the thoracic legs are progressively evolving, and yet there is no gene difference between the cells of the thorax and those of the abdomen of the same individual. The gene pattern influencing the development

of an organ under certain physiological conditions may have strikingly different effects under other physiological conditions. The basic gene pattern, once selected through the efficient functioning of an organ, may be intact even though no trace of the organ can be detected. The physiological conditions are themselves dependent upon hereditary factors in large measure and are undergoing adaptive evolution. The point that is illustrated by the phylogenetic regression of the eyes of termite soldiers is that the social insect population shows a striking parallelism to the individual organism and that an analysis of the biological mechanisms of one system is a step toward an understanding of the other.

Although it is not possible in so short a discussion to describe the series of integrated populations leading up from the species of asexual organisms to the highly social groups, it is increasingly clear from the experiments of Allee, Tinbergen, and their associates that numerous physiological and behavior mechanisms give rise to various levels of group coordination and that the social insects merely offer an extreme example of a population system. Such experimental work as that of Thomas Park on grain beetles gives us further precise information concerning population interactions. The species with its population parts is a true biological category taking rank in importance with the concepts of the cell and of the organism. As Allee and others have emphasized, cooperative interaction between the parts at all levels of integration is a fundamental characteristic of life.

There has been much opposition to the recognition of the reality of intraspecies population systems by biologists whose vision stops at the cell membrane or the skin of the organism. Even more opposition is to be expected if the concept of the supraorganism is applied to the interspecies biocoenose or ecological community. And yet much evidence accumulating shows that such properties of the organism as division of labor, physiologic and behavior integration, symmetry, dynamic equilibrium, homeostasis, ontogeny, and phylogeny are likewise characteristic of interspecies populations.

I shall cite only a single example to substantiate this point. Especially through the work of Cleveland, Hungate, and others, the interdependence of intestinal protozoans with certain wood-eating roaches and termites has been established. These flagellate protozoans transform the cellulose by means of enzymes, and the host insects are then able to absorb the resulting nutritive products. In the case of the roach the hind-gut is particularly highly adapted to the flagellate population. Chitinous valves prevent the flagellates from passing into the mid-gut with the nutritive fluids or from passing out of the body with the feces. The adaptive adjustments to the protozoa are quite comparable to the endoadaptations between tissues and between organs in the body of an individual organism. In other words, the flagellates function much as a tissue of the host, are serviced by the body of the host, and in turn service the body. Here we have an extreme example of a mutualistic relation between widely different species that are adaptively integrated into a supraorganismic interdependent system. Through the work of Kirby, Cleveland, and others, we can trace the phylogeny of such supraorganisms from the roaches to the termites and among the termites through generic and family divergence. The reciprocal evolution of both protozoans and host insects is apparent. Cleveland has advanced the ingenious hypothesis that the family adjustment of adult and young roaches was selected because of the greater efficiency in transferring the protozoans from one individual to another. Once the family

became the unit of natural selection, further adaptation toward more complex social life gave rise to sterile castes and greater social division of labor. Thus the social specialization of termites was preceded by the earlier evolution of the roach-protozoan supraorganism.

With the evolution of such a mutualistic system one might expect that the relationship between interdependent organisms, once established, would not be lost. However, the most advanced taxonomic family of termites, constituting three-fourths of the known species, has lost the protozoans, digests the cellulose without their assistance, and seems to have undergone an almost explosive adaptive radiation subsequent to its divorce from protozoan symbiotes. The evolution of this termite-flagellate interspecies supraorganism together with its subsequent regressive evolution indicates that an interspecies system may give rise to a better-adapted intraspecies supraorganism. We may also guess that greater cooperation between the parts of an integrated system is more easily evolved in genetically continuous systems than in ecologically continuous systems.

A large number of ecologists have contributed detailed information concerning the fascinating interrelations between organisms composing the ecological community. Among them Shelford, Wheeler, and Orlando Park may be mentioned. Much remains to be learned about community coordination, but it is already becoming fairly clear that the interspecies system has many organismic attributes and that it represents a real biological system of great importance. As with the social supraorganism, the ontogenetic and phylogenetic processes have led toward greater integration, interdependence, and ecological homeostasis.

The analogue of a community boundary or skin may even be significant. If the species of a given community have evolved reciprocal adaptations, one would expect that it would be difficult for unadapted foreign species to invade a community to which they had not become adjusted. A high proportion of plants and animals introduced from foreign communities fail to pass through such a biotic barrier. The new physical environment may be quite well suited to the introduced organism, but the relations to the other organisms are likely to be incompatible without a long process of natural selection that guides them toward a reciprocal adjustment.

One case among termites may be used as an illustration of a community barrier. Prior to 1890 an oriental species, *Cryptotermes dudleyi*, was introduced into Panama. Much searching has failed to disclose its presence in the natural forest or savanna, but the species is common in the houses and man-made structures throughout Panama. This termite was found living in the furniture of one of the laboratory buildings on Barro Colorado Island in the Canal Zone, but it could not be found in the forest twenty feet away from the house.

Whether it be competition with closely related indigenous species or an inability to ward off various predators or parasites that prevents the invasion of the natural community, I am unable to say, but there is a rich field for ecological analysis involved in the solution of such a problem. The economic aspects of biotic barriers are tremendous. If the introduced African *Anopheles gambiense* carrying malignant malaria had been able to invade the natural forests of Brazil, the dramatic eradication of the species from human habitats would hardly have been possible. A similar biotic barrier enabled entomologists to wipe out the infestation of the Mediterranean fruit fly in Florida.

I venture the opinion that certain parallels between the insect society in its community relations and human society have been suggested by this discussion.

In making comparisons between termites and men are we leaving the realm of science? Are parallels purely chance resemblances with no common attributes or causes? Are we in danger of entering the sphere of mysticism and metaphysics when we use the term "social" to include both a termite colony and a human aggregation?

There is much controversy concerning these questions. Some stems from ignorance of recently discovered facts. Some is the result of the inability of certain analytical minds to conceive of the synthetic whole. Emphasis upon differences in comparative systems sometimes blinds one to the similarities. Much difference of opinion is the consequence of too much unsubstantiated theory, and better solutions must await the patient accumulation of more detailed information. Verbal metaphors may be misinterpreted. Biology has been used to rationalize political bias, as I suspect was the case when Churchill suggested the study of termites to Stalin.

Insect and human societies are different in certain fundamental aspects. The evidence is strong that the major societal integration of insects is based upon genetically induced physiological and behavior mechanisms. The evidence is equally strong that the major societal integration of man is founded upon individual experience and learned behavior. Man shares his hierarchy founded upon leadership and dominance with the vertebrates. Man transcends the highest vertebrates in his ability to communicate by means of symbols and to transmit the experiences of one generation to the next by use of these symbols, thus building up a social heredity distinct from his germinal heredity. Whereas the insect society is closely parallel to the multicellular organism in its hereditary mechanisms, human society has acquired a traditional culture not transmitted through the germ plasm but through objects, customs, institutions, and language. As a result of learned behavior and symbolization, great complexities of social interaction occur in human society not manifest in other societies. Educational institutions, religion, and political ideology are absent from subhuman societies. The similar social patterns of insect and human populations are preponderantly analogous and not homologous, although there is much functional resemblance. We still find a host of striking convergences in the two systems—division of labor, communication, cooperation, architecture, agriculture, and partially isolated units within the species that tend to repeat and maintain the social structure of other units.

Both termites and men control their own microclimates through cooperative construction; they both protect, shelter, and feed associated plants and animals; their food supply is stabilized through storage and cultivation. The two types of societies have evolved toward more intricate and more controlled homeostasis.

The evidence clearly points to the germinal evolution of the insect society through natural selection. It seems reasonable to believe that human cultural evolution is also the result of a selection of the more cooperative systems leading toward increased social coordination. But, in the case of human society, selection must act upon cultural units in addition to gene systems. Comparable correlated orderability between widely different phenomena indicates a common causation.

An understanding of well-founded principles of organic evolution enables us to guide the rapid evolution of our domestic plants and animals. It seems reasonable to suppose that an understanding of the principles of human social evolution will enable us to direct our own rapid social advance. I think I am correct in stating that a tremendous increase in fact finding, correlation, and synthesis is necessary before the fruits from applied social science

can compare with the harvest from the natural sciences. Biology is as fundamental to the social sciences as the physical sciences are foundational to the biological sciences. Already genetics, medicine, agriculture, ecology, geography, psychology, and anthropology are bringing aspects of biology and sociology together, but I am confident that we are on the threshold of a much greater mutualism between these sciences. In the face of our increased power for destruction, it would seem imperative that much more support be given to researches in both the natural sciences and the social sciences. At the present time there is a lack of balance between these two fields of knowledge that may be disastrous if not corrected.

There may be more of the subjective, emotional, and untestable in social science than in natural science, but we are all aware that the natural sciences have faced great difficulties in overcoming prejudice and defeatism—difficulties that now hinder social science investigation. Some social scientists have a glorified concept of the precision and predictability of the natural sciences. Those social scientists who believe biological laws enable us now to predict coming events with exactitude are probably not acquainted with the Harvard Law of Animal Behavior which states that under precisely controlled conditions animals do as they damn please. Many problems in both the natural and social sciences resist solution, and a complete understanding of complex phenomena cannot be anticipated, but even the slow progress already realized has fully repaid the effort. The pessimism often detected among social scientists is in part the result of comparing their techniques and theories with those of the exact sciences. A comparison with the advances in biology would probably allow greater optimism. The problems of human society are much closer to those being solved by the biologists than they are to those of astronomy or nuclear physics. Without expecting Utopia, one may confidently expect an intelligent solution of some of our major social problems following an energetic application of scientific methodology to an analysis of social systems.

Notice my prediction does not include an increase in happiness! Possibly the constructive channeling of human aggressions away from class, racial, and international warfare may produce greater happiness, but happiness has a large subjective component not greatly altered by the environment of the individual. With our goals expressed in more objective terms, the applications of scientific methodology to social, economic, political, and moral problems will surely lead to a more adequate understanding of the forces involved, a more adequate control of our physical, biotic, and social environment, and greater security—in other words, society will evolve toward greater social homeostasis and will thus repeat the evolutionary trends so clearly indicated in the evolution of plant and animal populations.

Lest the concept of the evolution of the population supraorganism be construed as an advocacy of totalitarianism, either of the fascist or communistic type, I should like to sound a note of warning. The human social group, whether an institution, a class, a race, a tribe, a nation, or the species as a whole, has probably evolved through the operation of factors similar to those guiding biological populations—namely, a certain moderate amount of variation, a certain degree of isolation, a selection pressure favoring beneficial changes in relation to both the environment and the organization of the system, and the transmission of the basic pattern together with the surviving variations to succeeding generations. Whether human social evolution leads toward autocracy, fascism, communism, or democracy may

well rest upon the interaction of these political systems with the prime social evolutionary forces that parallel those of organic evolution in general. Variation may be partially analogized with the creative arts and sciences; germinal heredity with tradition and culture; isolation and consolidation of gene patterns with the isolation and consolidation of cultural patterns; natural selection with social selection of more efficient interactions; and "artificial selection" may be analogized with intelligent selection and perpetuation of the beneficial social variations. As in the case of the social insects, the integrated, cooperative, human group may be selected as a unit, so that survival may well be in terms of the value of the variant to humanity as a whole rather than for the good for a few at the expense of the many. Sacrifice by some for the good of the species as a whole has convergently evolved in both biological and human systems at all levels of organismic integration. Many ethical principles may thus be seen to have a scientific foundation.

Biological elimination is often conceived as ruthless and absolute. Competition may be beneficial at an optimum and deleterious at a pessimum or maximum, as Allee has experimentally demonstrated. Even at the physiological level, we find gradations in the factors of survival. The destruction of a soldier termite while it is protecting the colony may well exert a selective pressure favoring soldier genes in the reproductive castes. With integration based upon learning, elimination or survival of an attitude or an idea may not be correlated at all with the elimination or survival of the individual involved. We have witnessed a gross misunderstanding of the operational factors of social evolution in totalitarian states that eliminate individuals with variant ideologies, thus attempting rather unsuccessfully to destroy the social variability upon which progressive evolution depends. As in the evolution of nonhuman populations, the benefit to the whole group is not independent of the benefit to the individuals composing the group. The fittest society, in all probability, will not be either one in which the group exploits the individuals composing it or in which the individuals exploit the group. Rather it may be expected that surviving populations will be coordinated under such a formula as "one for all and all for one." And in the case of humans, we may expect an intradependent world society to emerge.

I hope I have conveyed some small inkling of the fascination that termites exert on the biologist and potentially on the sociologist. A comparative study of termite social organization gives us an insight into biological and social mechanisms, and principles of far-reaching significance are the outcome. It is my conviction that each taxonomic or ecological group of plants or animals, including humans, will well repay a close study of its organismic and ecological relations, both contemporary and ancient. An enormous number of correlated facts are necessary to build sound generalizations, and each tentative hypothesis should be verified by the further accumulation of pertinent information, whether the field of inquiry be natural or social science.

None of the reasons I have given for the study of termites occurred to me until long after I found myself engaged in detailed study of their life. Inductive science demands years of routine investigation by many individuals. In order to understand why anyone dedicates his life to such specialized research, I suspect we should have to consult the psychiatrist mentioned earlier. I also suspect that such abnormal aberrations have survival value. As Wheeler once put it, mutations in ancient apes produced a type of insane behavior characterized by a drive toward progress. Having survival value, this insanity soon spread to most of the ape's descendants.

TO A CADAVER

By Arden Almquist

I take this scalpel reverently,
Hesitant to violate the one thing
That was yours alone to give.
I do not know you—not even your name—
But I know your race.
You belong to those who stand in Simon's line—
Black Simon, who bore the burden of the Cross
When Jesus fell beneath the load—
Who have, the centuries since,
Borne patiently their own crosses,
Searching for liberty and life and honor . . .
If I dissect as reverently with this blade
As Carver held the test tube in his hand,
I can do you and yours no wrong.

I also know your sex . . . You are of those
Who give the race what continuity it knows
In all its fitful change.
Yet I know not how many sons you may have borne
Or whether you have known motherhood at all.
I can but guess your age, the work you have done,
The pains and joys you've had in alternation.
These are secrets shut from me forever.
But there are other secrets, hidden now by your brown skin,
Which through you I hope to learn.

I do not know you.
I only know that I am in debt to you . . .
And this much more, perhaps—
That but for some unguided stroke of Fate,
Or, more mysterious still,
But for some uncomprehended purpose of our common God,
I might lie upon this table,
Strong-smelling, cold, and hard,
Forsaken and alone,
And you might hold this knife.

LIAISON WITH YOUTH

By HARLOW SHAPLEY

President, American Association for the Advancement of Science

I HAD obtained my radio operator's license from the government," writes Irwin Cole in the essay which follows, "and was discussing the oscilloscope with a friend on the air" when the answer was found to a problem that had bothered for a year, and forthwith the "slight trace of ripple in the pattern" was removed. Some unnamed radio amateur, professionally associated with a technical laboratory, had made a liaison by "air" with a young explorer.

In the other essay selected from the Talent Search winners, David Shappirio, young expert on wasps, reports the reinforcement of his knowledge and enthusiasm by the professional entomologists.

I am impressed with the importance of helping to remove ripples from the pattern at the critical times when discouragement is throttling enthusiasm. Probably the greatest boon to the exceptionally talented young scientist is the free and easy contact with competent elders. It is too easy to impress one's teen-age associates. (And teachers too often are too tired.) Their genuine bafflement and adulation often lead the gifted schoolmate into superficiality and empty exhibitionism. The experienced elder, however, with no less admiration for youthful talent, can easily in a single interview teach the value of balance, modesty, judgment, and accuracy. With proper handling even the fanatic can be rescued.

In our annual Science Talent Institute in Washington we try to provide for this liaison between those who would go far in science and those who have done it. We confront young talent with adult knowledge and experience. The forty winners of the Talent Search not only get to see the sights, scientific and otherwise, of the national capital and have the excitement of competition for the Westinghouse scholarships, but they meet on an intimate basis the top-ranking men of several fields. Personally and scientifically those conferences of experts constitute the most profitable phase of the Institute.

The continuing intellectual association of the isolated or semi-isolated scientist with those whom circumstance or choice has put in the thick of scientific activity, is an important problem throughout America at this time. The professional scientific societies do something to alleviate the intellectual loneliness of the amateur and of the heavy-scheduled teacher. But such occasional service is not enough.

Is there not perhaps an opportunity near at hand for deliberate and profitable liaison work on the part of the scientists who are in those large institutions where doctorates are earned and bestowed? Could there not be a systematic program of keeping in personal touch with every graduate? Many of them have gone out highly trained and fully ambitious. But they have too often become negligent of scientific responsibilities because of a heavy load of teaching and choring, or because of family diversions, or of unfriendly environment. A periodic check-up from the old school, or especially from a sympathetic former teacher and research guide, will often serve to keep willing sparks alive, and often serve to remove some ripples from the pattern.

OBSERVATIONS ON WASPS

By DAVID G. SHAPPIRIO
Roosevelt High School, Washington, D. C.

During the past three years, I have collected wasps in the District of Columbia and surrounding area. A paramount reason for my interest in this field lies in the fact that, although the occurrence of wasps is quite widespread, little is known of their ecology. The group thus offers excellent opportunities for new and valuable observations.

My project is an investigation into the habits of these insects. This investigation will provide the basis for a complete list of the wasps occurring between the areas covered by the New Jersey and North Carolina lists, together with flight tables and notes on the habits of each species, as far as they are known. There has never, in recent times, been a list of wasps from this region, and such a list would undoubtedly have much value. It could be supplemented at a later date by similar investigations in other fields.

The following field observations comprise material not heretofore recorded.

Family Mutillidae. Female mutillids, because of their often dense pubescence and lack of wings, are known as velvet ants. They possess a long sting capable of inflicting a very painful wound on the collector who attempts to acquire them by hand. The male mutillid has no sting but possesses wings.

This family offers much opportunity for observation. Larval hosts of only seven of the thirty-one local species are known, and so little is known about specific characteristics that males and females often must be treated as distinct species. I have occupied much time in observations upon these very interesting insects.

On a June afternoon I was pulling up moss in a search for beetles when I noticed several mutillid females under the moss. They seemed unaccustomed to the bright light, and four specimens were taken. These were identified by Dr. H. K. Townes, of the U. S. National Museum, as *Dasymutilla carinicepes* Fox. Once in 1945 I found another *carinicepes* under moss, but attempts to trace it to its prey failed. I believe that this species may prey on certain insects found under moss.

On August 14, 1946, I noticed what appeared to be a queer spider and, on closer investigation, I saw that it was a pair of mating mutillids. Immediately I picked up the pair, since it is very unusual to capture mutillids while they are mating. I recognized the male as *Pseudomethoca geryon* Fox, but the female appeared to be *P. simillima* Sm., a species of which the male was already thought to be known. *Geryon* had until this time never been associated with a female *Pseudomethoca.* Mr. Schuster, of the University of Minnesota, confirmed my identification. This observation has proved that *P. geryon* is the male of *P. simillima*, and the male which formerly was known as *simillima* becomes a separate species. I am advised that this observation will be described by Mr. Schuster in a paper he is preparing.

On August 28, 1946, another pair of mating mutillids was captured. These were *Pseudomethoca frigida* Sm., identified by Mr. Schuster. Several additional matings of *Pseudomethoca* were observed during August, all on the surface of the ground at the edge of woods.

We have caught thirty-one species of mutillids in the past three seasons here, including many specimens of the rare genera *Ephuta* and *Photomorphus.* Professor Fattig took only twenty-one specimens of these

mutillids in fourteen years of collecting mutillids in Georgia.

Family Psammocharidae. One of the more interesting families in the class Insecta, all genera, with the exception of *Ceropales*, are parasitic on spiders. Although these insects inhabit shady areas which afford excellent cover and are in addition reluctant to fly, they are, without doubt, the most difficult of the wasps both to observe and to capture. Little appears to have been published on the rarer subfamily Psammocharinae.

While collecting late in an afternoon, I noticed a small red-and-black psammocharid dragging a paralyzed wolf spider. I should have liked to observe her further, but she approached dense bushes, and, to avoid her loss, I captured her. She was identified as *Arachnophila divisa* Say, by Dr. K. V. Krombein, of the U. S. National Museum. Her prey was identified by Dr. H. H. Swift as *Lycosa sp.*

Family Vespidae. To this family belong the commonest of the wasps in this region, those of the genera *Polistes* and *Vespula.* These genera include the true social wasps. Much about their ecology has been recorded.

During September 1946 I attacked an underground nest of *Vespula squamosa* in order to obtain any parasites present. I sprayed DDT into the nest by way of an entrance from which *squamosa* workers issued and found that the wasps emerged from another entrance. When I sprayed DDT down that entrance also, wasps emerged from a third entrance, this one over a yard distant. Then I noticed a fourth entrance, paper-lined, out of which queens and males only rose. Further investigation revealed a total of five entrances, one of which was lined with paper of the type used by the wasps in covering their nest. The passage connecting this entrance was also lined with paper.

The outstanding fact remains that only queens and males used this entrance. No previous record of such an occurrence has been noted.

Family Sphecidae. To the family Sphecidae belong a great part, if not the majority, of the wasps. For the most part, they are beneficial, because they utilize a variety of harmful insects as larval food.

During July 1944 I was fortunate in capturing a new species of the genus *Larropsis.* I saw a small black wasp dive upon a cricket and sting the cricket until it offered no further resistance. The wasp then began dragging the cricket off toward the base of a tree. As she entered some tufts of thick grass, I captured her to avoid losing her, but the cricket, unfortunately, was dropped and lost.

Dr. Townes could not find any similar species in the U. S. National Museum collection and identified her tentatively as *Larropsis* n. sp. Dr. G. E. Bohart, of the University of California, confirmed this identification. Because of the fact that this was a new species, the wasp was donated to the U. S. National Museum collection. The wasp is now being described by Dr. G. E. Bohart at the University of California in connection with his studies on the Larrinae.

I returned many times to the area in which I captured this wasp, to try to obtain more specimens of it, but the plants which grew there had been removed, probably dispersing many of the insects which nested there.

Motes argentatus, ordinarily very common in this locality, was extremely so during 1944. In fact, it appeared in surprising numbers throughout October, usually the "end" of the season. On November 19, in Rock Creek Park, wasps were noticed flying in an area in which I often collect. Since the temperature could scarcely have been more than 55°F., the matter was pursued further.

The only wasps present were *Motes argen-*

tatus Pal. de B., but these were flying quite actively, despite the temperature. Several specimens were taken, all females. On November 26 I was surprised to capture two more females; and by this time, regular visits were made to the lot to see how long the *Motes* would remain. The following is a list showing the dates of capture of *Motes argentatus* throughout the winter 1944–45:

November 26, 1944.....	2 females
December 19, 1944	1 female
January 28, 1945	1 female in snow
February 11, 1945	3 females
February 25, 1945	3 females
March 10, 1945	1 female in snow

Thus, these insects, normally considered to appear in hot weather only, were captured quite consistently during the winter months. May this not necessitate a revision of our usual beliefs concerning the seasonal appearance of wasps, at least for this species?

During 1945, several wasps were caught which did not resemble anything I had previously seen. These proved to be *Bembecinus nanus* Hdl. Throughout the summer of 1945 this species was abundant, and during 1946 it was often observed nesting, so that the prey, which up to that time was apparently unknown, could quite easily be ascertained. Several females with their prey were taken, the prey in all cases being *Graphocephala versuta* Say, a common leaf hopper. (Kindly identified by Dr. P. W. Oman, of the U. S. National Museum.)

The method by which the female *Bembecinus* gets her prey and transports it to her burrow may be described as "typical" for many of the solitary wasps. The female wasp seizes her victim, overturns it, and paralyzes it by stinging it in the thorax. She grasps the prey under her body with her middle and hind pairs of legs and, carrying the leaf hopper in this way, flies to her burrow. She lands directly at her burrow and opens it with her front pair of legs, which have been left free.

I have not observed the method by which she deposits her egg on the paralyzed prey, but she returns daily with one or two freshly paralyzed leaf hoppers for the larva now living in her burrow, resembling in this habit many of the Bembecine wasps. A paper on this observation and the one immediately preceding it has been accepted for publication in the *Entomological News*.

IT WILL thus be apparent that the field of entomology in this region offers endless possibilities for research. The broad implications of such studies will be manifest from the following quotation from Rau's *Wasp Studies Afield:*

> The social and solitary wasps afford especially fine material with which to work out such problems as the correlation of habit to structure, the origin of socialization among insects, the development of intelligence from instinct, as Whitman would have it, or the independent origins of instinct and intelligence, the problem of psychic acquisitions as per Lamarck, the position of natural selection as a factor in preserving and accumulating favorable variations in behavior, ...

THE GRAPHING OF EQUATIONS BY ELECTRONICS

By IRWIN H. COLE

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THE idea of graphing mathematical equations on a cathode-ray oscilloscope occurred to me while studying second-year algebra. The assembly of the proper controls in a cabinet in conjunction with the oscilloscope made it possible to graph equations very quickly.

Before discussing the method of graphing algebraic equations, it would be helpful to relate my experiences in building the oscilloscope. When I became interested in electronics seven years ago, my main goal was to obtain a radio operator's license and have my own short-wave station. I began to study the fundamentals of electricity, supplementing textbook study with experimentation using old radio parts. As I learned the functions of the various parts, I became so enthusiastic that I began constructing many devices for testing and checking information. It was in this way that the necessity for a cathode-ray oscilloscope first became obvious.

My original oscilloscope was constructed from a basic circuit diagram from the *Radio Amateur's Handbook* and was essentially a device for lighting the cathode-ray tube and producing a spot on the screen. This diagram had omitted an intensity control which was vital to the operation of the unit, and, since the other circuit diagrams were too costly to build, I decided to design this and several other additions myself, using available "junk-box" parts.

I discovered that, by changing the voltages on the focusing electrodes, the operation of the electron microscope could be duplicated. Unfortunately, specimens could not be placed inside the tube, but a very clear shadow picture of the various elements inside the tube appeared on the screen, magnified several hundred diameters. The image even showed various flaws and irregularities of the metals used in the tube.

After building a linear sweep oscillator, it was necessary to design positioning controls for centering patterns on the screen. This seemed to be quite difficult, as each circuit would cover only one quadrant of the screen. After trying about seven different circuits, I finally evolved one that would cover the entire screen. I now had an instrument that would perform quite well for my purposes.

It was about two months before the need was felt to add any improvements to it. A vertical and a horizontal amplifier increased the sensitivity of the unit to about one volt per inch, making it so sensitive that it had to be rebuilt and enclosed in a steel case. Even then, no matter how well the transformers were shielded or the d-c supply voltage filtered, there was a slight trace of ripple in the pattern. It was not until another year had gone by that I learned how to eliminate this fault. I had obtained my radio operator's license from the government and was discussing the oscilloscope with a friend on the air. He told me that he worked at Dumont Laboratories and had had similar troubles with an oscilloscope he had built. He suggested that I obtain a length of two-inch iron pipe from a plumbing supply company and enclose the cathode-ray tube in it, thus shielding it completely from the magnetic effects of any a-c components. This was very effective in eliminating the ripple.

Now the application of the oscilloscope to the graphing of equations may be described. The X and Y axes correspond to the horizontal and vertical movements of the spot on the screen. By varying the

voltages applied to the horizontal and vertical plates, the spot can be made to trace the graph expressed by the equation. Linear and quadratic equations in two unknowns are the easiest to produce, although many others are possible.

Any equation may be expressed by a pair of parametric equations using a third unknown, which in this case is time (θ). In general, if $y = f(x)$, then $y = f'(\theta)$ and $x = f''(\theta)$. When voltage varying as one of the parametric equations is applied to the horizontal deflection plates, and another voltage varying as the second parametric equation is applied to the vertical deflection plates, the image on the screen represents the combined parametric equations, which is the original equation.

To produce the graphs of linear equations, two alternating voltages of the same frequency and of the correct phase relationship are applied to the horizontal and vertical deflection plates. The slope of the line determines the ratio of the two voltages and their phase relationship. If the slope is negative, they will be in phase; if the slope is positive, they will be 180° out of phase. The line may be shifted to the right or left, according to the constant of the equation, by the positioning controls.

The circle is the easiest of the graphs of quadratic equations to produce. It is produced by impressing equal voltages of the same frequency, but 90° out of phase, on the horizontal and vertical deflection plates. By changing either the horizontal or vertical voltage, an ellipse is formed. The ellipse may be rotated on the axes by varying the phase relationships of the two voltages. Its location with respect to the origin can be set with the positioning controls.

The general equation for the circle is $x^2 + y^2 = R^2$, which can be broken down to the two parametric equations $x = R \sin \theta$ and $y = R \cos \theta$. When both parametric equations are functions of θ, as is the case with the circle, the frequencies of the voltages are equal. When one equation is a function of θ and the other equation is a function of 2θ, as is the case with the parabola, the frequency of one voltage is twice the frequency of the other voltage. In every case the magnitude of the voltage is determined by the constant R. Since the only difference between $\sin \theta$ and $\cos \theta$ is that one is 90° out of phase with the other, by having one of the applied voltages lag the other by 90°, all the mathematical conditions of the parametric equations have been complied with, and the resulting curve is a circle.

The chief value of this project has been not only to make me familiar with the construction and applications of the oscilloscope, but also to further my knowledge of the graphing of algebraic equations.

Book Reviews

THE NEW ASTRONOMY

The Earth and the Stars. C. G. Abbot. xii + 288 pp. Illus. $3.75. D. Van Nostrand. New York. 1946.

SOME sixty years ago Dr. S. P. Langley, afterwards Secretary of the Smithsonian Institution, wrote an exceedingly readable book entitled *The New Astronomy.* About forty years later Dr. C. G. Abbot, then Assistant Secretary of the Smithsonian Institution, published a similar popular work, and now after another twenty years Dr. Abbot, as retired Secretary of the Institution, has revised the work under review, which as before is "intended for those who wish to acquire by easy reading a general survey of the universe they dwell in." In the preface to his book in the 1880's Langley made a plea for greater emphasis on the New Astronomy, or astrophysics, stating that the Old Astronomy was receiving most of the benefits of governmental and private support. The situation by now is entirely reversed, and the Old Astronomy is doing the begging, as might be inferred from the general content of Abbot's current work.

Comparing this revised edition with the first one, we note at once the improvement in format and typography. Even where the wording is the same, the entire work has been reset. Also the order of the chapters has been changed; we might say that what is now the first part was formerly the second, and vice versa. Beginning with The Mighty Universe, the author carries out his purpose "to give the general reader first of all the awe-inspiring picture which, with present knowledge, the universe presents." Many details are suppressed or transferred to later chapters in order that nothing should blur the large view. The solar system and related topics are now discussed in the second part; atoms, spectra, and star conditions are taken up after the chapters on the stars. This method of proceeding from the outside inward, from the large to the small, may have its advantages but it results in sketchy treatment in places, and there are some curious omissions. For instance, the velocity-distance relation for extragalactic nebulae is illustrated by a plate without comment, and neither here nor elsewhere in the discussion of galaxies is the name of Hubble even mentioned.

Very properly, considerable space is devoted to the author's own field of the sun. Besides a popular account of his well-known studies of the possible correlation of solar variation with changes in the weather, we have the always fascinating account of his experiments on the direct utilization of solar energy. He has accomplished, perhaps, as much as anyone in this field, having carried on all the cooking operations of a small family for a season or so with his solar cooker on Mount Wilson. In solar engines, however, no really striking advance seems to have taken place in the past fifty years. The development of atomic power close at hand on the earth bids fair to outstrip the use of the same kind of power transmitted to us by radiation from the sun.

The numerical data in the text have usually but not always been brought up to date and, like the glossary of terms in the appendix, would often not withstand the critical scrutiny of a teacher and students in the classroom. The general student of astronomy is usually introduced on the first day to the conception of fundamental

circles on the celestial sphere, but here he would find the meridian defined as a plane, the equator as a circle, the ecliptic as a path, and the horizon as bounded by a plane.

On the whole the author has written a very interesting work for general readers, and many of these will turn to it again and again. The New Astronomy never gets old.

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CARE AND TRAINING OF EXECUTIVES

Executive Ability: Its Discovery and Development. Glen U. Cleeton and Charles W. Mason. 540 pp. $4.50. Antioch Press. Yellow Springs, Ohio. 1946.

CLEETON and Mason have re-examined the executive's place in industry and society to take account of new information and in the light of changes in governmental and industrial policies since the first edition of this book was published. Thirty percent of the references are of 1940 or later and 54 percent are of 1935 or later. Yet the basic philosophy remains unchanged, and the orienting introduction of the first edition is reprinted in the current one.

The executive is analyzed from several standpoints, but always he is viewed as an important, socially conscious member of a democracy in which he must show broad vision and mature understanding of the interrelated interests of capital, labor, and consumer in order that all three may share equitably in the wealth produced by industry. The hard-driving exploiter of labor and the consumer is not the executive for whom or about whom Cleeton and Mason have written.

"An executive," they write, "is a person who is responsible for the efforts of others, makes decisions on questions both as to policy and practice, and exercises authority in seeing that decisions are carried out." About 12 to 15 percent of the personnel in a typical company have some degree of executive responsibility.

The book is written for the several million who are executives, who aspire to be executives, who must select and promote executives, or who must train them to meet their responsibilities as executives. It describes executive functions briefly in an early chapter and in detail in half a dozen later chapters. A final chapter considers the executive's place and obligations in a democracy.

The characteristics, selection, and training of executives are examined.

> An executive is a well-rounded individual who does not deviate outstandingly from the average person of general intellectual superiority when measured by psychological tests; but who does deviate outstandingly by exceeding the averages shown by others on estimated qualities involving health, drive, judgment of fact, reaction to human qualities, and leadership.

A number of rating scales are included in the volume to aid in selecting potential executives, appraising the abilities of present executives, or judging one's own executive ability. Two chapters are devoted to developing executive ability, and one consists of hints for prospective executives.

The psychologist who is not an executive is likely to be disturbed by the oversimplification of some of the difficulties of measuring personality traits and predicting their roles in the successful discharge of executive functions. For example:

> By applying the questionnaire it is possible to determine the extent to which a person is emotionally well adjusted. Using a similar list of questions, Bernreuter standardized and validated scoring keys for four personality traits referred to as neurotic tendency, self-sufficiency, introversion-extroversion, and dominance-submission. Later Flanagan validated scoring keys for the Bernreuter inventory

for two additional traits, self-confidence and sociability.

Neither of these men would claim that six separate scores should be derived from the Bernreuter inventory. In spite of such oversimplifications, the volume should be of interest both for the information it contains and for the problems it discusses which need further analysis and study.

The executive who is not a psychologist will find in the volume both elementary discussions of psychological processes and suggestions for their practical employment. Study and application of the principles discussed in *Executive Ability* should make him a better executive.

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THIS SHRINKING WORLD

Communication Through the Ages. Alfred Still. 201 pp. Illus. $2.75. Murray Hill Books. New York and Toronto. 1946.

THIS reviewer admits—but refuses for that reason to disqualify himself—a prejudice in favor of a new book by Professor Still, because of his experience in transmitting power electrically, his texts on the machinery involved, and his twoscore years as capable head of the electrical engineering department in Purdue University. Thumbing the pages of the book before settling down to reading, the reviewer recognized many of the diagrams which usually illustrate the development of the electrical arts of communication; but enough were unfamiliar to assure him of new subject matter and interesting reading.

And the book proved to be interesting, clearly and gracefully expressed, with now and then a humorous touch but more frequently a mildly caustic interjection which showed that the author was himself not without the saving grace of some prejudices.

Why was it, then, that the first chapter left a foreboding that one was on the way to something with which he wouldn't agree—and might even deplore? The last chapter justified the fear. Under the title, Communication without Words, it deals, sympathetically to say the least, with the subject of telepathy. Although the author says, "No attempt will be made here to prove the existence of telepathy," he then refers the reader to "the modern literature treating of this subject," which includes, as might be expected, the books of Thomas Jay Hudson, Oliver Lodge, J. B. Rhine, Whateley Carrington, Rudolph Tischener, and F. W. H. Myers.

The fact that the author does not adduce proof does not mean that he has any doubts, because on the same page he says (italics mine): "Telepathy *is* a means of communication. . .The *sages* of Oriental countries *have practiced it* from time immemorial. . . . The word [telepathy] was suggested by F. W. H. Myers *in 1882 when experimental proof was first given.*"

The author quotes Hudson as referring to "*telepathy* as *a normal means of communication between animals.*" (But how can anyone know that?) To it he credits the successes of many animal trainers. The friendly dog you meet when out walking "has in some occult manner gotten in touch with that part of you which is primeval and eternal." Well, if that is what the author wants to believe, the "will-to-believe" must be credited with some potency. From my own experience, however, I wouldn't trust the telepathy of a dog if I had a pack on my back or was mounted on a bicycle or scooter. When a child accepts a stranger, one usually ascribes his readiness to do so to past experiences with other men of similar clothes and manners who have proved friendly and interesting. Of course the child doesn't have the keen

scent of a dog nor his reaction to such faint odors as a surge of adrenalin might liberate from a stranger who was fearful.

If, as is usually true, the message of a book is to be found in its first and last chapters, this review might just as well end here. To no purpose would be any of the critical comments, captious or well justified, which might be directed toward some of the statements in the intervening chapters. If, in your scheme of beliefs, you need telepathy and extrasensory perception, read the book and enjoy what is to the present reviewer a still unjustified conclusion. If you don't, tear out the first and last chapters and you will be left some interesting historical information. But don't expect adequacy of treatment of present-day radio or the methods of television. Perhaps the sufficient reason for that inadequacy is that these portions of the electrical arts of communication are so much more complex than the earlier inventions that they resist similar portrayal.

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ODYSSEY

Charles Darwin and the Voyage of the Beagle. Lady Nora Barlow, Ed. 279 pp. Illus. $3.75. Philosophical Library. New York. 1946.

THIS collection of family letters and notebooks written by Darwin during the voyage of the *Beagle* contains a few letters which have been partly published in the various biographies, but most of them are published for the first time in this book. Letters are always an intimate window through which to view family life, and Darwin's are particularly so. They are also a fine mirror of the little details of life in the early days of Victoria's reign. As for the notebooks, they are a series of 24 small pocket books in which Darwin entered all sorts of impressions, most of which were later expanded in his diary, together with such miscellaneous memoranda as supplies to be obtained in the next port of call and experiments to be made to verify the questions that occurred to him. These notebooks have been edited by the omission of much of the routine geological matter and the duller spots, but they are nevertheless a fine introduction to the young Darwin informally putting down his random thoughts and observations.

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ART AND THE GOLDFINCH

The Symbolic Goldfinch: Its History and Significance in European Devotional Art. Herbert Friedmann. 254 pp. Illus. $7.50. Bollingen Series, VII. Pantheon Books. New York. 1946.

IT IS not given to many scholars to combine their vocational and avocational interest so felicitously as Dr. Friedmann has here done in this book on avian symbolism. Besides being, professionally, Curator of the Division of Birds in the United States National Museum, with all the proficiencies in zoology and museology that such a position entails, Dr. Friedmann is also, extracurricularly, a recognized authority on the history of art. In this volume he has made his science of ornithology the background of a study in the realm of esthetics. Essentially, however, it is a scientific study so far as method is concerned, and it is significant that two years ago the gist of this book was presented before, and well received by, so conservative a scientific body as the Washington Academy of Sciences.

Birds, as well as other classes of the animal kingdom, have served man for many centuries to represent his ulterior ideas. When his thought and expression promised to become too blunt and direct for his finer

sensibilities, what was more natural than for him to use some animal and its attributes to symbolize by a roundabout and often more beautiful way what he was trying to say? Sometimes, as in the case of the roc and the phoenix, he created fabulous birds to serve his imaginative ends. Frequently, with the passing of time, original symbolic meanings became obscured and submerged in the common heritage or lingered merely as traditional decorations. Now, after many centuries, virtually every bird and beast has become to us more than a bird or beast.

Dr. Friedmann has chosen to limit his exploration to the allegorical symbolism of one bird, the European goldfinch (*Carduelis carduelis*), as it appeared in European devotional art for half a millennium, from the mid-thirteenth to the mid-eighteenth centuries. He has diligently searched the literature and examined the paintings in many American and European art galleries and found nearly five hundred paintings in which the goldfinch figure occurs, the bird usually being held in the Christ child's hand. Italian paintings, representing all the main schools, yielded the greatest number, but the symbol was found also in Spanish, Flemish, French, Germanic, Austrian and even Russian religious art. Furthermore, it was soon discovered that the symbolism involved was polyvalent and extraordinarily complex and could not be reduced to simple interpretation. Although the original thought behind the use of the small bird motif in a painting was probably the desire to give to the Christ child represented some attributes of a child, this would not have been enough, says Dr. Friedmann, to perpetuate the innovation. We have the author's own brief summary of the multiplicity of meanings that he found the goldfinch to represent:

In common with other small birds, it was the symbol of the Soul; in common with other small birds with reddish markings it was a symbol of the Passion, the Crucifixion, and of Redemption [and even of Death]. Peculiar to it, and in this respect wholly Italian, was a connotation of Fertility (pictures in which it was included were thought as apt to insure offspring to the donors), and by a long and complicated series of mystical identifications proposed by various medieval ecclesiastics it also came to be a substitute for a long-established disease augur, the oldest version of which was the ancient Greek legend of the "charadrius" that cured jaundice by looking at the patient and absorbing the illness. The ravages of the plague, especially during the fourteenth century, brought the themes of disease augury and fertility close together, as a hope for survival, and gave the symbol greatly increased popularity.

Searching out the ends and ramifications of these symbolic meanings and assembling the evidence furnished a nice research problem. It meant combing medieval poems and sermons and the writings of Franciscan and Dominican mystics and correlating the data with the natural-history data of the old bestiaries and the medieval moralized versions of the classic sources, such as Pliny, Plutarch, Aelian, and Aristotle. It meant investigating the history of the plague for possible mystic usages. And the thoroughness with which Dr. Friedmann pursued his task is indicated throughout the book: by the succinct and well-organized way he has presented his material, the adequate annotations, and the extensive bibliography. There is a classified list of the illustrations as well as a complete list of paintings (arranged by schools) that have been found containing the goldfinch symbol.

The first part of the book is devoted to a general treatment of the symbolism of the goldfinch and to an analysis of the goldfinch in non-Italian art; the last part deals with the goldfinch in Italian art, the majority (about 90 percent) of devotional pictures containing goldfinches being of Italian origin. The Italian material falls into six groups, treated in detail: the Florentine school (Ambrogio di Baldese, Agnolo and Taddeo Gaddi, Daddi, Gozzoli,

etc.); the Sienese school (Taddeo di Bartolo, Fungai, Barna, Sano di Pietro, Benvenuto di Giovanni, etc.); the Venetian school (Giambono, Crivelli, Bellini, Solario, etc.) the Umbrian school (Raphael, Nuzi, Fiorenzo di Lorenzo, etc.); the schools of Milan, Bologna, and Ferrara; and the minor schools of Italy. In conclusion there are chapters on the iconographic and stylistic treatment of the goldfinch and on Michelangelo's unique reversional treatment of the bird in his marble tondo relief "Madonna and Child with the Young St. John."

Although this is a book primarily for the historian and student of art, it represents an excursion into scholarly fields that many others will appreciate. Touching upon such a wide variety of human interests (medicine, mythology, birdlore, painting, religion), it becomes more than an esoteric exercise; it forms an interesting footnote to the far-flung history of man.

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THE ANATOMY OF ERROR

The Natural History of Nonsense. Bergen Evans. ix + 275 + x pp. $3.00. Alfred A. Knopf. New York. 1946.

THIS is debunking at its classic best, a brainful of information about misinformation, written with a grand sense of humor, sometimes sly, sometimes rich, and sometimes with a sarcasm that nips.

The author is not any too convincing about the presence or absence of Adam's navel, with which he starts his discourse, but he is positive that the earth does not have four corners, though even he probably could not have convinced Voliva of Zion City, who had sailed around the world and knew.

Many other topics are introduced: the Arctic, which, at least in part, is not the desolation most people believe, but a delightful place in which to live; fish, which are never dropped by rain; and carrier pigeons, which have to be trained to find their homes. Instinct is discussed: there is a chapter on "birds in their little nests;" and several pages on pet dogs and wild wolves and the "spontaneous generation" of honeybees in the carcass of a bull.

Throughout the volume Evans treats fanciful beliefs and warped facts in a most entertaining manner and, by undermining untruths, gives a great deal of information on a variety of subjects, among them anatomy, physiology, and even sociology. He has done a great deal of research attempting to track down some foundation for legends which recur in various parts of the world—such as the rearing of human children by wolves. In this particular instance he can find no reputable authority for such an occurrence.

The footnotes constitute an interesting bibliography and are worth reading. In stating that the gorilla, contrary to general belief, has no hair on its chest, the author cites his authorities as follows: "See Robert M. Yerkes and Ada W. Yerkes: *The Great Apes*. . . . See Carl Akely: *In Brightest Africa*. . . . See a gorilla."

Also, your human form divine is not so divine after all; instead of on four legs, as was intended, you walk erect, and some of the inner parts therefore do not fit so well.

The author is a trifle doubtful about Noah and questions the accuracy of some of Ernest Thompson Seton's observations, but the book is a good one, well written, informative, and entertaining.

I wish everyone would read it, but some people will not; and I am sure that tomorrow or next week they will telephone me, asking: "To decide a bet, Do snakes sting with their tails?" Or, "Do snakes swallow their young?"

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TWO CASE REPORTS

New Aspects of John and William Hunter. Jane M. Oppenheimer. xvii + 188 pp. Illus. $6.00. Henry Schuman. 1946.

The Endeavour of Jean Fernel. Sir Charles Sherrington. x + 223 pp. Illus. $3.50. Cambridge Univ. Press and Macmillan. New York and London. 1946.

MISS OPPENHEIMER's two delightful essays on John and William Hunter are examples of the pseudopsychoanalytic approach to historical personalities. The first essay concerns itself chiefly with Everard Home, the unfortunate brother-in-law of John Hunter who for reasons best known to himself burned Hunter's voluminous notes some thirty years after Hunter's death, apparently after using them for his own publications. The second essay is an evaluation of the personality of William Hunter as revealed by his contacts and friendships with some of his prominent contemporaries. Under Miss Oppenheimer's essentially light touch and sympathetic feminine conclusions, the main characters emerge as fallible but lovable human beings, a no mean accomplishment when dealing with the truculent Hunter brothers. Even Home is described as "not so much wicked as he was unwise;" the drama of the situation would make it suitable material for a successful play with the currently popular ingredients of complex interfamily relationships and explorations into the dark corners of the human mind. The book is an achievement in the art of typography and bookmaking by The Southworth-Anthoensen Press.

Sir Charles Sherrington's definitive work on Jean Fernel, the outstanding French physician of the sixteenth century, is an example of the more usual scholarly work in the history of medicine. It is replete with learned footnotes and references to original sources, reproductions of original title pages, and sixty pages of appendices. It presents a vivid, thoughtful picture of medical practice and of medical art during the Renaissance. In contrast with the eighteenth century, when the experimental method had permeated medicine sufficiently to enable the Hunters to add surgery and obstetrics to science, the chief accomplishment of the medical leaders of the sixteenth century was to throw off the dead hand of scholastic systems and dogma. Fernel eschewed many superstitions, added new observations, and recognized the deficiencies of his knowledge. In that way he helped to clear the path for the next great step in medicine, the coupling of observation with experiment.

The two books provided two entertaining and instructive evenings, not only because of context and the periods touched upon, but because they inevitably led to further musings on the history of man.

The importance of history cannot be overestimated because history is the case report on the human species, whose physical and psychological health is not encouragingly demonstrated by its two attempts to commit suicide within the past three decades. Improvement does not seem likely unless, as the first step, the motives and the actions of the species are evaluated through careful perusal of the record.

In the field of the political-social history of man, there is almost no outdated material. Plato's *Republic*, Aristotle's *Politics*, Machiavelli's *Prince* have as much applicability and direct interest today as a current *New York Times*. This cannot be said of medical history. There are but few pages of Fernel's writings that have any application to the world of today. The truth of the matter is, I presume, that this demonstrates the sad discrepancy between man's material accomplishments and his advances in the more essential political and social areas. Whereas medical thought of the sixteenth century is

as dead as a dodo, the fundamental problems of politics and of sociology are almost as unresolved today as they were in the days of the Grecian city-states.

The physician of the Renaissance confronted by an epidemic must have been as helpless as is the modern politician face to face with the implications of nuclear fission. The triumphs of modern medicine would have been impossible without the historical transition of medicine through two great phases. The first, as exemplified by Fernel, was the destruction and abrogation of false concepts, pat systems, and verbose theories signifying nothing. The second, a positive period, whose classic example is William Harvey and which includes the Hunters, was the use of the experimental method. Unfortunately, politics and sociology have yet to reach the age of Fernel.

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RECENT MEDICAL PROGRESS

New Worlds in Medicine. Harold Ward, Ed. 707 pp. $5.00. Robert M. McBride. New York. 1946.

THE anthology has become an acceptable channel for the dissemination of information and is, in a way, a reliable indicator of the growth of public interest in a given field. That an anthology of recent progress in medicine performs a valuable service and finds an eager public, goes without saying. Ward's volume acquits itself of its task better than the average because the editor spices it with social needling, although his needles are of a special design. The social implications of medicine are, however, so vital to the future welfare of the nation and mankind in general that it is high time those concerned with the conquest of disease should also devote some attention to the immediate application of medical progress to society at large.

The salient features of this anthology are the broad scope of the medical information included in it and the readability of the selections. After gaining an enlightening bird's-eye view from the editor's introduction, the reader is led directly into a survey of the role of medicine in wars, ancient and modern, an intimate analysis of the unique problems of aviation medicine, the uses made of blood or plasma on the battlefield and far behind the front lines, and the general procedures of medical attention in modern warfare. As was only to be expected, the complexities of modern military medicine have kept pace with the general growth in complexity of modern warmaking. The excerpts are from high authorities and thoroughly lucid in style and content.

A considerable portion of the book deals with summaries of what many readers might consider by now well-established aspects of medical lore, such as the services of pathology in diagnoses, the value of medical physics and chemotherapy, the nature of the virus and bacteria, the scourge of epidemics and the strategy employed in their conquest or control, the nature of immunity, a survey of hormones, and the disorders covered by the term cancer. These sections do contribute to completeness and contain besides much recently acquired information.

Less well known to the public will be the sections on old age. It is now widely known that the percentage of older people in our population is constantly on the increase, and this group therefore well deserves the attention it has recently been receiving. Nutrition also comes in for its due recognition; particularly refreshing is the essay by A. J. Carlson, which is a just challenge to much thoughtless and fashionable ballyhoo. Subsequent selections deal with mental diseases

and some basic problems and theories of modern psychiatry.

The concluding three essays deal with the social implications of medical progress, which means with the question: How is all the preceding information to be best put to use by and for mankind as a whole? To the editor this part should have been the most challenging since its problems are as yet unformulated and judgments should still be in the creative or ripening stages. By right the reader should be aided by them to formulate an intelligent point of view on socialized medicine and related topics. The best service the anthologist can render is to stir up thoughts along new lines, supply fuel, and perhaps also direction.

It may well be claimed that this part is the weakest of the entire volume. The reader is not left with sufficient illumination to see the problem in its entirety. The obstacles to socialized medicine in our society and the arguments, right or wrong, of the medical profession, are slurred over. All he is offered is David Riesman's excellent essay on the need of preventive medicine, which is granted by all and sundry; B. J. Stern's article on Medicine and Society, in which it is elaborately proved with the aid of poignant historical and contemporary evidence, that medicine affects social welfare; and, finally, H. E. Sigerist's enthusiastic summary of the presumably admirable state of medical practice and health security in Soviet Russia. Recognizing the fact that an anthology is in some ways an individual creation reflecting the editor's taste and personality, one may still offer the humble comment that the reader can get little enlightenment from these essays on the none-too-easy problem of socialized medicine in the United States here and now.

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DENTAL PHASES OF PUBLIC HEALTH

Dentistry, An Agency of Health Service. Malcolm Wallace Carr, Ed. xvii + 219 pp. $1.50. Commonwealth Fund. New York. 1946.

THIS book is one of a group of twelve being published as a result of the studies of the New York Academy of Medicine Committee on Medicine and the Changing Order. Its object is to relate dentistry to "national health in a changing order." The separate chapters are the work of individual contributors who are outstanding in their particular fields. The whole has been ably edited and arranged by Dr. Malcolm Wallace Carr.

A brief review of the organization of the dental profession in its Civil, Military, Veterans Administration, and Public Health Service aspects is followed by parts dealing with the history, the present educational situation, the various facets of practice, the status of research, and dental problems in the field of socioeconomics.

Here is presented in one small volume an over-all view of dentistry. It should be helpful alike to the layman interested in its health service aspects, to the physician interested in an allied field, and to the practicing dentist. The last, as much as any other individual of a group, needs such a survey in order that he may not lose sight of the forest because he happens to be surrounded by trees. No dentist can afford to omit this book from his required reading list.

The chapters on dental education, dentistry in rural areas, dentistry for Negroes, and socioeconomics contain much factual information and suggestions for the improvement of present unsatisfactory conditions.

One might wish that the various subjects could have had more nearly equal treatment; however, they do not all possess the

same amount of background material for discussion.

The relation of dental to medical education and the difficulties of complete dental health service for everyone are realistically presented.

There are frank admissions of dental shortcomings in education, in practice, in research, and in ability to date to solve socioeconomic problems. But the remarkable progress of a profession only a few years over a century in age, coupled with the ability to publicly acknowledge its own shortcomings, augers well for continued improvement in all its relationships.

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FOR PUBLIC HEALTH WORKERS

Health Instruction Yearbook, 1946. Oliver E. Byrd. ix + 399 pp. Stanford Univ. Press. $3.00. 1946.

THIS member of the group of yearbooks is of special interest to health workers. With the wide multiplication of medical literature it is increasingly difficult, but equally necessary, for a busy individual to keep up with developments in many fields. Time is limited, and journals are many. The abstractor, then, is a valuable person, whether his work appears in the back of another journal or in a yearly collection like this. The reader owes a debt to the person who selects articles with judgment and summarizes them with skill.

Dr. Byrd, who is Associate Professor of Hygiene at Stanford University, has done his work well. A suggestion or two is ventured. It is arguable whether it is not the province of such a book to question the occasional statement that looks erroneous. That would seem a service that could well be given. Otherwise, original errors are simply handed on. At times, too, the subject matter of this book includes clinical notes that seem to belong rather to the medical or surgical yearbooks. It would be helpful also if the editor covered some of the leading foreign journals, rather than depending on a correspondent in the A.M.A. The amount of space given to world health conditions is intelligent. The high lights at the beginning of each chapter are excellent summaries.

It is a good collection and will be helpful to the health worker who wishes to keep abreast of the times or is looking for recent material in some special field.

H. R. O'BRIEN
Office of International Health Relations
U. S. Public Health Service
Washington

Comments and Criticisms

EXTREMES

Dr. Compton's article, "Science and the Supernatural," in the December issue of The Scientific Monthly, is a beautiful and a brilliant piece of work. I should like to present my humble congratulations to him through you.

DANIEL LUZON MORRIS

Evansville, Ind.

Compton's mistitled article, "Science and the Supernatural," with its disgusting regression to evangelism, belongs in a Baptist Sunday-school paper. It is beneath the notice of the enlightened.

ROBERT C. GIVLER

Tufts College

The foregoing comments are the briefest of those received on "Science and the Supernatural" by Authur H. Compton. They are sufficient to show the extreme divergence of opinion elicited by articles on science and religion. No other comments on Dr. Compton's essay will be published.—ED.

INFORMATION, PLEASE

Although I have been a member of the Association for several years, I am still in a quandary as to how one can obtain more detailed advance information regarding meetings and conferences. As a member of several other scientific societies, I became accustomed to receive regularly the advance announcements of meetings which are being planned. In addition to these announcements many other societies send out bulletins or programs containing all titles of the papers to be presented, frequently accompanied by brief abstracts.

It is my strong belief that the Association has not been making adequate use of its publications for publishing in advance the programs of meetings and conferences. It might serve a very good purpose to have even abstracts of the papers published before or after the meetings. Schrödinger pointed out recently that "We have inherited from our forefathers the keen longing for unified, all-embracing knowledge... but it has become next to impossible for a single mind fully to command more than a small specialized portion..." It might well be that such abstracts of papers dealing with all the multifarious branches of science would help to establish contact between the various sections of the Association. In this manner the Association could successfully counteract the dangerous tendency of overspecialization in present-day science by a very desirable integrating influence.

JOSEPH M. LAMBERT

Easton, Pa.

Prior to the Boston meeting of 1946 the A.A.A S. anticipated the suggestions made by Dr. Lambert. All publications of the Association carried preliminary announcements, particularly Science *and the* A.A A S. Bulletin. *In the November issue of the* Bulletin *it was stated that the* General Program *of the meeting would be ready for distribution about December 1. Although its completion was unavoidably delayed, it was delivered before the meeting to those who requested it. Thus, except for abstracts of papers to be presented, full information about the Boston meeting was made available in advance.*—ED.

THE GOLDEN RULE

Dr. T. V. Smith uses good logic in pointing out that the golden rule has, in reality, a highly self-centered, arbitrary implication. However, this implication can be avoided by rewording the rule as follows "Suggest unto others as you would have others suggest unto you."

A heavy traffic of dissimilar points of view is both robust and democratic. To restrict the area of discussion as Jefferson chose to do, often retards the intellectual growth of the person who makes the denial. The person who is applying for clarification and the interested public are also losers as a result of such a negative attitude. I can see no benefit in excluding any problem, religious or otherwise, from a value-seeking discussion. Even though the questions are asked with malicious intent, the answers can be so devised as to guide the discussion back into civil and constructive channels. Christ himself, when confronted by the malicious questioning of wily lawyers, did not side-step the issue but instead countered with ingenious replies which are classics of adroit technique.

VICTOR HOLTAN

San Francisco

The Brownstone Tower

NOT long ago I had lunch at the Cosmos Club with the Assistant Secretary of the Smithsonian Institution who, like myself, was once an entomologist engaged in research. We fell to talking about the good old days when we were young. Then every day was an adventure. We asked questions, devised means to get the answers, and sometimes succeeded in solving our problems. The days were never long enough to do the things we wanted to do. We got tremendous satisfaction from our small discoveries, for we felt that we were adding something permanent to the structure of our science. In short, we had the enthusiasm and idealism of youth and, for a while, the opportunity to express in personal research that longing to understand and conquer some of the secrets of nature. But the paralysis of paper work crept upon us until my friend became an administrator and I an editor. The truly great scientists are those who retain the enthusiasm of youth throughout their lives and have the capacity to add other activities to their personal research.

Nowadays much thought and effort are being devoted to the discovery of young people who have the talent and desire to become scientists. Talent can be detected by performance tests, but intensity and persistence of desire are difficult to evaluate. How can one tell whether a brilliant young scientist will or will not succumb to the lure of a higher administrative salary? He may move up into a job that needs to be done and that he can do well, but his research may bear little fruit thereafter.

These were some of the thoughts that occurred to me as I read the essays of the winners of the Sixth Annual Science Talent Search. Each writer, all under eighteen, described his or her personal scientific investigations. I decided to publish two of these essays in this issue, not only to demonstrate the productiveness of very young scientists, but to show older scientists how to write more effectively for the SM. These young people have not yet learned to write artificially in the conventional third person, passive voice. They bring their science to life and make of it a vital and thrilling personal adventure. No professional scientist, I think, can read these essays without a feeling of nostalgia for the days when unlimited time and unlimited problems seemed to lie ahead.

It is all to the good that the Science Clubs of America, sponsored by Science Service, and the Junior Academies of many states (see the article by J. W. Thomson, Jr., in this issue) are encouraging the early expression of scientific talent among high-school students. Their lives are being immediately enriched by their scientific pursuits, and they may later contribute much to the philosophical and material welfare of the world. The science teachers in many high schools must also receive much credit for making possible the splendid achievements of their best students. How this is being done in one high school is described by Dr. Paul F. Brandwein in the March SM.

Many of the high-school students who have participated in the Annual Science Talent Search are certainly worthy of membership in the A.A.A.S. and are capable of understanding the SM. Yet, as I pointed out in the February issue, not one reader of the SM in my sample of 1,000 was under twenty. Cannot we older scientists give these promising youngsters additional help and encouragement by making them SM members of the A.A.A.S.?

F. L. CAMPBELL

THE SCIENTIFIC MONTHLY

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MEMORIAL LABORATORY, NATIONAL INSTITUTE OF HEALTH

By CHARLES ARMSTRONG

Division of Infectious Diseases, National Institute of Health

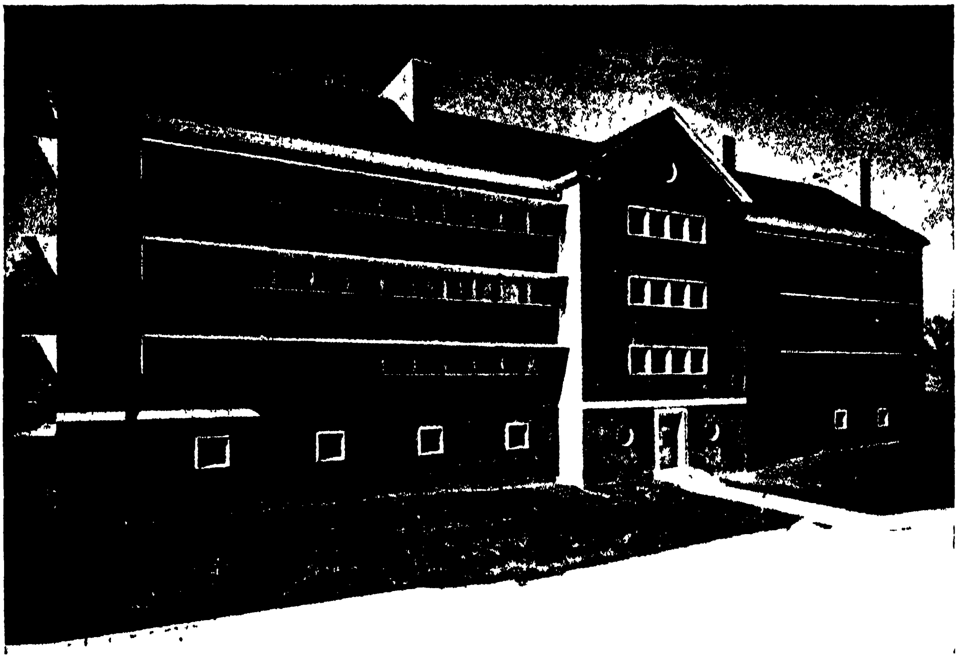
COMPLETE protection of research workers in infectious diseases has long been an unrealized hope of medical science. Each year many valuable men and women contract disease in the course of their work. Most laboratories take certain protective measures, to be sure, or the list of victims would be much larger that it is today. The value of most protective measures, however, has largely depended upon how thoroughly they were observed by the laboratory workers themselves.

The Memorial Laboratory for the study of infectious diseases at the National Institute of Health, formally dedicated on October 27, 1946, is the result of an intensive effort on the part of the United States Public Health Service to provide a safe environment for research personnel. It is designed and equipped to control and contain infections at their source, thereby affording greater protection for every individual in the laboratory.

The need for a specially designed laboratory such as this becomes readily apparent when we review the fifty-nine-year history of the National Institute of Health. At least two of our men have died of laboratory-contracted diseases during each decade. The present one, not yet completed, has already taken four. We have not only lost our friends and co-workers, but medical science has also lost the fruits of their years of experience.

There is another large group of our workers who have contracted diseases in the laboratory but have fortunately recovered. Civil service employees at the Institute enjoy no unusual privileges because of the risks of their employment. Those who have contracted diseases as a result of their work in the laboratory have lost sick and annual leave. If the illness was prolonged beyond their accumulated leave time, they were remunerated under the United States Employees Compensation Act at a lower income rate than they would have received on active duty. Still these people returned faithfully to work under conditions that previously had been a contributing factor to their disability.

Medical science is the loser in other ways, too. At times we have felt that certain projects—vital though they were—should be suspended because of the great danger involved. For instance, while we were working on "Q" fever in 1940, 16 cases of



USPHS Photo

THE NEW MEMORIAL LABORATORY, NATIONAL INSTITUTE OF HEALTH

THIS BUILDING, LOCATED IN BETHESDA, MD., IS UNIQUELY EQUIPPED TO PROTECT RESEARCH WORKERS FROM THE DEADLY EFFECTS OF INFECTIOUS DISEASES THAT WILL BE STUDIED. IT WAS DEDICATED OCTOBER 27, 1946, IN HONOR OF THOSE EMPLOYEES OF THE U S PUBLIC HEALTH SERVICE WHO LOST THEIR LIVES IN LINE OF DUTY AFTER HAVING CONTRACTED THE INFECTIOUS DISEASES THEY WERE STUDYING.

the disease suddenly broke out among the men in the laboratory. One died. We then stopped work on this particular disease.

Five years later, after we had DDT'd the building thoroughly to control insects, work on Q fever was resumed. Within less than two months, another outbreak occurred—this time 47 cases. Luckily, there were no fatalities. We now believe the disease is airborne. Our new laboratory is so constructed that it is thought that air currents may be controlled and airborne diseases contained within a small area. This time, research on Q fever will go on.

Previous to this outbreak of Q fever, we were badly shaken by events that occurred in the fall of 1944. Three of our employees died within a period of six weeks. Bacteriologist Rose H. Parrott died from a tularemia infection contracted in her laboratory at the National Institute of Health on September 11, 1944. Eighteen days later, Philip L. Jones, Scientific Aide, died of tsutsugamushi at the Rocky Mountain Spotted Fever Laboratory, Hamilton, Mont. Twenty-two days later, Dr. Richard G. Henderson was fatally infected with the same disease here at Bethesda, Md.

The conclusion was obvious. Something more needed to be done in order to give full protection to our workers.

Dr. Thomas Parran, Surgeon General of the United States Public Health Service, immediately moved to obtain funds for a building especially designed to protect persons, engaged in research on infectious diseases. He and Dr. R. E. Dyer, Director of the National Institute of Health, his assistant, Dr. L. F. Badger, and virtually every member of our laboratory staff

contributed something to the final plan. In addition, Dr. Badger and Dr. N. H. Topping made a tour of a number of the new laboratories in the country, searching for ideas related to our problems.

In the main, our concerns were fourfold. First, we wanted to separate research on different diseases. Second, we wanted to control the air flow in and out of every room and working space in the building. Third, we needed heretofore undesigned equipment to protect the worker against infection. Fourth, we needed an easily enforceable set of rules affecting the movements of personnel about the building. We believe that the memorial Laboratory as it now stands provides for the solution of all these problems.

There are six individual research units. Two are located on each of the three floors and are separated by a "clean" (non-contaminated) area housing administrative facilities and personnel. Refuse from each unit is dropped by elevators to incinerators, one at each end of the basement. Separate elevator shafts for each unit preclude any possibility of cross ventilation between laboratories. Refuse cans are sterilized in the basement by steam before being returned for further use.

The air and its flow are under control from the time air enters the building through intakes on the roof until it is drawn off by outlets, also on the roof. Clean areas contain a higher pressure than the contaminated areas so that the drift will always be toward, and not from, the location of infectious materials.

This drift-direction control is the basic concept of our air-conditioning system and is carried out in every room and in every working space. The air enters large perforated ceiling panels at a low velocity, minimizing the possibility of drafts or dust-raising. It is drawn toward the infectious material on the workbenches and is exhausted through a wall slot at the rear of the bench. Circulation is constant and so planned that no contaminated air will be recirculated.

The problem of air control has not only influenced the architectural scheme of the laboratory but has also been a prime consideration in the construction of all the newly designed protective equipment.

Each of the six research units is identically laid out and equipped and may be entered only from a clean area. A double set of doors serves as an air lock between the wings and the central part of the building. Once inside, the worker changes to his distinctive work clothes in a clean dressing room and enters the unit proper. Upon leaving the unit, he reverses the procedure, leaving his work clothes in a contaminated dressing room before donning his other garments in the clean one.

Following the policy of containing the most dangerous threats to health in as small an area as possible, each unit has two laboratories—one slightly smaller than the other—and these in turn are subdivided into special rooms equipped with sterile cubicles and protective cabinets.

The protective cabinets were designed to guard against the spray from high-speed equipment such as tissue grinders or electrical blenders. Safety features in these devices include 30-watt ultraviolet lamps, which sterilize the air in the cabinets, and electric circuits that remain broken unless the entrance window is latched shut, completely enclosing the equipment.

Tissue culture and other bacteriological procedures will be carried on in temperature-controlled sterile cubicles, each about the size of four telephone booths. Incoming air is filtered through spun glass and subjected to ultraviolet irradiation to prevent contamination of the experimental material.

Workbenches are provided with glass hoods that protect the face of the worker

and provide fluorescent illumination, as well as ultraviolet radiation that destroys exposed pathogens. Controls for water, gas, and electricity are installed on the near face of the bench, making it unnecessary to reach over infectious material. An electric grill air exhaust, also present in the protective cabinet and the sterile cubicle, draws the air away from the bench and sterilizes it at temperatures exceeding 500° C. before releasing it through the roof outlet. A can for experimental refuse is inserted into a cutout in the bench. When closed with a tight-fitting cover, it can be safely carried to the autoclave for sterilization before the contents are sent to the incinerator.

Other facilities of each unit include:

1. An autopsy room where infections are transferred and autopsies performed The workbench here is also equipped with hood and electric grill air exhaust
2. One constant high room (with a range from 70°F to 120°F.) and one constant low room (with a range from 10°F to 60°F).
3. One large and two small animal rooms with ceiling-hung cage racks Following the air-drift policy, pressure in these rooms will be lower than that in the rest of the unit to minimize the spread of odors.
4. A storage room, complemented by a much larger one in the basement.
5. A cage washing and sterilizing room containing one large and one small autoclave. The entrance to the elevator shaft is also located here.
6. A water-distillation room equipped with two water stills having a production capacity of 5 liters per hour per machine.
7. A serological laboratory.
8. An office and a library-conference room.

Employees will wear simply designed, white, zippered coveralls in the research units and like garments of blue in the clean areas. Since there is only one exit from the units—through the dressing rooms—there is small possibility that workers will carry infection on their persons.

The new Memorial Laboratory was constructed and equipped at a cost of approximately $1,200,000. Like the seven other buildings at the National Institute of Health, it is constructed of red brick, but its deep-set, triple, thermopane windows and a substantial solar canopy give the new laboratory a rather different appearance.

Research operations will begin in the new structure as soon as the units are completed and fully equipped. One unit will work on Rickettsial diseases—typhus fever, Rocky Mountain spotted fever, Q fever, Rickettsialpox, and the like. Another will be confined to pathogenic molds; a third to psittacosis and related diseases; a fourth to brucellosis; a fifth to poliomyelitis and other central nervous system diseases; and the sixth to the common cold.

Although the last-mentioned is not as dangerously virulent as the others, we believe that the causes of the common cold are perhaps multiple. In this instance, we shall use our protective devices to isolate the material from extraneous infections rather than personnel from the material.

The study of these projects is our program for the immediate future. The schedule is not inflexible, however. As new problems arise in the field of medical research or as old problems gain new significance, we shall shift our attention and our facilities to meet the new need in the shortest possible time.

Whatever the future course of our studies may be, we can now go to work—despite the dangers involved—with a feeling of confidence that we have provided for our people every possible protection within the realm of our present knowledge and experience. We sincerely hope that our honor roll of heroes—to whom this laboratory is dedicated— will not grow longer

THE ONLY KNOWN FISH-CATCHING PLANT: UTRICULARIA, THE BLADDERWORT

By E. W. GUDGER

The American Museum of Natural History, New York

IN HIS unique monograph *The Carnivorous Plants* (1942), Francis E. Lloyd lists (excluding the carnivorous fungi—*Cordyceps*, *Empusa*, etc.) 6 families, including 15 genera and 515 species, of these plants. Since some of these feed on animals other than insects, Lloyd prefers to call them carnivorous rather than insectivorous plants. Among the noninsectivores are the 275 species of the genus *Utricularia*—most of them aquatics. These Utricularias are of world-wide distribution and are ubiquitous in tropical and temperate regions.

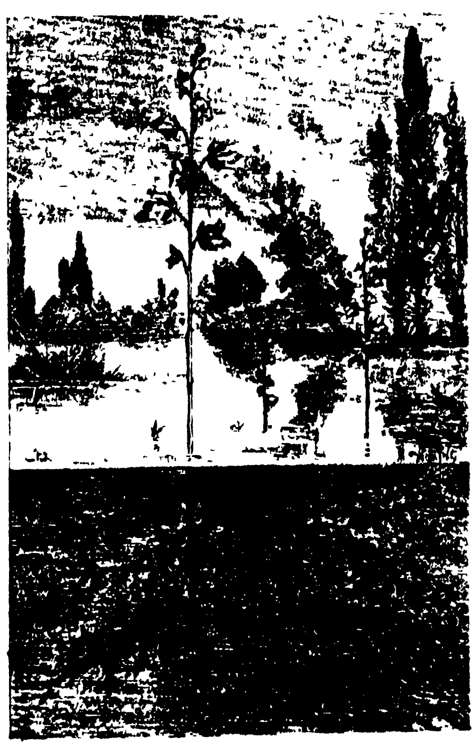
Carnivorous animals capture their prey either by virtue of superior speed or superior cunning. Carnivorous plants, being sedentary, have to wait for their prey to come to them and have developed traps wherewith and wherein to catch their prey. Furthermore, many of them secrete in their traps certain ferments which prepare their catches for digestion. Thus these plants turn the tables on animals, which are directly or indirectly plant-eaters.

Most of these traps are passive—the trap makes no effort or movement to catch the prey. A few kinds have active traps which display special movements in capturing their prey; for example, the "steel trap" of Venus's-flytrap. But the most highly specialized of the carnivorous plants is Utricularia, and its trap is possibly the most elaborate and peculiar of all the traps ever found.

THE FISH-CATCHING PLANT

Most flowering plants are fixed to the earth and restricted to certain food from it—mineral salts dissolved in water—which food they draw up through their roots. The aquatic bladderworts, however, are mostly floating, rootless, annual flowering plants, which at the proper season send up aerial stems with a considerable inflorescence for the production of seeds. Their underwater parts are most interesting. The submersed floating Utricularias of the *vulgaris* type have the widest distribution and are all potential fish-catchers.

The species illustrated in Figure 1 is the commonest and perhaps the most wide-



After Halperine, 1885

FIG 1 *UTRICULARIA VULGARIS*

THIS ROOTLESS, FLOATING BLADDERWORT BEARS AERIAL FLOWERS THAT ARE POLLINATED BY INSECTS. THE BLADDERS APPEAR UNDER WATER.

spread floating form, *Utricularia vulgaris*. As seen in the figure, the underwater parts are the more abundant, branching out loosely over an area several feet in circumference. The most interesting of these underwater parts are the abundant utricles (*L. utriculus*, "a bottle or bladder") which give the genus its name. In these multitudes of bladders, small water dwellers—microscopic plants and animals (unicellular algae, worms, crustaceans, mosquito larvae, infusoria) and even small fishes are entrapped.

That active fishes, vertebrate animals, even though tiny ones, should be entrapped in the bladders of an aquatic flowering plant is certainly turning normal plant-animal relations topsy-turvy. The fish-destroying habits of the common bladderwort are extraordinary in the *n*th degree and to botanists and students of fish predators of very great interest. Hence it seems well worth while to bring together the relatively few accounts (most of them about 60 years old).

Drawings and photographs of the fish-catching plant, made to illustrate various accounts to follow, are all too small to show with any accuracy the details of its structure. Hence, they will be passed over and for an adequate representation of the plant in its habitat, there will be introduced a photographic reproduction of a part of Dr. Roy W. Miner's splendid pond-bottom group in the Darwin Hall of the American Museum of Natural History. Here a cubic half inch of pond bottom is magnified 100 times.

In Figure 2 is shown part of a pond-bottom jungle with a large Utricularia stalk extending diagonally from lower left to upper right. This stem has numerous bladders attached to what are morphologically leaves. Figure 3 portrays (greatly enlarged) the bladder shown in the center of Figure 2. This contains partly decomposed prey—not fishes, however.

DISCOVERY OF THE FISH-CATCHING HABIT

The bladderwort has long been known to be a carnivorous plant, but by various early students it was thought to catch only microscopic aquatic invertebrate animals—small crustaceans, worms, and insect larvae.

That the bladderworts catch fishes was discovered in the spring of 1884 by G. E. Simms, Jr., of Oxford, England. Immediate publication of his discovery led to the appearance of a number of articles on this fish-catching habit in 1884, 1885, and 1890. Since 1890 almost nothing of value has been published. These accounts will be reviewed as nearly as may be in the order of publication. But since the early ones are by two men with dates overlapping, the contributions of each man will be considered in a group, beginning with those of the discoverer (1884. 1) who, so far as the records go, was the first to witness the curious phenomenon under study. Though he was a resident of Oxford, he apparently was not a student at the university. However, he carried his material and his problem to Professor H. N. Moseley in the museum of the university. Moseley became greatly interested and advised him how to deal with his discovery.

Simms's first article was published in the *Fishing Gazette* of May 31, 1884. To this I have not had access, but fortunately it and other early articles on Utricularia did not escape the eye of Professor Spencer F. Baird or of his assistants in the U. S. Fish Commission. This and other early accounts of fish-catching by Utricularia were republished (presumably verbatim) in the Bulletin of the Commission for July 30, 1884. Since Simms's account is the basis for everything that follows in this article, and since in it he has put everything very clearly, it seems best to follow him closely and to quote him extensively. In reading his account, constant reference should be made to the illustrations.

Simms's materials were collected not from active streams but from "still ponds and deep ditches," quiet waters in which Utricularia "is most likely to work mischief to young fry" of fishes. Of the action of this plant he writes as follows:

I have recently discovered amongst the aquatic weeds placed in my aquarium, where I have also a large number of newly-hatched perch and roach, a novel and unexpected enemy to the pisciculturist in the bladder traps of *Utricularia vulgaris*, which is capable of catching and killing young fry

My attention was first drawn to it by observing

FIG 2 A POND-BOTTOM JUNGLE SHOWING BLADDER-TRAPS

A PHOTOGRAPH OF ROY W MINER'S POND LIFE GROUP IN THE AMERICAN MUSEUM OF NATURAL HISTORY.

that several of the tiny fish, without any apparent cause, were lying dead on the weeds, while the rest of the brood looked perfectly healthy and in good condition At first I was somewhat puzzled at the strange position in which they were lying, and in trying to move one with a small twig I was still more surprised to find it was held fast by the head, in what I thought, when I pulled the plant from the water, were the seed vessels; and a still closer examination revealed the strange fact that others of the little fish had been trapped by the tail, and in one or two instances the head and tail of the same fish had been swallowed by adjacent bladders, thus forming with its body a connecting bar between the two

At first I was undecided how to act, for I could

FIG. 3. A MAGNIFIED BLADDER-TRAP OF *UTRICULARIA VULGARIS*

DETAIL FROM THE CENTER OF THE GROUP SHOWN IN FIGURE 2. THIS BLADDER DOES NOT CONTAIN FISH.

bring to memory no instance in which I had seen the existence of a piscivorous plant—i e , one preying on vertebrates—recorded in any book I had ever read, and I was unwilling to make such an assertion without the opinion of some one better capable of forming a judgment on the subject than myself; so I placed one or two good specimens in a glass jar and went to the Museum, where I was fortunate enough to see Professor Moseley, who immediately verified my suspicions.

Moseley brought out a copy of Bentham's *British Flowering Plants* and showed Simms that his plant was the bladderwort, *Utricularia vulgaris*. With help from this work and Darwin's *Insectivorous Plants*, Simms sets forth his observations in the proper botanical setting as follows:

A peculiar fact in connection with it [this plant] is that it has no roots at any time of its life, and the floating, root-like branches, which are covered with numerous capillary and much divided leaves, are interspersed with tiny green vesicles, which were supposed by a former school of botanists to be filled with water, by which means the plant was kept at the bottom until the time of flowering, when the water gave place to air, and the plant then rose to the surface to allow its bloom to expand.

As a matter of fact, these vesicles exercised no such function, their real work being to entrap minute crustaceans, worms, larvae, etc , for its support, and without a good supply of which it is impossible to keep it alive in an aquarium

Their form is that of a flattened ovoid sac, or, in other words, when seen under a low-power microscope, they are precisely like a human stomach, and they are attached at their hinder extremities each by a very short and fine pedicle or foot-stalk in the axil of the leaves

Each, too, has an opening at the opposite free extremity, somewhat quadrangular in outline, from either side of which project two branched processes, called by Mr. Darwin antennae. . . .

On either side of the quadrangular entrance several long bristles project outwards, and these bristles, together with the branches of the antennae, form a sort of hollow cone surrounding the entrance, and there cannot be the slightest doubt that they act as a guide for the prey.

The entrance is closed by a valve, which, being attached above, slopes into the cavity of the bladder, and is attached to it on all sides except at its posterior or lower margin, which is free, and forms one side of the slit-like opening leading into the bladder. . . .The valve is colorless and transparent, and is extremely flexible and elastic.

Animals enter the bladders by bending inwards the posterior [or lower] free edge of the valve, which from being highly elastic, shuts again immediately.

The edge is extremely thin and fits closely against the edge of the collar, both projecting into the bladder, and it is extremely difficult, if not impossible, for any animal to escape, although I have observed a long worm do so at the expense of a part of its body; yet, as a rule, it is a case of "all who enter here lose hope". .

When a fish is caught, the head is usually pushed as far into the bladder as possible till the snout touches the hinder wall. The two black eyes of the fish then show out conspicuously through the wall of the bladder.

So far as is known, there is no digestive process in Utricularia; neither is there any sensibility to irritation. Mr. Darwin was unable to detect either, his opinion being that whatever nutriment the plant obtained from its prey was by absorption of the decaying matter, and it would appear that the longer of the two pairs of projections composing the quadrifid processes by which the vesicles are lined, which project obliquely inwards and towards the end of the bladder, acts, together with the spring valves at the mouth of the bladder, in utilizing each fresh struggle of the captive for the purpose of pushing it further inwards . . .

Of its destructive powers all I can say is, that out of 150 newly-hatched perch [roach?] placed in a glass vessel only one or two were alive two days subsequently, and I hope in a few days to be in a position to speak of its powers in nature

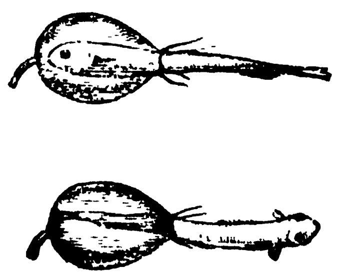
Unfortunately Simms did not publish any figures to illustrate his observations. This is probably due to the fact that just at this time he became ill. Such figures are, however, to be found in his second paper (1884.2).

Simms unfortunately does not give the date when his first observations were made. Probably he saw the fish-catching early in May since he first published May 31, 1884. But shortly after publication of his first report (1884.1), he was probably advised that he should also publish an account in a scientific journal. This was evidently delayed by his illness and it was not until July 24, 1884, that an article by him appeared in *Nature*. This contains little

matter additional to his first article, and it is not necessary to do more than call attention to various particulars therein.

He notes that, while many plants prey on various invertebrates, his discovery of the fish-catching habits of an aquatic Utricularia made known for the first time that such a plant entraps the young of a vertebrate animal. However, this might have been expected of the bladderwort since it catches various water-dwelling invertebrates, crustacea, worms, etc. Then, since Utricularia grows in quiet shady ditches and ponds where "coarse fish" lay their eggs, this baby fish-catching might be expected there. Experiment shows that the plant does not do well unless well shaded.

Simms's second paper (1884.2) is illustrated by a figure of the underwater part of the plant. This is not nearly so good as that shown in Figure 1 and will be omitted. But he does have two figures of little fishes caught by bladders -one by the head and the other by the tail. These (Fig. 4) are the first-known portrayals of this extraordinary phenomenon.



After Simms, 1884

FIG 4. FISHES CAUGHT BY BLADDERS

THESE EARLIEST FIGURES OF THE PHENOMENON SHOW TWO FISHES CAPTURED BY HEAD AND TAIL

OTHER CONTEMPORARY ACCOUNTS

Simms had hoped to publish other observations on this fish-catching phenomenon. Probably his illness and the appearance of a short article by Moseley made this superfluous.

One week after Simms showed his plant and captured fishlets to Moseley, and one week before Simms's account appeared in the *Fishing Gazette*, Moseley anticipated him by publishing in *Nature* (May 22, 1884) a short article entitled "A Carnivorous Plant Preying on Vertebrata." This account was also republished verbatim in the Bulletin of the U. S. Fish Commission for July 30, 1884. Moseley states that his article in *Nature* was published with Simms's permission. But, to say the least, it is unfortunate that Moseley did not delay his article a few days until Simms, the discoverer, could publish first and have absolute priority.

Moseley refers (1884.1) to Simms's "find" as follows:

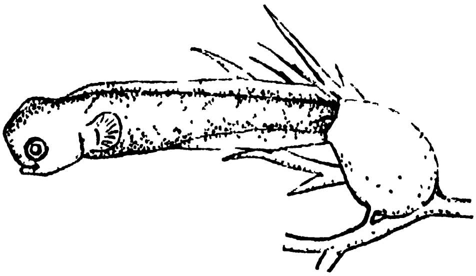
> An interesting discovery has been made during the last week by Mr G E Simms of Oxford. It is that the bladder-traps of *Utricularia vulgaris* are capable of catching newly-hatched fish and killing them Mr. Simms brought to me for examination a specimen of Utricularia in a glass vessel, in which were numerous young roach newly hatched from a mass of spawn lying at the bottom Numbers of these young fish were seen dead, held fast in the jaws of the bladder-traps of the plant. . . . Mr. Simms supplied me with a fresh specimen of Utricularia in a vessel with fresh young fish and spawn, and, in about six hours, more than a dozen of the fish were found entrapped. Most are caught by the head, and when this is the case the head is usually pushed as far into the bladder as possible till the snout touches its hinder wall. The two dark black eyes [sic] of the fish then show out conspicuously through the wall of the bladder. Rarely a specimen is seen caught only by the tip of the snout. By no means a few of the fish are, however, captured by the tail, which is swallowed, so to speak, to a greater or less distance, and I have one specimen in which the fish is caught by the yolk-sac Three or four instances were observed in which a fish had its head swallowed by one bladder-trap and its tail by another adjacent one, the body of the fish forming a connecting bar between the two bladders.
>
> I have not been able to see a fish in the actual process of being trapped, nor to find one recently caught, and showing by motion of the forepart of its body signs of life. All those trapped were found already dead, but I have had no opportunity of prolonged observation.

It was not clear to Moseley just how a little fish is so fully swallowed, but he thought that the "quadrifid processes" lining the bladder and projecting "inwards and towards the posterior end of the bladder" had something to do with it. They and the spring valve of the bladder must work together to this end. However, on cutting open bladders which had caught fish, he found these latter badly decomposed but without any suggestion of digestion. In this he corroborated Darwin's finding. And Simms in his article of May 31, 1884, distinctly says that, so far as he could ascertain, there was no digestion of the fish by the plant.

On June 20, 1884, Moseley dispatched a letter and some specimens in preservative to Professor S. F. Baird, U. S. Commissioner of Fish and Fisheries. Baird had some of the Utricularia material drawn under the direction of John A. Ryder. On July 30, 1884, Moseley's letter was published under the title "The Fish-eating Plant Utricularia or Bladderwort," and the drawings referred to appeared as a plate. He nowhere calls the bladderwort a fisheater in this letter, nor in that copied from *Nature*, May 22, 1884, published just ahead of the present letter. The editor of the Bulletin made a bad slip in his title -- "The Fish-eating Plant, Utricularia or Bladderwort."

The figures drawn under Ryder's supervision are four. One showing a young fish caught by the tail in a bladder is reproduced as Figure 5. Note the great disparity in size between the fish and the bladder.

Moseley in his letter to Baird stated that he had not been able to make any observations additional to those already published since the supply of the particular fishlets that get caught had been reduced to nil and since his few bladderworts had seemingly become inert. However, Moseley also stated that he intended to bring specimens and to make a report on this phenomenon at the meeting of the British Association for the Advancement of Science at Montreal later in the year. The Report for the Montreal meeting reveals that Moseley spoke before Section D (Biology) on August 28, 1884. The published abstract (1885) contains nothing not noted above.



After Ryder for Moseley, 1884

FIG. 5. ANOTHER FISH CAUGHT

In the July 11 issue of *La Nature* (Paris, 1885) appeared "Plantes Piscivores," by E. Halperine. This was translated and republished in the Bulletin of the U. S. Fish Commission for August 21, 1885. The writer is an enigma. None of the biographical dictionaries at hand contain his name.

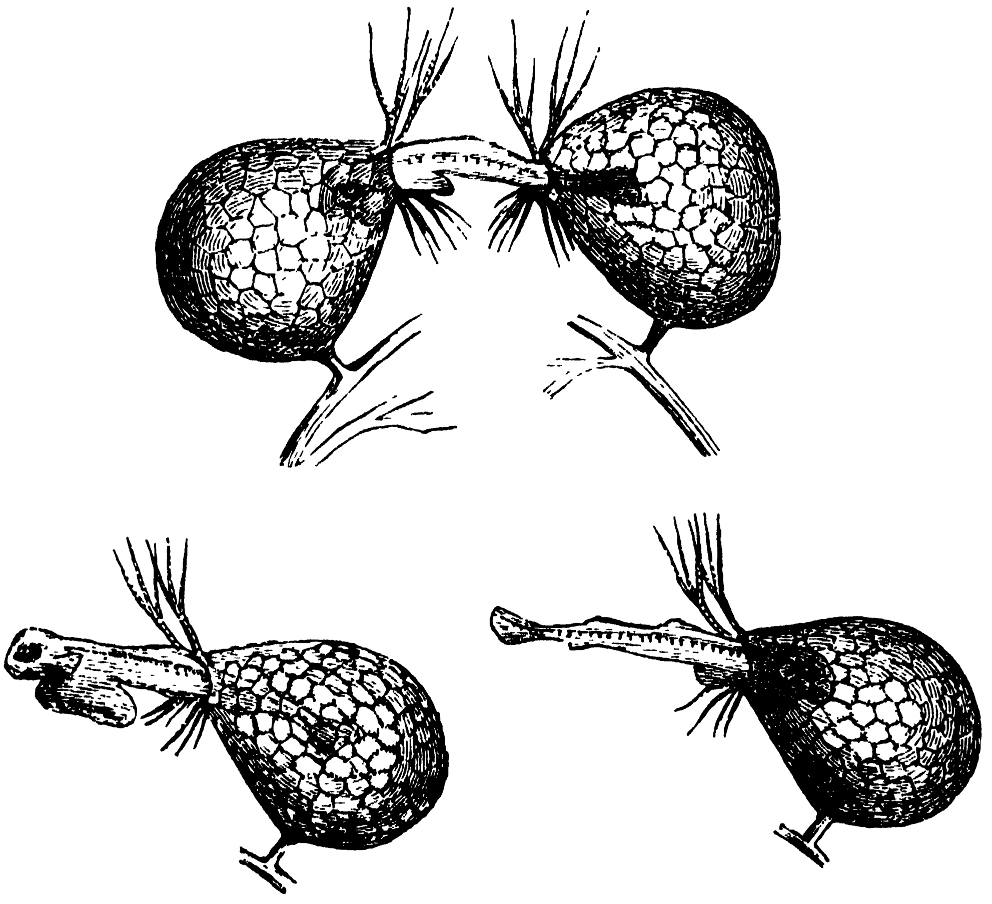
In this article there are various footnote references to writers on "Plantes Insectivores," and there is a brief reference to the observations of Simms and Moseley, but no citations to their publications. It seems probable that this writer knew Utricularia at firsthand. The figure of the plant at the flowering season is the best yet found (Fig. 1). The second figure shows three larval fishes, still in the yolk-sac stage, caught in the bladders—No. 1 by the head, No. 2 by the tail, No. 3 with the head caught by one bladder and the tail by another (Fig. 6). Attention is called to the relative sizes of fishlets and bladders. These are by far the best figures yet found of trapped fishes.

The last of Halperine's drawings shows a small fish completely swallowed and curled

up in the interior of a bladder (Fig. 7). As heretofore attention is called to the relative sizes of fish and bladder. It should be expressly noted that no other student of this plant-fish phenomenon has alleged a complete swallowing of a fish, though there is no reason why a very tiny one could not be so engulfed.

There is nothing to tell us whether author and artist knew the plant and its fish-catching at firsthand, whether their drawings are based on Simms's figure, or whether they are drawn *de novo*. They are different from Simms's figure and are much better drawn. There are no captions. These figures are certainly authentic for the phenomenon and are reproduced for what they show.

Dr. Bashford Dean published in 1890 his second scientific paper and his first one having to do with fishes. It is entitled "Report on the Supposed Fish-eating Plant [Utricularia]." He knew of the studies of Simms and of Moseley and was incited by them to make personal investigations. He studied his plants not as specimens kept in aquaria, but as fresh material from plentiful supplies found in the ponds around the laboratory and hatchery at Cold Spring Harbor, Long Island, N. Y. He spent much time during the months of July, August, and September 1889 in studying



After Halperine, 1885

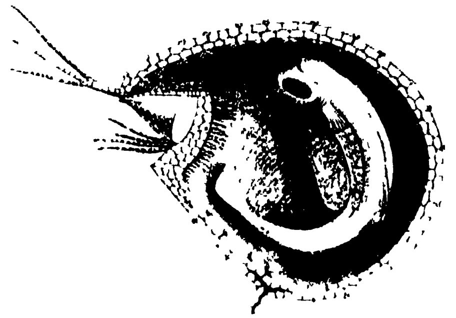
FIGURE 6. A DOUBLE CAPTURE AND TWO SINGLES

the contents of the bladders and must have opened scores, if not hundreds, of these. He is the first and, so far as I know, the only investigator to analyze and to give percentage records of the organisms found in the utricles.

He found that remains of plants, (diatoms, desmids, and the zygospores and oospores of filamentous algae) made up 87 percent of the food remains; animals, living and dead (crustaceans, worms, rotifers, paramecia, tardigrades, etc.), made up the remaining 13 percent. He experimented by giving the bladders living minute crustaceans and found that they were readily taken, that many of them would live for some time in the bladders, and that, when they died, decomposition took place very slowly. Dean judged that "the plant, therefore, possesses no pawpaw-like ferment."

From this mass of evidence, Dean concluded that while the bladderwort catches minute animals, it cannot be considered as very destructive to the young fishes of the pisciculturist since he found no fishes caught in the bladders of his wild plants. However, he concedes that in aquaria with minute fishes and plants crowded together many captures might result. Thus he explained Moseley's observation that nearly all the little fish in a small aquarium were caught and killed. However, in the open, where carp and related coarse fishes may deposit their spawn on aquatic plants, including bladderworts, the hatched fishlets when very small and encumbered with their yolk sacs might be caught if with head or tail they touched and sprung the valves of the bladders. These captures in the open, Dean thought, would be relatively infrequent. At this point he quotes a letter from Professor Minot, of Harvard, that he had found captures by Utricularia "exceedingly rare."

The figures which Dean drew in natural size are reproduced in natural size as Figure



After Halperine, 1885

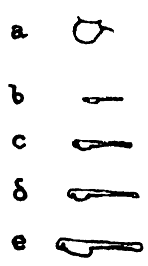
FIG. 7. A FISH ENGULFED

NO SUCH SWALLOWING, HOWEVER, HAS BEEN REPORTED OR FIGURED BY ANY OTHER OBSERVER.

8 and are especially valuable. The bladder, *a*, is 3.5 mm. ($\frac{9}{64}$ in.) long—outside measurement. The opening of the mouth (at top in the vestibule) is 2+ mm. (*c.* $\frac{8}{32}$ in.) wide. Of the fishes shown in actual size at hatching: *b* is a goldfish 5.5 mm. ($\frac{13}{64}$ in.) long; *c* is a carp 8 mm. ($\frac{5}{16}$ in.) long; *d* is a black bass 9 mm. ($\frac{6}{16}$ in.) long; and *e* is a brook trout 11 mm. ($\frac{7}{16}$ in.) long.

After studying the drawing of the bladder and its mouth and these figures of the fishes and contrasting the relative sizes of all, it seems well to quote Dean's own words (p. 191) regarding the capture of fishes by the bladders:

> The entrapment of newly hatched fish is... abnormal, and appears to occur only when the fish



After Dean, 1890

FIG. 8. RELATIVE SIZES

NATURAL SIZE OF A BLADDER AND FOUR NEWLY HATCHED FISHLETS. OF THESE *b* IS A GOLDFISH; *c*, A CARP, *d*, A BLACK BASS; *e*, A BROOK TROUT.

at time of hatching are exceedingly minute... It seems that only during the first few days after hatching the young fish may be secured, since it is then that the fish resting near the bladders, rendered helpless by the yolk-sac, may by a sudden movement find its way to the valve

In these natural-size portrayals and in his own words just quoted, Dean has given a criterion for studying certain captures previously reported or to follow. It should be noted that he states that the utricles of *A. vulgaris,* which he studied, are among the largest known.

To complete the record, there will now briefly be noted various incidental accounts of reported catchings of fishlets by Utricularia.

Walter in 1894 discussed at length the capture by Utricularia of many forms of fish food, and then gave the following personal observation:

On finding Utricularia, I made an investigation with young 14-day old carp, which were fished out of a breeding pond In the evening I put a number of these carp fry in a cylindrical vessel 22 cm. high and 14 cm [about 5 6 in] in diameter with ten small pieces of Utricularia of about 15 cm in length, which only covered the space on the surface. The next morning, I was astounded to see that about ten fish had been captured by the bladders

This account leaves much to be desired. If Walter had been as meticulous in describing the size and activity of these "14-day old" carplings and in stating whether or not the yolk sac had been absorbed, as he was in giving the unessential dimensions of the jar in which they and the plant were contained, his account would have much more value. Furthermore, how did he know that the fishes were fourteen days old? Dean's figure of a baby carp (8 *c*) in natural size at the time of hatching is 8 mm. long. A fourteen-day old carp (i.e., 14 days after hatching) must be at least twice as long (16 mm., $\frac{11}{16}$ in.), or three times larger, and proportionately bulkier. How then could a bladder 3 mm.–5 mm. long have caught and held so large a fish?

Walter in 1899 published a book dealing with the enemies of baby fishes. After discussing the normal food of Utricularia he states.

Small carp fry are caught by the bladders, and indeed either by the head or by the tail, and as Fig 2*b* shows are held fast until they disintegrate Trout fry because of their size have nothing to fear from the bladders, but the latter can be damaging to carp fry, even though these soon outgrow the power of the enemy to harm them.

Walter's Figure 2*b* is evidently the product of the unrestrained imagination of the artist. As drawn the bladder is about 23 mm. long. The fish has the tail curled around *outside* the bladder from front to back. Using a thread to follow the curve of the fish's body, I find that in this drawing the fish is about 85 mm. long. It should be noted that the end of the fish's body as drawn is shaded into the drawing of the bladder. Hence it is impossible to get the full length of the fish. Because of its size this fish surely had "nothing to fear from the bladder."

E. E. Green, of Ceylon (no locality noted), sent a letter to the Quekett Microscopical Club (read February 20, 1903) in which he stated:

I have had no experience of the English species of *Utricularia;* but it may be of interest to note that a small aquatic species of the plant in Ceylon can catch and hold young fish in the way described. I have had experience of this fact in my own aquarium, in which I had a species of Utricularia bearing bladders scarce one-sixteenth of an inch in longest diameter On several occasions I observed young fish (about one inch in length) caught and firmly held by their tails in these little traps [of the size of a pin head]

This statement raises a grave question: Can a bladder one-sixteenth of an inch long hold by the tail an active fish sixteen times its length? Here again we must go to Dean's figures of a bladder and certain just

hatched fishes shown in natural size. Here (Fig. 8*a*) are seen in natural size a bladder ($\frac{9}{64}$ in. long) and a little fish (*e*, a brook trout at hatching, $\frac{7}{16}$ in. long). From this can be judged the difficulty a bladder one-sixteenth of an inch long would have in holding an active fish sixteen times as large as itself. Surely the author must have overestimated the length of his fish.

In J. E. Harting's *Recreations of a Naturalist* (1906) there is a chapter "Fishes Trapped by Bladderworts." This consists of very brief notes of the work of Simms, Moseley, Darwin, and Green. Nothing not contained above is found in it, but this paragraph is added here for the sake of completeness.

Theodore Delachaux published an article —"Fischfressende Pflanzen"— in 1894. No copy of the issue of the journal can be found in the United States. However, one could hardly expect to find much in this one-page article by this fish-culture writer.

Alfred Carpenter had in *Nature* (1884) an article entitled "A Carnivorous Plant." This is but a few pages away from Moseley's account (1884.1) and from Simms's second article. As will be seen below, Carpenter had chanced on something new, and, because his title has got in the bibliographies and will be associated (wrongly) with the bladderwort, it calls for presentation and comment. Carpenter states:

> With reference to Prof. Moseley's letter in your issue of May 22 [1884] (p. 81) on "A Carnivorous Plant Preying on Vertebrata," I may mention that in 1881, when surveying at the Paracel Islands in South China Sea, I saw a somewhat similar occurrence. The tide was low on the reef on which I was strolling and admiring the lovely forms of coral existence. As I neared a pool cut off by the [low ?] tide from the sea, I noticed amongst other submarine verdure a very ordinary-looking flesh-coloured weed about one foot high and of similar girth. My appearance alarmed numbers of tiny fish, which darted to the cover of overhanging ledges, but I noticed about half a dozen apparently seeking-cover in the weed. Bending down closer, I saw that they were lying helpless about the fronds, with very little life left in them. Putting my hand down to pick up one of the half-dead fish, I found my fingers sucked by the weed, the fronds of which closed slightly on them. The fish were not caught by the head especially, but held anywhere round the body. The death seemed to be slow and lingering, and where the fish had been held its skin was macerated. These captives may have been caught some time, and were in different stages of exhaustion I regret being unable to name the plant, or the young fish They were from an inch to an inch and a half long. The plant had a dirty and rather slimy look about it.

The explanation is not easy. This, in my judgment, was not a plant—no marine plant given to this habit is on record so far as I can find. There are, however, certain noxious plantlike marine animals, colonial hydroids, one of which might be guilty. There are colonial hydroids which to the nonzoological and uncritical eye might without difficulty be thought to be marine plants. Here are some things that further the idea that this "plant" was a hydroid. The fishes "were lying helpless about the fronds. . . were not caught by the head especially but held anywhere round the body." Then when Carpenter attempted to pick up one of the dying fishes, his "fingers [were] sucked by the weed, the fronds of which closed slightly on them." In other words, his fingers were stung by the lasso cells, and by them the "fronds" were drawn toward his hand. Furthermore, the maceration of fish- and fingerskin was probably due to the poison given off from the lasso cells. Moreover, it is well known that colonial hydroids catch and eat fishes.

It is also alleged that Utricularia catches tadpoles. These and other small embryonic amphibians, the fish stage of these animals, are found in the same shady, still waters where the young of the "coarse" fish noted are found, and where Utricularia is likely to abound. However, their alleged captures are only incidentally noted here. That the tiniest forms might be caught by large bladders is conceivable. The trap might possibly catch a very small tadpole by the

tip of the tail. But that it could catch by the blunt nose or by the tail tip and hold a large tadpole seems impossible.

Various persons have referred to this tadpole-catching, but most of their notices are merely incidental. However, W. Bath (1905) has written specifically on tadpole-catching, with illustrations. These show large tadpoles caught by bladders.

In the drawing of the smaller pair, the bladder is about 30 mm. long by 20 mm. deep, and the head of the tadpole (caught by its blunt nose in the mouth of the bladder) is 15 mm long by 12 mm. deep. In the larger pair, the tadpole is held by the tail. Here the utricle is 43 mm. long by 32 mm. deep, and the head of the tadpole is 22 mm. long by 15 mm. deep. Comparing these drawings with Dean's, one finds it hard to believe that such large, heavy, and powerful animals could be held by the small bladders. Either author or artist would seem to be in error with regard to sizes. Further consideration of this phenomenon must be left to another student.

LE PROBLÈME DE L'UTRICULAIRE

Brocher (1911) coined the phrase, *Le Problème De l'Utriculaire*, and a problem it is. It must be attacked from two angles. First, we must get some clear idea of the structure of the trap and its valve and then we must learn how they work But first of all it must be understood that the valve of the bladderwort is not a mere check valve, like that in the veins of a man, as was thought as late as 1911. It is probably the most complicated structure known in a plant.

Of this Lloyd says (1942, p. 243) that "it may well be said that the study of the anatomy of the trap is by no means easy." And he demonstrates this by devoting his pages 234–243 to a masterly summary of the work of his predecessors in this study to show the progressive development of our knowledge of this most highly specialized mechanism. Then he presents the results of his own researches in 25 pages. These are illustrated by many plates of photographs and line drawings—all originals. The photographs are so heavily opaque and the line drawings so lacking in lettering and explanations that I can get little from them. However, after many days of study, the essential structures of the trap and their action in the capture of fishes have become well enough understood for them to be briefly presented. In this I have been much helped by reading Skutch's scholarly paper and the other articles listed in the bibliography.

Lloyd (1942, p. 233) gives a good description of a bladder as a trap but has no good figure of the trap as a part of the plant. Fortunately, however, Skutch (1929, pl. to face p. 263) gives a splendid portrayal of a trap of *U. vulgaris* with the door of the valve facing the reader (Fig. 9). Done in wash with no attempt to show the cellular structure of the bladder, it is focused on the door, or valve. Lloyd expressly says that none of his figures are copies. Had he introduced into his description of the trap Skutch's figure (as I have done), his description would have been much clearer. Here follows Lloyd (p. 233):

> The vulgaris type of trap is a small flattened pear-shaped hollow body attached to the plant by means of a stalk placed laterally, and truncated obliquely across the narrow end, where occurs the mouth of [sic] entrance. The stalk side is ventral, the opposite dorsal The [dorsal] edge of the mouth carries in most cases a pair of branched antennae, and at the sides some slender elongated bristles These form a sort of funnel ["drift fence," p. 245] leading to the entrance, [presumably] acting as guides for prey. . . .Because of the flattened shape we may speak of the sides and the edge of the trap. The sides may be convex or concave . . according to physiological circumstances. When the trap is set, they are concave; after action they are less so, and the trap has now a more rounded form.

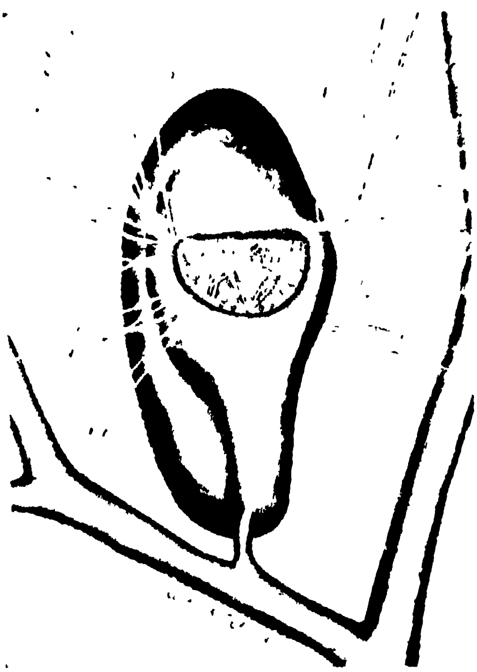
It is important to record just here that

the wall of the trap is everywhere two cells in thickness except the threshold, or sill (of the door), just under the lower, or free, edge of the valve where the threshold is a massive structure several cells in thickness (Fig. 7). The cells, both outer and inner, of the body wall (with exceptions to be noted later) are covered with a cuticle on their exposed surface which makes the bladder practically waterproof outside and watertight inside.

A utricle must have a stopper; hence the stopper, the door, or valve (two layers of cells in thickness), leading into the bottle is of prime interest just here (Fig. 9). It is the most highly specialized part of this most specialized of carnivorous plant traps. That part of Lloyd's description which applies to the mechanism for the catching of water animals follows:

> The door is attached to the trap along a semicircular line on the dorsal part of the entrance, its free edge hanging and in contact [below] with a firm semicircular collar or threshold against which the door edge rests. The convex outer surface of the door bears a lot of larger or shorter mucilage glands . In addition it bears four stiff, tapering bristles [set in pairs] based near the free, lower door edge. These are the tripping mechanism.

These outer things may be seen in Skutch's drawing (Fig. 9). Notice that the valve is set back within the "mouth," or entrance, leaving a shallow vestibule outside. It is hinged to the wall of the trap across and behind the flattish top of the vestibule and then down each side for a total of about two-thirds of the circuit of the "mouth." At the bottom (along the thickened threshold, and for a short distance up each side) it is free. When this free edge swings inward, it permits the entrance of water and foreign bodies, as we shall shortly see. The face of the valve, as the observer sees it, is convex. The upper part is beset with glandular hairs. I find no explanation for the semicircular lines which are situated in a flat, ring-shaped arch



After Skutch, 1928

FIG. 9. FRONT VIEW OF BLADDER

SHOWING VALVE SET BACK IN THE VESTIBULE: HAIR-GLANDS ABOVE, TRIGGER HAIRS BELOW

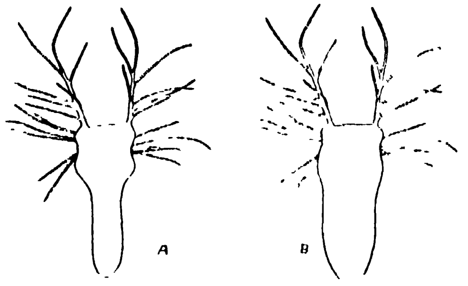
just above the trigger hairs. In this thin and easily flexed region the bending of the valve probably takes place when it swings inward.

Of particular importance are the four bristles (arranged in pairs) at the lower, or ventral, edge of the valve. When these are touched they mechanically lift that portion of the valve, which is tightly pressed against the inner bottom part of the door sill, to form a semicircular opening and permit the influx of water and small organisms (Fig 7).

HOW THE TRAP OPERATES

Like any other trap, in its functions the bladder of Utricularia exists in two stages or conditions—as the set and the discharged, or sprung, trap. When the trap is in the set stage, the trigger hairs are in position and the valve closed. The elastic sides of the utricle are flattened or pressed inward

(dimpled). This results from the fact that the bladder is largely free of water and is subjected to the hydrostatic pressure of the outside water. The gun is, so to speak, loaded and cocked. This condition is portrayed in Figure 10*A*, showing the bladder in rear aspect. The trap is now set.



After Skutch, 1928

FIG 10. THE TRAP BEFORE AND AFTER

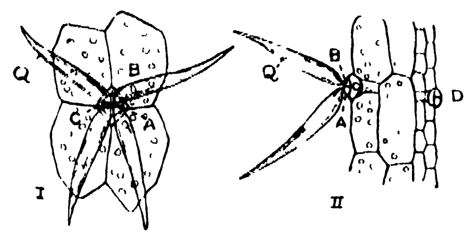
SKETCH OF (*A*) TRAP SET, (*B*) TRAP SPRUNG.

A little fish comes along and by a flick of its tail or a bump of its nose happens to strike the trigger hairs on the lower part of the door. These by their movement distort the bottom edge of the valve and cause it to swing clear of the threshold, and at the same time the walls of the trap expand. The sudden inrush of water carries with it the tail or the snout of the fishlet, and head or tail is held fast by the valve. The inrushing water and the natural elasticity of the walls of the bladder bring this back to its rotund condition as seen in Figure 10*B*. The trap has been activated and the prey secured.

It must be emphasized that these movements of bladder and trap are not activated by any contractile protoplasmic motor body. That the response of the bristles is purely mechanical is seen in their opening the valve when touched even though they have been killed by iodine. Like activity of the trap (return to "sprung" condition) is seen after its having been immersed in picroformal for half-an hour (Lloyd, p. 241).

The bladder is now full of water, and not until it is freed of this can the trap catch more prey—vertebrate or invertebrate. When the valve is freed of any foreign body, helped by the pressure of the water inside, it comes back to its normal set position against the threshold. Then the process of removing the water is begun. This is effected by certain specialized cells, the "quadrifid cells" which line much of the interior of the bladder. These cells are shown in Figure 11, copied from Dean's Plate III (1890). Being without cuticle, they absorb the water within the bladder and pass it through other noncuticularized cells to and through the like cuticle-free outer cell *D*. Thus, presently most of the water in the bladder (about 88 percent, according to Hegner) is excreted, the walls become indented, and the trap comes back to the set condition shown in Figure 10*A* and is ready for another little fish or other prey.

One can understand that minute organisms, diatoms, desmids, protozoans, small crustacea, etc., will go in with ease with the sucked-in water. But how about long worms, mosquito larvae, etc.? These larger organisms often get stuck in the doorway.



After Dean, 1890

FIG 11. SPECIALIZED CELLS

THE QUADRIFID (*Q*) AND ADJACENT BLADDER CELLS IN SURFACE VIEW (I), IN SECTIONAL VIEW (II).

The sheet of the flexible valve must wrap around these when stuck and, acting as a plug, make the doorway tight. A worm with the forepart outside has been seen to break in two— whereupon the forepart swam away. In the case of a mosquito larva

similarly caught, the anterior parts were gradually drawn in—presumably, the water was exhausted and the softened larva engulfed by repeated suckings-in. Lloyd (p. 253) figures and describes progressive experimental intaking of a soft, slender shred of coagulated albumen. But the explanation becomes more complicated when little fishes are concerned.

How little fishes activate the trap and are caught has already been described. But it is interesting to speculate on their ultimate fate. The flexible curtain part of the valve must clamp down on the body so as to permit no further ingress of water—it and the body forming an effective plug (Lloyd, p. 251). The fishes must die from strangulation of the circulation if from no other cause. If the head of the fish, including the pectoral fins, is swallowed, as various figures show, then there is no hope for it. It can only move inward. Various observers have noted that many fishes push forward until the head reaches the opposite wall of the bladder. It should be noted that fishes on meeting an obstruction tend to drive forward. Eventually a small fish might be completely engulfed as portrayed by Halperine (Fig. 7). However, it is notable that no such occurrence has been found in the records quoted above.

When the fishlet is caught by the tail, the final result becomes more difficult of explanation. If the baby fish is very small and the bladder large enough, as the body softens it might gradually but slowly be sucked in. In the case of a larger fish, decomposition might set in after death, and, if and when some predator (a crustacean?) seizes the head, this might be torn off and carried away. Certainly Halperine's larval fish with the yolk sac (Fig. 6) could never be engulfed—unless the yolk sac sloughed off. The fate of Halperine's (and other's) little fish swallowed head and tail by two bladders (Fig. 6), I leave to the reader's imagination.

The question of the fate of the organic bodies taken into the bladders, like Banquo's ghost, will not down. Utricularia, like all plants, must obtain some nitrogenous food. Since it is a rootless aquatic plant, one might expect that it would absorb its nitrogenous food directly from the water by means of its body cells. But it has been found that not only the outer bladder cells but also the general epidermis of the plant are covered with an impervious cuticle. Since the plant has to have nitrogenous food, can it be that this has led to the development of traps to procure such food? Most terrestrial plants that have traps secrete digestive ferments.

Since the utricles of the various aquatic bladderworts vary in size from 1.5 mm. to 5 mm., it is very difficult to ascertain whether they have any digestive ferments. Evidence has been alleged pro and con. But in any case they surely feed on the invertebrates they ingest and digest with or without the help of bacteria. They must absorb this soluble food by their quadrifid cells (Fig. 11) and transmit it by osmosis through the noncuticularized adjacent cell walls to other cells of the wall of the trap.

Perhaps the fish bodies just break up in the same way as do the invertebrates. This matter is very obscure, and, since no investigator has even alleged that the bladders digest and absorb nitrogenous matters from the fishes more or less accidentally taken in, it is necessary to leave to the future the matter of the fish as food.

With all the evidence before us, it must be decided that while the bladderwort does catch fishes this is more or less an accidental, and not necessarily a purposive, action. And most assuredly the plant is not to be called a fisheater. Hence from this standpoint it is not an enemy of the pisciculturist as mistakenly alleged by Simms in 1884. However, since it takes into its bladders and destroys vast numbers of the minute aquatic plants and animals on which little

fishes depend for their food in their earliest days, it is in this respect an indirect enemy of the fish culturist.

The evidence of the amount of Utricularia's intake of the food of young fishes is too extensive to be given here.

Finally, let it be said that one of my purposes in writing this article has been the hope that its publication will lead someone, favorably situated for studying the bladderwort in wild waters, to determine how much fish-catching is done under such natural and unconfined conditions and to illustrate his observations by photographs.

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FUN WITH FOSSILS*

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FUN with fossils is the thrill of finding the unexpected. One never knows what may be turned up in a layer of rock. One may unearth the bones of a horse with three toes where now roams a horse with only one toe; one may chisel from a mountainside the skeleton of a fish preserved ages ago though there may be no piscatorial luck in the stream below; one may dig the leaves of ginkgo, magnolia, or sweet gum from the rocks of "Greenland's icy mountains" or, at heights of over 7,000 feet in the Rockies, wrest impressions of the delicate tissue of a fossil jellyfish. The search is full of adventure, fun, many times failure, but always hope.

Henry Fairfield Osborn contrasted the hunter of living creatures with the hunter of fossils. He said: "The hunter of live game, . . . is always bringing live animals nearer to death and extinction, whereas the fossil hunter is always seeking to bring extinct animals back to life."

Delving into the rocks to obtain the secret of the past is a gamble, but it is a game which people from the rich man to the chief enjoy playing.

In the city of New Haven, there was once a certain rich man by the name of O. C. Marsh, who had so much fun with fossils that he became one of the "three founders of the science of vertebrate paleontology in America." Because this man wanted all his time to study and explore for fossils he served (until his last years) as a professor at Yale University without salary. He had persuaded his rich uncle, George Peabody, to establish and endow a museum of natural history at Yale. And to this museum were brought the many fossil bones of dinosaurs, flying reptiles, marine lizards, birds with teeth, extinct mammals, immense and small, which Marsh had collected or had had collected during the time between 1868 and 1892. There were over 3,000 shipments on which he spent $200,000 of his own money. In addition, he had had dug enough fossils for the U. S. Geological Survey to fill 9 freight cars and cost the federal survey nearly $150,000.

Living at the same time, in the "City of Brotherly Love," was another rich man, Edward D. Cope, who also had fun with fossils in a big way. He was one other of three founders of American vertebrate paleontology. He built up a great collection of fossil reptiles, mammals, birds, and much else, which was ultimately bought by the American Museum of Natural History.

Those two great rivals, the brilliant Cope and the able Marsh, with their organized expeditions of expert collectors in the Great Plains and Rocky Mountain areas in the pioneer days of the second part of the eighties, engaged in a furious race to obtain and describe the bones of extinct creatures of the past. Although their methods became cutthroat and their feelings bitter, they built up vast collections which, together with their own brilliant and scholarly studies and those of subsequent gifted, trained, and leading fossil students, have made possible the beautiful, scientific, and popular restorations and learned treatises of the Peabody Museum, the American Museum of Natural History, and the U. S. National Museum.

But one does not need to be wealthy or go thousands of miles to have fun digging fossils from the earth. We may be re-

* From an address, American Nature Study Society Dinner and Annual Meeting, Boston, December 27, 1946.

minded of Robert Dick (1811–66), the poor, humble, and modest baker-naturalist of Thurso, in the dreary northernmost town of Scotland. Dick, self-educated, earned his daily bread by baking it. Never neglecting his baking, he timed his duties so that he could frequently start at midnight on his roamings over the countryside. He loved all nature and knew its forms well, whether fern, flower, insect, shell, stone, or fossil fish. Through wind and rain he tramped, 16, 20, 50, or even 80 miles, but he never traveled beyond the limits of his native county of Caithness. Here, carrying "3 pounds of iron chisels in his trousers pocket, a 4-pound hammer in one hand, and a 14-pound smiddy forehammer in the other; and his old beaver hat filled with paper and twine," he would leave the drudgery of his bakeshop and ramble amongst the objects he loved. Dick discovered and collected the remains of queer, primitive, extinct fossil fish of the Old Red Sandstone or Devonian rocks. Many of the specimens were glorified in the writings of another famous Scottish fossil-fish collector, Hugh Miller. Some of Dick's and Miller's fish reached the great Agassiz and thereby formed the basis for many of Agassiz' fish descriptions. Dick's was the joy of original discovery, and his fun was cracking the rocks to unearth the unknown creatures of the past. As he puts it in rhyme:

> Hammers and chisels an' a'
> Chisels and fossils an' a'
> Resurrection's our trade, by raising the dead
> We've grandeur an' honour an' a'
>
> Hammers and chisels an' a'
> Chisels and fossils an' a'
> In spite of the devil we'll dig as we' able
> Hurrah for the hammers sae braw.

Even poverty does not prevent exploring in science. Lamarck, France's illustrious botanist, zoologist, and paleontologist, at a time when thinking was fettered by the biblical version of creation and the Flood, gave to the world for the first time a scientific, intelligible conception of main trends in evolution and an appreciation of the vastness of time not previously grasped. Throughout his eminent career, Lamarck lived on a pittance, and when he died there was naught to buy him a decent grave His body, like that of a common beggar, was thrown into the general pauper trench, from which the bones were removed after a certain period and dumped in the catacombs of Paris, the place of his burial will ever remain unknown. Lamarck is called the founder of invertebrate paleontology, and his position as naturalist and philosopher is among the great. It was in the vicinity of Paris, from the rich Tertiary rocks, that he gathered the remains of mollusks, which he described and from the facts formulated general evolutional truths.

To some, the joy of having fossils could only be satisfied by keeping what did not belong to them. In 1848 the skull and carapace of a glyptodont, the ancient relative of the modern South American armadillo, was discovered in Montevideo. This fossil was presented to Vice Admiral Dupotet, who took it to France to give to his home city of Dijon. On the way, he left the specimens for exhibition in the Jardin des Plantes in Paris. When Dupotet later tried to remove his fossils, the authorities of the Jardin des Plantes had become so enamoured of the glyptodont that they refused to part with it Unsuccessful in obtaining his own property, Dupotet made a will bequeathing the glyptodont to the Museum of Dijon and died, leaving his wife to carry on in its behalf. She eventually was partly successful, for the Jardin des Plantes parted with the carapace but retained the skull. Thus the skeleton of the glyptodont became a "house divided." When Professor Henry Ward, of the early Ward's Natural History Establishment, went to Europe to make casts of representative fossils in the various museums, he asked

permission of the Jardin des Plantes to make a cast of the skull of the glyptodont. He was at first refused, but later permission was granted provided he did not sell a replica to the Museum of Dijon. Ward also asked the Museum of Dijon to be allowed to make a cast of the carapace. They too refused but eventually gave permission if he would promise not to sell a cast to the Jardin des Plantes. These promises Ward of course gave, and he returned home with casts of skull and carapace of the fossil. These he combined and later distributed duplicates to the early institutions of our country. Many of those Ward casts are still available in remnants of the museums of the Gay Nineties, where they were gazed at with awe. So, if you ever see one of those large replicas, particularly identified by its heavy armored carapace, like a coat of mail, and the long spiny tail, or one with a clublike tail, like the cudgel of the funny-paper cave man, ponder the moral of how much fun stolen property can lead to!

The life of a doctor is traditionally busy. Yet physicians have been a leading group in the pursuit of natural history. Perhaps a form of relaxation from the arduous duties of visiting the living may be obtained from association with the silent relics of the past. Dr. John Collins Warren, born in Boston in 1778, Professor of Anatomy in the Harvard Medical School, one of the founders of the Massachusetts General Hospital and the McLean Asylum for the Insane, onetime president of the Boston Society of Natural History, and perhaps best known for having been the first to use ether in surgery, took time off occasionally to enjoy working with bones of prehistoric creatures. The most nearly complete and perfect skeleton of a mastodon, one of the extinct proboscidians which roamed North America 20,000 to 30,000 years ago, was found in the region of Newburgh, N. Y. In 1846 Dr. Warren purchased that skeleton for $5,000 and subsequently built a fireproof building at 92 Chestnut Street in Boston to house it. That building became famous as the Warren Museum, and to it the doctor brought specimens of mastodons from all over the world; he finally wrote a fine book on the subject. In 1906, many years after Warren had ceased practicing in this world and the last of his heirs were no more, the Warren collections were sold to the American Museum of Natural History. In 1908 the Warren mastodon was given a new mounting, a shampoo, and glorified by the paintings of its restoration by Charles R. Knight. It is now exhibited as one of the prize showings of the Museum of the city of the Great White Way.

The profession of law may well do honor to one of its profession, Sir Charles Lyell, who because of his love for rocks and fossils finally gave all his time to that pursuit. He wrote much and well and when he died in 1875 he was properly buried in Westminster Abbey, for it was said he was "the most philosophical and influential geologist that ever lived." He traveled from England over the world, including four trips to America. Amidst the fun that he had in collecting fossil sea shells from certain of the rock layers of France and Italy, he contemplated the fact that in each area the percentage of the fossil shells which appeared like those living today was different in each of the four places. And when the percentages were calculated it was found that those in the Paris Basin were the least like the Recent or present-day forms, those in southern France were less like the Recent, those in central Italy were more like the Recent, and those in Sicily were the most like the living sea shells. So Lyell translated the geological history of clams and snails into Eocene, Miocene, Pliocene, and Pleistocene, names which give us handles to talk about parts of the great periods of time previous to our own.

But besides being of philosophical and

scholarly use, the love of fossils may have a practical application. One of the most unusual of merchants was the female fossilist Mary Anning, who, in 1810, a child of eleven, set up and then continued until her death a fossil shop in her native town, the watering place of Lyme Regis in west Dorset on the southern coast of England, near the border of Devonshire. The cliffs of Lyme are famous for the wealth of fossils which weather from their flanks and are strewn along the beach or lie half buried in the clays. There the vertebrae, or "verterberries," in the Dorset dialect, of strange Mesozoic fish, sea reptiles (the ichthyosaurs and plesiosaurs), the remains of ammonites and belemnites (extinct cephalopods), primitive cuttle fish, and many other queer fossils are common wares in the village stores. Both living and fossil fish have been seen for sale on the same counter.

Mary Anning became the Tiffany of the fossil vendors of her day. Through her skill foremost scientists were provided with many remarkable and perfect specimens, and from her stores the visitors to the seashore were furnished curiosities. When selling a fine six-foot ichthyosaur to the King of Saxony and signing her name to the transaction, she quietly and concisely summed up the status of her reputation: "I am well known throughout the whole of Europe." At her death and down through the ages, the geological world has paid tribute to the talent and labor of this female fossilist.

The pleasure derived from the study of fossils may also be a panacea for those who suffer the headaches of public affairs. Thomas Jefferson, when Vice-president of the United States, was elected President of the American Philosophical Society in Philadelphia. This man who wrote the draft of the Declaration of Independence and helped frame the constitution of Virginia, along with many other important documents, in 1797 read to the members of the Philosophical Society a paper entitled "A memoir on the Discovery of certain Bones of a Quadreped of the Clawed Kind in the Western Parts of Virginia." He named his "Great Claw" *Megalonyx*. Later Jefferson's name was perpetuated among the names of the animal kingdom by his giant ground sloth being named *Megalonyx jeffersoni*. While President in 1806, when political debates were many, Jefferson had brought to the White House many hundreds of fossil bones from Big Bone Lick in Kentucky. These relics occupied an empty room in the first house of the land, and the President spent happy moments in this sanctum when not engaged with affairs of state. But such pursuits in science did not strike the public as anything but ridiculous, and in squirtish fashion William Cullen Bryant vented his spleen in poetical satire:

> Go, wretch, resign thy presidential chair
> Disclose thy secret measures, foul or fair
> Go, search with curious eyes for horned toads
> 'Mid the wild wastes of Louisiana bogs
> Or where the Ohio rolls his turbid stream
> Dig for the huge bones, thy glory and thy theme.

Thus from the parade of human endeavors have been picked examples to show that fossils have delighted the soul and stimulated the mind of rich man, poor man, the beggarman, and even the thief, the doctor, the lawyer, the merchant, and the chief.

THE NUMBERS AND DISTRIBUTION OF MANKIND*

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THE problems presented by the distribution of the human population over the surface of the land, and its relations to the natural resources of the earth, form the principal subject matter of human geography.[1] Our knowledge of these matters is still very inadequate. There are wide margins of error in the available statistical material,[2] and still wider gaps in our knowledge of the earth's resources. Yet it seems worth while to attempt to set down some of the facts bearing on these problems as fully as possible.

In this discussion I shall attempt to consider briefly only three of the groups of factors in these problems:

1. The actual magnitude of the present human population and the main features of its distribution over the land surface.
2. The relation between the distribution of the population and that of the fertile lands from which the food of mankind is obtained.
3. On the basis of the first two factors I have ventured to estimate the population capacity of the world on some existing standards of production and consumption.

Numbers. The first question is, How many people are there in the world today? It is not possible to answer this question very exactly. In most of the lands of Western civilization, and in many lands under Western control, fairly reliable censuses[3] have been taken. So we can state the numbers of the inhabitants of Europe and North America, of the countries of the South Temperate Zone, and of Japan and India with some approach to accuracy. But for the large population of China, and for the intratropical lands of the Americas and Africa, we have only estimates of extremely varied value; some of these estimates are based on partial censuses, some are hardly more than guesswork.

Thus there is necessarily a wide margin of error in all estimates of the world's population, which should be remembered in studying the figures in Table 1.

TABLE 1

ESTIMATES OF THE WORLD'S POPULATION[4]

Authority	Date	Population in Millions
E. Levasseur	1908	1,626
Sir G. H. Knibbs	1914	1,649
Times' Atlas	1921	1,646
International Institute of Agriculture	1921	1,820
Statistical Yearbook of the League of Nations	1931	2,025
Statistical Yearbook of the League of Nations	1940	2,145

These estimates do not form a concordant series. In Europe and North America the chief areas of doubt were Russia and Mexico, respectively. In Asia the whole difference may be explained by various estimates of the population of China, but there is equal uncertainty as to the numbers of the peoples of southwest Asia. There were wide differences in the estimates of the population of Africa, which illustrate the difficulty in respect to the numbers of barbarian peoples. Estimates of the population of the Belgian Congo have ranged from 30 millions down to 8 millions.[5]

Table 2 states some of the recent estimates of the population of China to illustrate the differences in regard to the principal area

* From the A.A.A.S.-B.A.A.S. Exchange Lecture, Boston, December 30, 1946.

TABLE 2
POPULATION OF CHINA AND ITS DEPENDENCIES[6]

Authority	Date	Population in Millions
Mingchingpeng Census	1910	324
Government Gazette, Pekin	1911	315
China Continuation Committee	1918	441
Chinese Post Office	1920	428
Times' Atlas	1921	321
Chinese Maritime Customs	1922	443
Chinese Post Office	1922	433
Statistical Yearbook of the League of Nations	1931	450?
Ministry of the Interior (Nanking)	1931	475
Statesman's Yearbook	1931	486
Statistical Yearbook of the League of Nations	1940	450?
Statesman's Yearbook	1946	458

of doubt in estimating the total numbers of mankind.

The census of 1910, which was the basis of the estimate of 1911 and perhaps also for that of the *Times' Atlas*, was a census of households. The multiplying factor (the assumed average number of persons per household) was not the same in all the provinces and is open to doubt.

From the figures here given it appears that the population of the world has increased by about 25 percent since 1911 and that of China by nearly 50 percent. But since 1911 China has suffered from revolution, followed by years of internal disorder, from civil war, and from foreign invasion on a very large scale. There have also been floods and famine and pestilence. The extent of these disasters and their persistence over more than 30 years make it unlikely that there has been any considerable increase in the population of China over this period. In India the influenza epidemics of 1918–20 almost canceled the natural increase of population for the intercensal decade 1911–21. Europe has undergone two great wars and the resulting famines and pestilence since 1914; Russia has also suffered revolution, civil war, and further famine. These facts taken together make it improbable that there has been any large increase in the total world population since 1914, in spite of the growth in the New World.

In view of these doubts and of the variations in many of the estimates which have been quoted, it is impossible to give an exact figure for the world's population. The total is probably near 2,000 millions, with a wide margin of error.

Growth. The experience of the civilized lands during the last two centuries has accustomed us to the conception of a continually increasing population. From 1801 to 1921 the population of England and Wales multiplied more than fourfold[7] in spite of a considerable emigration. Since 1800 the total population of Europe has increased from 175 millions[8] to 500 millions, in spite of the emigration of not less than 40 million[9] people. Under especially favorable conditions some smaller populations have increased even more rapidly. The French Canadians now number about 4 millions. Practically all of them are descended from the 5,800 immigrants who reached Canada before A.D. 1680, when immigration from France ceased.[10] This gives a six hundredfold increase in 250 years.

During the first decade of this century the mean rate of increase in the countries which had regular censuses was 1.159 percent per annum. At this rate the numbers would be doubled in a little more than 60 years.[11] If this had been the average rate of increase in the past, the whole of the present population of the world would be descended from one couple living near the end of the first century A.D. If it could be maintained in the future, then in another thousand years the earth would have about 250 millions of millions (25×10^{13}) of human inhabitants, i.e., more than one to every square yard of land. Such

calculations make it very obvious that the recent average rates of increase among the civilized peoples are far greater than those which existed in the past, and also that such rates of increase cannot be maintained for any considerable time.

Evidently we have been living in a period of exceptionally rapid increase of population. But it is clear that we are approaching the end of that period; for the birth rates are now falling, even more rapidly than the death rates, in a large part of the civilized world. While in the past the direct check to too rapid an increase in numbers was usually the existence of a high death rate, and in particular of high rates of infant mortality, it is now attributable chiefly to a fall in the birth rate. Mankind is able to choose which of these two checks shall be applied; but one of them must be. If the naturally rapid increase in numbers is not controlled by human acts, the appeal will be to the ancient trinity of "War, Pestilence, and Famine." For the surface of the earth is incapable of expansion; and its resources, though great and capable of much fuller utilization, are limited.

WE MAY note very briefly the conditions which made possible the great and sudden expansion in the numbers of the European peoples in the nineteenth century. Evidently these conditions affected the English-speaking peoples to a greater extent than any others, for their numbers have increased ninefold since the beginning of the last century. They now form more than a fourth of all the peoples of European origin, whereas in 1800 they were less than one-eighth.

It is clear that this particular expansion is chiefly due to the peopling of North America, for that continent now contains two-thirds of the English-speaking peoples, whereas it contained only one-fifth of them in 1800.[12] The growth began with the Industrial Revolution, when the applications of mechanical power increased the production of manufactured goods and so led to an increase in the populations of the industrial areas. The increased demand for food was at first met by a more intense cultivation of the homeland, but the insufficiency of this source of food was shown in the "Hungry Forties" of the last century. The pressure of a hungry people removed the tariff barriers which had hindered the free import of food; improvements in transport made the virgin lands west of the Appalachians accessible; and the population of Great Britain multiplied on a food supply obtained from overseas. The demand stimulated the colonization of North America and the even greater increase of its population. But there is little likelihood of the discovery of another "New World" to allow another such expansion of numbers, until man conquers the equatorial jungles.

Distribution. The final limiting factor to the growth of population is that of the food supply; and since man must obtain practically the whole of his food from the land,[13] the next important questions in this study are: What is the extent of the available land? and How much of this land is capable of being used for the support of mankind by the production of materials for food, shelter, and tools with which to satisfy human needs and wants?

The area of the lands outside the polar regions is known to a fair degree of accuracy. Omitting the permanently icebound lands, the total area of the remainder, the available land, is about 50 million square miles.[14] Thus, the average density of population is nearly 40 persons per square mile, a figure which may be of some interest as a basis for comparisons; though in fact the density varies very widely, and the most characteristic feature of the distribution of population is its extreme unevenness.

Of the total of 2,000 million people the greater number live in three comparatively

small regions of particularly favorable environment. In the northwest area of the Old World the populous region of Europe is limited on the north by the parallel of 60° north latitude and the upper valley of the River Volga, on the east by the Ural Mountains and the Caspian and Persian deserts, and on the south by the Sahara-Arabian Desert.[15] This region contains more than 500 million inhabitants on less than 3 million square miles of land. In the Far East the similarly populous region, which includes most of China and Manchuria, the Japanese Empire south of 40° north latitude, and Tonkin, is occupied by nearly as many people on an area of barely 1,700,000 square miles. And in India and Ceylon, between the Thar Desert and the eastern edge of Bengal, there are 400 million people on about a million square miles of land. Thus in these three major populous regions of the Old World there are crowded together nearly two-thirds of the world's population on one-eighth of the available land (Table 3).

TABLE 3

THE FOUR MAJOR HUMAN REGIONS

Continuous Habitable Region	Area in Millions of Sq Mls.	Population In Millions	Population Per Sq Mi.	Central Latitude
Europe	2 8	520	186	50°N.
Eastern North America	1.9	130	52	40°
Far East	1.7	500 (?)	292	35°
India	1.0	400	400	25°

There is a fourth region of the same type in eastern North America which is comparable in extent. But, since it has been accessible to civilized man for only a few generations, it is not yet fully occupied; and it carries only the moderate population of about 130 millions on a little less than 2 million square miles of land.

Nowhere else on the earth is there any similarly large area of dense population, though the island of Java is as densely peopled as England, and parts of West Africa have more than 40 inhabitants per square mile. In other regions there are a few dense clusters on small areas around large cities, especially in South America and Australia. But outside the four great populous regions that have been noted the remaining six-sevenths of the available land is very thinly peopled.

Next we may briefly consider the reasons for this, at first sight peculiar, distribution. Rather more than three-fourths of mankind dwell in the Old World, by which is meant that part of the earth which has been accessible to civilized men during all the historic period, in contrast to the New World, which has been similarly accessible only since the Age of Discovery at the end of the fifteenth century. The Old World, as thus defined, includes most of Asia, Europe, and Africa north of the Sahara, and nearly half the available land. Over this vast area the population is in fact distributed in general accordance with the food-producing capacity of the various regions. The fertile areas are densely peopled—the barren lands are comparatively empty. All the oases of the deserts are, or have been, occupied; and many of them are crowded. By a process of trial and error, which has already extended over some thousands of years, men have succeeded in establishing themselves in all parts of these lands which can be made to provide subsistence. Though the knowledge and equipment gained by Western civilization in the past two centuries has made it possible to utilize lands which could not be occupied by civilized man before (as, for instance, Siberia), yet the general adjustment of population to natural resources in the Old World is the best available guide to the possibilities of maintaining any comparable masses of population in the newer lands of the earth.

Elsewhere[16] I have estimated the extent of the cultivatable land of the world at 30

percent of the total land area, that is, about 16 million square miles. Hence I assume in the rest of this article that that area, less than a third of the available land, is cultivatable. Another 30 percent may be classed as "productive" but not cultivatable; and the rest (40 percent) is occupied by the deserts (cold and dry).

The productive but uncultivatable third of the land is mainly divided between areas of forest and poor grazing land, such as the mountain and hill pastures of northwest Europe and the semiarid range, or bush, of North America and Australia. The wetter areas of this land are often suitable for forest; so are some considerable areas on the margins of the tundra, where the hardier conifers can grow, although the lack of sufficient summer heat prohibits agriculture. The semiarid regions and the summer pastures of the tundra and the high mountains are likely to remain grazing lands.

The wet, forested lands of the hot belt, which are included in the cultivatable land of our estimate, offer the chief possibilities of any considerable extension of the cultivated land. Everyone who has studied the matter must have been impressed by the contrast between the island of Java, which supports a population as dense as that of England, and the uncultivated wastes of forest and savanna which occupy the greater part of the hot lands. A few other small tracts of these lands are relatively well cultivated and populous, as in parts of the African lakes region, the Benue Valley of Nigeria, and parts of Upper Guinea. In South America there seem to be no correspondingly populous patches, though the vast Amazon lowland is a region very favorable to vegetable life. Here the chief obstacles to cultivation appear to be (*a*) the combination of heat and humidity, which forms a most enervating climate; (*b*) the very marshy character of much of the region, which is deltaic in origin and can only be reclaimed for cultivation by a great expenditure of labor; (*c*) the density of the jungle and the rapidity of plant growth; (*d*) the scarcity of labor; and (*e*) the lack of any immediate incentive strong enough to induce any civilized people to attempt the task of colonizing in this region.

It is significant that the chief crops which are now under cultivation in the hot lands are broadly divisible into two distinct categories. The one includes the trees or shrubs of the plantations, whose produce forms a money crop, such as tea, coffee, rubber, bananas, and oil palms, for export to the populous lands of the mid-latitudes. These crops are usually grown on well-drained slopes in the hilly areas. The other category consists of local food plants, such as the rice and associated annuals of the wet, irrigated flatlands of the deltas and valley bottoms of the East Indies.

Up to the present the efforts of European and North American planters in the hot lands have been directed mainly to the first group of products and therefore to areas of considerable relief. Their plantations and habitations avoid the wet and marshy lowlands. Only incidentally, through the growth of a local food supply for their working people, have they developed cultivation of the second type, as in the rice fields of the Guiana coast. Yet the experience of the planters has sufficed to show the nature of many of the obstacles to be overcome in bringing the fertile lands of the hot belt under cultivation. It justifies the prediction that these lands will be reclaimed for man only under great economic pressure and at the cost of enormous amounts of labor. The peoples of the temperate regions are not likely to migrate to the hot lands in any considerable numbers so long as they can find room in the more attractive lands of their own climatic zones. Therefore man's effective conquest of the equatorial regions will be postponed until necessity drives him to it.

Population capacity. In discussing the numbers of the people who can be supported on the world's resources, which we may call its population capacity, we are in fact studying the possibilities of the food supply. There are three sets of conditions on which we may base estimates.

1. We may assume that the principal foods and the methods of producing them will remain much as they are today; and that the increase of supplies will only be that due to raising the standards of cultivation in backward areas, using improved strains of food plants and animals and making full use of all the cultivatable land.
2. We may make the additional assumption that man will, in the near future, overcome the difficulties of cultivation in the wet lands of the hot belt and add them to his area of cultivation
3. We may guess at more or less speculative advances in the developments of science applied to agricultural production, which may enable man to increase the food supply very largely.

Also, we should bear in mind that the number of people who can be maintained at any given level of production varies inversely with their standards of living. It seems probable that the civilized peoples will prefer to check the increase in their numbers rather than accept a lower standard of living.

On the first assumption, that the present methods of food production will be extended but not greatly modified, we may calculate the world's population capacity on the bases of France and British India. These lands are chosen because—

a) Both of them ordinarily produce sufficient of their staple foods for their own needs—on their present standards.
b) Both are "old" lands and are fully peopled under present conditions.
c) Fairly reliable statistics are available for both.
d) They offer a contrast in type of climate and have different staple food plants.

France is a fair instance of conditions in one of the long-civilized countries of Europe which is still, under normal conditions, self-supporting in respect of her necessary foodstuffs; and the standards of living of the French are probably a little above the average of those of the rest of Europe. In France no less than 90 percent of the land is classed as "productive" and half of it as cultivated.[17] The density of the population is a little over 400 persons per square mile of cultivated land. At this rate the cultivatable land of the world would be able to provide food for 6,500 million people, more than three times the present population.

In British India the mean density of the population is more than 600 per square mile of cultivated land; so that on the present standards of India the world might maintain nearly 10,000 million inhabitants, five times the present population.

But it should be remembered that in bad years neither France nor India is able to produce all the food needed by her people. After a bad harvest France must import wheat; and a failure of the monsoon rains may bring famine to large areas of India. If the whole world were peopled up to its full normal capacity on these standards of production and consumption of food, it would in fact be overpeopled, and the surplus population would be periodically removed by famine.

Our second assumption is that the pressure due to an increasing population and a falling standard of living may compel mankind to utilize all the cultivatable lands of the hot belt, at least as fully as those of some small areas in it are now used. At this rate that portion of the cultivatable land which lies in the hot belt, nearly a quarter of it, or 4 million square miles, might be capable of producing food[18] for a population as dense as that of Java. That island contains 42 million people on 50,000 square miles of land, of which a little less than 60 percent is cultivatable; so that the density per cultivatable square mile is about 1,200. If we take France as our basis for

the temperate lands and Java for the hot lands, the possible population becomes 9,600 millions, nearly the same as our second figure based on Indian standards.

In relation to the third assumption we should note the existence of such views as those put forward by the late Prince P. Kropotkin,[19] who could see no limits to the productivity of the land and claimed that the food production of England and other countries could be easily doubled by the application of intensive methods of cultivation. Such an increase would, however, more than double the labor cost of the products and so tend to lower the standards of living. It is true that agricultural productivity can be increased by such expenditure of labor and capital, and still more by the application of the results of scientific investigation into its problems. Many optimistic forecasts have been made, but I know of no data sufficient to justify even an intelligent guess at the limits of such productivity.

It is clear from the estimates here given that the world, as a whole, is capable of supporting a population much more numerous than that which it carries today. The immediate problems of overpopulation are limited to some comparatively small areas; and the present-day "pressure of population"[20] is not against the limited resources of the earth but against the various barriers, natural and artificial, which hinder access to those resources. With our present powers of production the world may well be able to support three times its present population in reasonable comfort. But at the rates of increase of 1900–1910 that number would be reached in about a century from now. And the fact that the size and natural resources of the earth are fixed and limited insures that its human population cannot increase indefinitely.

NOTES

1. E g., Part I of F. RATZEL's *Anthropogeographie* is given to this topic; so also is Part I of P. VIDAL DE LA BLACHE's *Principes de Géographie humaine.*
2. Because of these difficulties all the figures used in these calculations are round numbers; and none of the resulting estimates should be regarded as more than first approximations. Cf. the General Note, *Statist. Yearb. of the League of Nations*, 1940, 13.
3. Where most of the people can read and write a census can be fuller and more accurate than among an illiterate people. Also, great mobility among a people, as in the United States, makes for less accuracy in the census. No census is quite accurate, but I have no data for a numerical estimate of the error in any census.
4. LEVASSEUR. La repartition de la race humaine *Bull. Inst. Int. Statist.* 1909, 48–63. KNIBBS. The Mathematical Theory of Population. *Appendix A to the First Census of the Commonwealth of Australia*, 1917, 31. *The Times' Atlas*, London, 1922, pls. 5, 7. *Int. Yearb. Agric. Statist.*, 1909–1921, published in 1922 at Rome. *Statist. Yearb. League of Nations*, 1932/33 and 1931/40.
5. EAST, E. M. *Mankind at the Crossroads*, London, 1924, 100. LEVASSEUR, *op. cit.*, gives 20 millions. *Philip's Handy Reference Atlas*, London, gives 16 millions in 1900 and 20 millions in 1913. *Annuaire Belg. et Congo Belg.*, 1914, gives 15 millions, and in 1945, 10 millions.
6. The first and fourth figures are from the *China Yearb.*, 1922; the second, sixth, seventh, ninth, tenth, and twelfth from the *Statesman's Yearb.*, 1924, 1932, and 1946; the third is from P. M. ROXBY's The Distribution of Population in China, *Geog Rev*, January 1925; the fifth is from pl. 7 of the *Atlas*.
7. From 9 millions to 38 millions; figures from the *Census Reports*.
8. Estimate of LEVASSEUR, *op. cit.*, and see J. HALICZER's The Population of Europe, 1720, 1820, 1930, *Geography*, 1934
9. To the United States alone more than 33 millions. WARNE, F. J. *Annals of The Amer. Acad. of Pol. and Soc. Science*, January 1921.
10. MARQUIS, G. E. *Social and Economic Conditions in Canada*. The American Academy of Political and Social Science, May 1923, 7.
11. KNIBBS, *op. cit.*, 31. See also article by F. SHIRRAS, *Econ. J.*, March 1933.

12. Populations of the English-speaking peoples in millions (whites only).

1801	British Isles	15,9	1931	British Isles	49
			1941	Australia and New Zealand	9
				South Africa	1
	Canada	0,1		Canada and Newfoundland	8
1800	U. S. A.	4,3	1940	U. S A	119
	Totals	20,3			186

13. It is not possible to determine with any precision what amount or proportion of human food is obtained from the sea. This does not affect the value of our estimates of population capacity, since these supplies are included in the resources of the existing populations on which those estimates are based.
14. Fawcett, C. B. The Extent of the Cultivable Land. *Geog. J.*, December 1930.
15. See also Mackinder, Sir H. J, *Democratic Ideals and Reality*, London, 1919; and Fawcett, C. B. Centres of World Power, *Sociol. Rev*, April 1926, and The Changing Distribution of Population. *Scot. Geog. Mag.*, November 1937.
16 *Op cit.* Note 14 above.
17 *Int Yearb. Agric Statist.*
18 It is well to note that the possibilities of transporting perishable foods are likely to improve still more. Hence such an increased amount of food might be used to feed the people of the temperate lands so long as their economic and military power enabled them to take it.
19 Kropotkin, P. *Fields, Factories, and Workshops.* London, 1898, and later editions.
20 Fawcett, C. B. Pressure of Population *New Commonw. Quart*, London, January 1943

UNNUMBERED SPRINGS

Unnumbered springs have gone before
This day that floods its golden store
Of radiance upon the heart;
Yet all men rediscover, start,
Beside this gate, this ancient door.

Lovers in legendary lore
Have thrilled to hear the thrush downpour
His burthen of remembered art
Unnumbered springs.

Spring is not new; ambassador
To Pterodactyl, Dinosaur,
Older than chaos or the chart
Of God . . . Cretaceous seas apart,
Pteranodon, new-flying, bore
Unnumbered springs.

Barbara Whitney

THE SENATE PONDERS SOCIAL SCIENCE

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ON July 3, 1946, the United States Senate passed a bill to establish a National Science Foundation, after excluding the section designed to make the provisions of the bill applicable also to the social sciences. Since the social sciences must perhaps depend in the future largely on public provision for their advancement, the reasons assigned for excluding them in the most comprehensive legislation yet proposed for the advancement of science is a matter of some interest from several points of view. In the first place, the discussion of the matter in the committee hearings and on the floor of the Senate gives us some indication of the present attitude of legislators toward the social sciences. In the second place, an analysis of this attitude affords us clues as to the nature of the obstacles to be overcome before the social sciences can expect to share the prestige and the support accorded the other sciences.

The present article will deal only with the public expression of attitudes as found in the *Congressional Record* for July 1-3, 1946, and in the reports of the preceding committee hearings.[1] I have no "inside" information regarding the "real" or "true" views of everyone concerned, or of lobbies or "politics" involved. Likewise, this article does not deal with the efforts of individuals and organizations for or against the inclusion of the social sciences, except as these find expression in the published record. Finally, I am not primarily concerned with a criticism of the senators opposing the legislation as a whole or the inclusion of the social sciences in particular. Their attitudes may be regarded as entirely sincere and as a true reflection of public opinion on the subject of the social sciences. It is more pertinent to inquire into the possibly legitimate grounds for such views as exist, in the actual behavior of social scientists themselves at present. While taking this impersonal view of the Senate's action, it is proper to compliment the sponsors and certain supporters of the bill on their earnest efforts on its behalf. Special credit in this connection is deserved by Senator Magnuson, of Washington, Senator Kilgore, of West Virginia, Senator Fulbright, of Louisiana, and Senator Thomas, of Idaho.

The bill as reported favorably by the Committee on Military Affairs included the social sciences. Let us therefore consider first the record of the bill on the Senate floor, confining ourselves to the discussion leading up to the exclusion of the social sciences. Later we shall review the more extensive discussions of the subject at the hearings.

On the first day of the debate Senator Radcliffe, of Maryland (A.B., Johns Hopkins, 1897, and Ph.D., 1900), made certain justifiable remarks regarding the danger of crackpots and "applied" social science research with specific references to "men addicted to isms" (8166) and "wild-eyed so-called research" (8167). (Senator Radcliffe, however, voted for the inclusion of the social sciences.) At this point also there arose the question as to the definition of social science. Senator Fulbright had the floor, and the following discussion ensued:

SENATOR FULBRIGHT: I asked an able scientist yesterday if he would define social science. I had been worrying about that. He said in his definition, "In the first place I would not call it science. What

is commonly called social science is one individual or group of individuals telling another group how they should live" (8164).

SENATOR WILLIS (B.A., Wabash College, 1896; M.A., *honoris causa*, 1902): I wonder if that is not a pretty good definition (8164).

It may be stated here that the general tone of the discussion throughout the three days indicated that most senators were of the opinion that the above was a pretty good definition. There is evidence too that one reason why provision for research in the social sciences appeared to the senators to be unnecessary was that, after all, we already know the answer to social problems. For example, Senator Willis delivered himself of the following: "It is a question of keeping selfishness in restraint, that is all" (8165).

I suspect that the above represents a major type of reason for the exclusion of the social sciences from the provisions of Senate Bill 1850. However, the assigned reasons which finally were conclusive seem to have to do with the feeling that the legislation was for the purpose of promoting basic research and that the most that could ever be true of the social sciences was that they had to do with practical applications and planning. For example, Senator Hart, of Connecticut (Naval Academy, 1897, Admiral, Ret., 1945), one of the two principal opponents of the inclusion of the social science provision, made the following statement:

SENATOR HART: Support of social science and research should be limited to studies and planning. That is a very good and practical reason why the social sciences should be omitted entirely from the bill which is primarily for improvement in the basic sciences... (8216). The fact is that social studies and basic science are not sufficiently alike either to be joined by the same legislation or to be administered by the same organization (8217).

This point of view was also much stressed by the chief opponent of the social science provision, Senator Smith, of New Jersey (B.A., Princeton, 1901; L.L.B., Columbia, 1904; L.L.D., *honoris causa*, Brussels, 1930, Princeton, 1945; Executive Secretary of Princeton University, 1919–27, lecturer, Department of Politics, Princeton University, 1927–30). Both Senator Smith and Senator Hart had recently become members of the Senate to fill unexpired terms and both had served only about one year. There was on the part of the senators much deference to Senator Smith on account of what is referred to as "his distinguished academic career" as giving him special qualifications to speak on this subject.

SENATOR SMITH: I should like to see the social sciences given aid, but I think their problem is such a different one that the two should not be joined in this bill .. (8233). [Social science research] definitely has nothing to do with this bill. The bill has to do with basic research in pure sciences as they are understood in the academic world (8237).

Consider the further remarks of Senators Hart and Smith:

SENATOR HART: In the first place, no agreement has been reached with reference as to what social science really means. It may include philosophy, anthropology, all the racial questions, all kinds of economics including political economics, literature, perhaps religion, and various kinds of ideology.... There is no connection between the social sciences, a very abstract field, and the concrete field which constitutes the other subjects to be dealt with by the proposed science foundation. Mr. President, this may well be a field in which the Government should proceed to foster and subsidize research; but I submit that it has no place in this bill. This is a bill for the promotion of research in the fundamentals of natural sciences.... Furthermore, Mr. President, what to my mind is one of the greatest objections to its inclusion in the bill is the fact that no board, no administrative organization which we could set up could possibly be adequately qualified to administer such policies and carry on work in two fields so absolutely diverse... (8348–49).

SENATOR SMITH: I have conceived of this bill, as I have said so many times, as a bill for research in pure science, not in applied science but in pure science. We are trying to subsidize pure science,

the discovery of truth. This has nothing to do with the theory of life, it has nothing to do with history, it has nothing to do with law, it has nothing to do with sociology (8349).

This latter view of the case, coming from a man regarded as an authority on the matter at hand, seems to have carried the day. The principal voice raised to the contrary was that of Senator Thomas, of Utah, who toward the close of the debate delivered the following:

SENATOR THOMAS: Mr. President, no invention, no patent, no scientific development amounts to anything for the benefit of the people anywhere unless it has its social aspect, and for us to assume that we can carry on this great political institution, the Government of the United States, and cause it to develop for the benefit of the people of the United States, without having reference to that great branch of knowledge which is called social science, would be to make the mistake of all time.... If we insert limitations barring social science in this bill, which establishes a great foundation, they will cripple—probably forever—the very things that government wishes to do most (8349).

A few incidental remarks regarding the relationship of academic and research life to politics may be of sufficient interest in this connection to be included in this record.

SENATOR MORSE [referring to his twenty years of experience in education and service on the scholarship and research boards of two institutions]: We have a lot of educational politics in America. If anyone wants to get a graduate course in practical politics I think he has only to belong to the faculty of an educational institution. Certainly that is where I got my training.

SENATOR SMITH: Mr. President, if the Senator will yield to me for a moment, let me say that I am glad to corroborate what he has said. I, too, got my experience in politics there.

SENATOR MORSE: The same remarks may be made in relation to some of the great private foundations. I mean no criticism but it is simply a fact (8359).

A study of the three-day debate on Senate Bill 1850 shows that the matter of the inclusion of the social sciences received no considerable amount of the total attention, which was devoted to other aspects of the National Science Foundation proposal. The question of the inclusion of the social sciences was definitely a side issue. The whole proposal for a National Science Foundation was apparently a somewhat mysterious subject to most of the senators. The following remark is significant in this connection:

SENATOR HAWKES: I wish to say that I am in favor of scientific development, but I personally do not believe that this body understands what it is doing. I have talked to any number of senators and all seem to be at sea and in a fog as to what we are asked to do (8265).

The main reason that the bill finally passed even with the social sciences excluded seems to have been a general feeling that perhaps the legislation had something to do with the atomic bomb or protection therefrom. Coming up, as the bill did, toward the close of a legislative session, there is every reason to believe that Senator Hawkes's observations are correct. The vote[3] to exclude the social sciences, therefore, should perhaps not be taken as reflecting any considered hostility or opposition on the part of the Senate, but simply as a reflection of the common feeling that the social and the physical sciences have nothing in common and that at best the social sciences are a propagandist, reformist, evangelical sort of cult. At the conclusion of this article we shall consider whether the attitudes and behaviors of social scientists themselves do not largely justify the position taken by the senators. But first let us consider the more thorough discussion of the subject as found in the committee hearings on the bill in question.

NOWHERE in the approximately 1,200 pages of hearings reports do we find any vigorous opposition to the social sciences

as such. This was especially true of the testimony of physical scientists. A questionnaire submitted by the Executive Secretary of the American Association for the Advancement of Science to the members of the Council of that organization showed that 67 percent favored inclusion of the social sciences in the bill. As Dr. Meyerhoff remarked in this connection: "Naturally that included all the social sciences but the social scientists are a small number of the Council and the large vote, you see, must come from the people in the basic or physical sciences themselves"(85). From this point of view the record is, in fact, rather encouraging. There are a very large number of references to the desirability of including the social sciences on the part of witnesses who were not asked to testify on that aspect of the matter but who gratuitously suggested it. Such, for example, was the case of Dr. J. R. Oppenheimer, director of the New Mexico laboratories of the Manhattan Project. Speaking of the desirability of providing scholarships for students in the social sciences, he said:

> DR. OPPENHEIMER· I am aware of difficulties of establishing in these fields rigorous criteria of competence and qualification. Nevertheless, at a time when the whole world realizes that many of its most vital problems depend on an understanding of human behavior and of the regularities which underlie the operations of our varied society we should recognize the great benefits which may come—I would like to say which will come—from attracting men and women of prominence to the study of these questions (301-302).

This kind of testimony from the physical scientists was quite frequent.

The objections which occur, like those on the Senate floor, rather take the form of suggesting that, while the social sciences doubtless deserve support, they are so different that they should not be included in this bill. Since that is the ground on which Senator Smith based his opposition on the floor of the Senate, it will be well for us to note the sources and nature of that argument.

Even the testimony that was friendly to the social sciences, however, betrayed a very great vagueness and frequently a gross misunderstanding of what the social sciences are or should be before they can ever hope for support as science. I am not here primarily interested in refuting these erroneous notions, but rather in diagnosing and classifying them. The refutations have been elaborated elsewhere in considerable detail.[3]

What were the principal misapprehensions regarding the nature of social science as revealed in the Senate hearings?

1. *Social science cannot be unbiased.* First of all, there is evident in the testimony before the Senate the deep-seated notion that there can be no such thing as disinterested, unbiased inquiry into social questions. Interestingly, but somewhat discouragingly, one prominent spokesman rather inclined to that point of view was Dr. Isaiah Bowman, who, in addition to being President of Johns Hopkins University, was for some years a member of the Social Science Research Council. He does not appear to have gained from the latter association any deep appreciation of the possibility of basic and unbiased research in the social sciences. Perhaps, in fact, he gained his skepticism from his contact with the Council. He testified as follows:

> DR BOWMAN: It is well known that so much of human prejudice and tendency and social philosophy enter into the study of social phenomena, that there is the widest difference of opinion as to what constitutes research in many instances in the social sciences. But I would, if I were drafting a bill that paid my respects to that principle, and it is a sound principle that men's field cannot be detached from their researches in the social views, I would still keep two places open in the bill for the play of minds in the social sciences.... At such times of national

emergency, when the most critical judgment is required of all our people, the social scientists should be called in as freely as circumstances will permit. The second of my two points respecting the work of social scientists relates to statistical matters (23)

Another college president was even more skeptical about the social sciences, and again it is significant that he also had had some background in the field. Dr. John Milton Potter, of Hobart and William Smith Colleges, Geneva, N. Y., spoke at some length in this vein:

DR POTTER: I might in a word say that I do not believe a scientist will learn about society by the study of social science as a science. Still less will he learn about it by the establishment of a research project in social science as a science. . . . As a matter of fact, there are some people who think you can extend the strict scientific method into almost any region of human affairs. I don't happen to believe so. I was trained as a historian. I have also had some experience with the so-called sciences (943).

The following discussion then ensued:

SENATOR MAGNUSON: Suppose this Foundation went into certain projects. Wouldn't it be desirable, in your opinion, to have some basic information regarding the social aspects question?

DR. POTTER: Yes, sir.

SENATOR MAGNUSON: What that research may result in?

DR. POTTER: Certainly. Of course. But these things, it seems to me, could be far better supplied by experienced men like Mr. Baruch or, rather, by you gentlemen here in the Senate, so many of whom are experienced lawyers, than by trying to tackle the thing entirely on the academic level.

SENATOR MAGNUSON: Statistics is one part of social science. They are very necessary for any project we may take on.

DR. POTTER: Yes, sir; and statistics are subject to enormous interpretation. It seems to me that to set up an organization with the prestige which such an organization would inevitably have to give the punch of official report to a group of statistics which might be extremely controversial rather than to have these statistics interpreted as they ought to be by the Congress of the United States, which includes men who are themselves experts in politics, political thought, political economy, and public law, would be a mistake (944).

We have here the interesting thought that Congressmen, lawyers, and politicians are more reliable interpreters of statistics than statisticians and scientists. Dr. Potter's further remarks are most illuminating as to his notion of science and its relation to human affairs. He is clearly under the impression that it is the business of science to tell people what they must do with scientific findings as well as what *can* be done:

DR. POTTER: It is not an exaggeration to say that the strictly scientific approach used in the problems of physics, if applied to major international political problems of our time could well produce such answers as (1) economically nonproductive and mentally backward populations should be destroyed; (2) it is cheaper to appease than to oppose a tyrant; (3) we should obliterate the populations of all potential aggressor nations which resist following our advice . . . (946).

A National Research Foundation which promulgates bigger and better atomic bombs, without consideration for fostering the teaching which will help men to seek to know how to live without using these new weapons, would be, in my respectful opinion, a betrayal of the aspirations of most Americans, however well it might fit snugly into the inhuman grooves of systematic scientific research. So would be a foundation which attempted to improve human relations by using laboratory methods of scientific research (946).

One wonders what Dr. Potter thinks social research would be for if not to reveal to men *how* to live without dropping atomic bombs on each other. What, on the other hand, does he consider the true solution?

DR. POTTER: In my opinion Congress should in this bill provide for a division of the humanities—rather than a division of the social sciences, a misnomer. Such a division should not engage in research. Instead, by use of scholarships and fellowships, it should seek to stir concern among young men and women for teaching in those fields of human knowledge in which the accumulated wisdom of our civilization can be made familiar to our use in the schools and colleges of the United States (946).

I am sure no one will be uninterested in the "accumulated wisdom of our civiliza-

tion." A major and most effective portion of that accumulated wisdom is science, and it has been accumulated reliably and has been made effective through scientific research. It is the social scientist's contention that scientific research is needed also in that field to determine how much and what parts of the accumulated lore according to which we attempt to live are reliable wisdom and what parts are dangerous and misleading folk beliefs.

2. *The social sciences are "applied," not "pure" or "basic."* The idea that the laws of sociology are as much laws of nature as are the laws of physics is not brought out anywhere in the testimony, and it seems to be taken more or less for granted by everyone that the laws of sociology could at most be only so-called applied laws comparable to the practical technology of social work and engineering. Except for some of the testimony of some of the social scientists themselves, to be mentioned later, most of the discussion on the part of physical scientists makes it clear that they think of social science entirely in terms of its relation to health, medicine, stream pollution, improvement of standards of living, improvement in public assistance, the care of orphans, and its significance in "raising the general level of humanity" and doing good in general. Such a program, in addition to a lot of general education, presumably of essentially the same sort that is now being dispensed, is probably what most people understand by social science.

The "applied" nature of social science research was urged both in opposition to, and in support of, the inclusion of the social sciences in the National Research Foundation. As an example in opposition, the following remark of Dr. Morris Fishbein, on behalf of the American Medical Association, is significant:

> DR. FISHBEIN: We doubt the desirability of entering at this time into research on the social sciences, and I will mention the chief reason for that, which is the great danger of the use of so-called research in the social sciences for political purposes and to influence legislation (496).

Other examples of opposition on the ground that the social sciences are "applied" will appear in the next section. On the other hand, much of the data cited by the social scientists themselves in support of the social sciences were of the "applied" type. This was also true of the testimony of some prominent government officials.

3. *Social science research should be controlled by a separate foundation.* Closely related to the above views, and more or less in consequence of them, was the view that, while research in the social sciences should be supported, the activity should be controlled by a separate foundation.

A good deal of the testimony of the physical scientists took this form. For example, Dr. I. I. Rabi, of the Columbia Radiation Laboratories, and Senator Fulbright engaged in the following discussion:

> DR. RABI: It seems to me that social sciences certainly need support, but it also seems to me that it would be wrong to tie it in with the physical sciences. Although they are both called sciences, their disciplines are actually very different, the type of training. One is certainly more controversial than the other, and I would say that it would need an entirely different kind of board, a different kind of administrator. I think that hitching them together in this way would harm both.
>
> SENATOR FULBRIGHT: If social science was made a separate division, they are not mixed up, that was discussed the other day. I didn't mean to put social scientists out in all the divisions which are concerned with natural science, but make a separate division. Would that still be subject to an objection?
>
> DR. RABI: I still think so. For example, most of the things or many of the things which a social scientist has to say are controversial in nature. It is not generally possible for them to prove a point by direct experiment and so on. I think hitching these two together you might find under certain political complexion of this country that the work of the social science would become unpopular and would therefore reflect on the whole job. It seems

to me that the two fields are sufficiently different that one wouldn't quite do that. It would not be wise to have them sink or swim together (998).

It is impossible to blame these scientists for their desire to avoid compromising their own sciences because of the character of much that is today offered to the public as social science. It is to be noted, too, that the remarks of the physical scientists are directed at the social sciences as they are today rather than making the broader assumptions that these weaknesses are intrinsic and inherent in social phenomena. Nevertheless, their testimony was probably extremely influential in crystalizing this view which was most conspicuously urged on the Senate floor against the inclusion of the social sciences. Dr. Rabi also raised one additional point regarding the danger of government control of social research which is worth mentioning:

DR. RABI: I have one other objection to its inclusion, if I may say so, Senator. That is, I am afraid of the power of this foundation, in the support of social sciences through fellowships and otherwise, to make such selections as to strengthen a preconceived point of view or a particular opinion. You see, social science comes very closely to the fundamental political question which are questions of the day, and I begin to see possibility of a Government's building up a certain body of opinion, a certain direction of thinking through that, whereas in the physical sciences I am not afraid of that simply because it is quite objective. You can prove things by experiment (999).

A similar viewpoint was common among the engineers. For example, Thorndike Saville, Dean of Engineering, New York University, speaking for 67 of the principal engineering colleges of the country, testified as follows:

DR. SAVILLE: By no means deprecating the importance of the social sciences, we believe that they merit separate support for research and should not be included in the present proposal. In view of the importance of the social sciences we believe that our association will support a separate bill to that end. Such separation would be of greater over-all good to the public; in other words, the two fields of physical and social science do not belong together in the scope of the agency it is proposed to set up (707)

This view of the matter is strongly emphasized also in the testimony of Dr. B. A. Bakhmeteff speaking as a member of a committee from five major national engineering societies:

DR BAKHMETEFF. Now the natural sciences, of course, deal with the immutable laws of nature and politics doesn't enter into that The social sciences, of course, on the contrary, deal with changing relations between men and you can't help in appointing for example, members of a board which is going to deal with social sciences, to find there is going to be a lot of pressure from pressure groups, and so on, and it must be an entirely different type of men who administer the one and the other, and our idea would be this We think that a social-science group, and a science group or a basic research group, will benefit tremendously by mutual contact, and I think that each can teach the other one something, but we feel that the purpose of your legislation will be better served if they are kept under different roofs and not fused together (715).

The same note appears in the testimony of Dr. Harlow Shapley who, again, was friendly to the social sciences but somewhat worried as to just how to provide for them:

DR. SHAPLEY: The scientists who worked on the various committees that produced the Bush report were acting under a directive from President Roosevelt, that they look into the field of science. That was interpreted as the natural sciences and, therefore, as a matter of course, the social sciences were not included in the report, nor in the resulting proposal for legislation. Nevertheless, the natural scientists, in thinking the problem over, seemed to me to have become increasingly aware of the necessity of including at an early date, as I suggested, at least some of the social sciences. . . . There has been some worry among the social scientists, I would say, as well as among legislators and natural scientists, that the social scientists have to deal with subjects that are close to politics at times and, therefore, there might be a bit of confusion in a clear-cut bill in the support of science if they were too generously or too clearly brought into the picture . . . statistics

and, I'd say, anthropology and studies of population migrations and all such problems are of immediate concern. If they could be included in this bill, I would heartily endorse such inclusion personally, but I speak only as an individual, not for any group (51-52).

Most significant of all in this connection, perhaps, was the testimony of Karl T. Compton, of M.I.T. It is doubly significant that Senator Smith, of New Jersey (who opposed the bill in the Senate), was present during Dr. Compton's testimony. The following type of discussion may have had great indirect significance in determining the ultimate action on the bill:

SENATOR SMITH: In your mind does this foundation for scientific research include anything but the strictly scientific? Would you take the social sciences and humanistics, etc., or should they be left outside the field of Federal support?

DR. COMPTON: That is a problem I have worried about a good deal, and I am not sure I can give a sensible answer. Theoretically, I think it would be fine to include the social sciences; practically, I don't know where you would stop, because everything is social science, really, everything that human beings are interested in. One difficulty that I see in trying to combine the two in one foundation is the fact that methods are so different, I think if they were combined in one foundation it would probably be necessary to do what is in fact contemplated in the bill—that is, have two divisions, one which specialized on one, and one on the other. That is the way some of the big foundations operate.

SENATOR SMITH: I might say I just read a report that Dr. Dodd of Princeton had submitted on the subject. He rather thinks you can't bring the social and humanistic sciences into this picture. This is strictly on the scientific end of the page.

DR. COMPTON: It would certainly be a lot easier to handle and I think it would be handled more effectively if they were not brought in, but I don't want to say the social sciences don't need help. They have some terrific problems, but I am not sure in my own mind whether this is the best way to help them or not (631).

Later, in connection with the same colloquy, Dr. Compton testified as follows:

DR COMPTON: It seems to me that the impact of the social sciences comes in under a very much bigger umbrella than a foundation of this sort. Everything in public opinion and the press brings about that impact. I don't think the additional gain that would come here would be very great. Also, *I am somewhat suspicious of any group trying to set out a program of discovery of the facts of nature, as far as the fundamental science is concerned, on the basis of an anticipated exploitation or intensification of one or another social objective* (631) [italics mine].

So far as the fundamental research is concerned, I don't believe the presence of the social scientist would be helpful and it would be better to have more of the natural science on there. When it comes to any stimulation of applied research, things that might be of benefit to the community, then I think the social scientist could be useful. I haven't given you a clear-cut answer, because I don't have a clear-cut decision in my own mind (632).

There is no doubt that Dr. Compton in the passage italicized has put his finger on a point on which a large proportion of social scientists are guilty, and which many, in fact, regard as their proper business. The implication of this statement is undoubtedly the most damaging one that at present lies at the door of social scientists.

The contention that the social sciences must be supported, but under a separate foundation, doubtless seemed reasonable to many Senators in view of the characteristics that had been imputed, not without justice, to these sciences as currently defined and practiced. Actually, however, the proposal to provide separately for the social sciences is open to two serious objections: (1) It would only tend to perpetuate the present unfortunate gap between the physical and the social sciences and thereby foster the very shortcomings of the latter which are today urged as the reasons for not including them with the physical sciences in the same foundation. (2) One of the principal arguments for the separation was the present undefined scope and content of the social sciences, which, as Senator Hart suggested, at present may

include "perhaps religion" and "various kinds of ideology." To others the term includes the even more amorphous area commonly called the "humanities." The possibility of any kind of effective social science research being carried on under an organization devoted to such incongruous methods seems extremely remote.

Inclusion of the social sciences would, of course, have required at the outset a more rigorous definition of these fields. Such definition could easily and defensibly have been arrived at on the basis of scientific criteria acceptable to scientists. President Conant, who is himself a chemist and, perhaps with good reason, not disposed to take the social sciences too seriously, nevertheless urged the inclusion of psychology, anthropology, sociology, and economics.

In this connection the following exchange ensued:

> SENATOR FULBRIGHT. I don't believe you mentioned political sciences. That is sort of vague.
>
> DR. CONANT: That is why I didn't mention it
>
> SENATOR FULBRIGHT: That is really the greatest weakness of our democracy
>
> DR. CONANT: Yes; the difficulty is whether your immediate advances in that wouldn't come through the role of sociology, anthropology, and psychology
>
> SENATOR FULBRIGHT: I confess I don't quite know how to approach it except through general education . . . (984).

The contention that the same officials qualified to administer a foundation devoted to physical and biological science would not be qualified to administer also an organization including the social sciences seems to me totally without merit. In fact, it might be a healthy thing for social scientists to be compelled to satisfy an administrative board composed largely of physical scientists both as regards the "basic" nature of proposed social research, as well as its freedom from "ideological" taint. To break down the traditional separation of the physical and the social sciences is precisely what is needed in the present situation and an end to which the National Science Foundation might have contributed.

4. *Education rather than research is needed in the social sciences.* There recurs throughout the hearings the idea that so far as social problems are concerned what is needed is education rather than research. The curious notion continues to persist that education is a panacea for all social problems, no matter what kind of education it is and no matter how erroneous the notions inculcated through education may be. Thus Senator Fulbright repeatedly comes back to the point that the present social situation requires primarily education and political measures, although he is also a strong supporter of research. In connection with some of Professor Ogburn's testimony, Senator Fulbright said:

> SENATOR FULBRIGHT: . The educational system from the beginning up, I think, is frankly very poor in this country. It hasn't kept up with our material problems, and I think one of the difficulties goes right back to our elementary schools. People simply don't appreciate the significance of social sciences, which means your legislators don't either. I mean they reflect the people's reaction. That is the great trouble. I have been bothered about it a great deal . . . I don't think that is an argument against doing this research, but it seems to me that one of the troubles of getting research is the poor level of the education of the people as a whole in this country. They can appreciate an automobile or a bathtub, but it is exceedingly difficult to get support, public support, for programs in this field, which I confess I think is our greatest weakness as a nation (780).

It is impossible, of course, to disagree with this statement of Senator Fulbright, although he seems to have become involved here in the old hen-egg problem in his feeling that the reason it is difficult to get support for social research is the low level of education, and the low level of education may in turn be regarded as the result of the failure on the part of the social sciences to demonstrate the importance of education

in these sciences. Actually, of course, the two must go forward together. This they have not done, especially in the social sciences, for the reason earlier mentioned, namely, that there is a widespread feeling that we know the answers to social problems and that therefore no research is needed. The feeling seems to be that all we need to do is to diffuse what we already know sufficiently widely and fervently. The proportion of university budgets devoted to teaching and to research, especially in the social sciences, reflects this attitude. Education has become a fetish. It is something which no one dares to question and for which enormous sums are available. The idea that the importance of education depends on the validity and relevance of what is taught has made little headway to date.

In this connection, Dr. Smyth, of Princeton, testified as follows:

> DR. SMYTH: I think possibly it is desirable to have at least scholarships and fellowships in the social sciences. I am more skeptical about attempting to set up in this bill a division of research in the social sciences, because, to my mind, it is very hard to limit it. I am not a social scientist, but, as Dr Compton said, it is very hard to say what shouldn't come under that, whereas it is fairly easy to define what you mean by research activity in the physical sciences. But, in line with my general idea that what you need is to get more people well educated, is the basis for scientific progress and every other kind of progress, and I would be glad to see scholarships established in the social sciences. I think they might be related with those in the natural sciences (652–653).

In the latter connection Dr. Smyth went on to say:

> DR. SMYTH: Let me say this: I believe that our great problem—I am sure this is obvious to everyone—our great problems that we face are not the problems of the natural sciences, they are the problems of the social sciences, and of politics and of ethics, if you like. If it were possible to do, I think conceivably the best thing for the world would be to retire all the natural scientists, pension them off in pleasant places, or else put them to work on social-science problems—at least stop their research until the world caught up with them in a sense (654).
>
> THE CHAIRMAN [SENATOR KILGORE]: No. You must keep ahead with your basic sciences You must keep ahead with the basic inquiry into the laws of nature, so that you know what law to employ when a social scientific problem comes up or how to modify that law to meet the problem (654).

The Chairman is apparently here under the impression that the laws of nature are necessarily confined to the physical sciences and that the problems of social science are themselves to be solved ultimately, if at all, by applying some law of the physical sciences.

We have already referred to this question above. The testimony of the social scientists who appeared before the Committee, although they made able and convincing statements of their case, probably did little to resolve the fundamental confusion which persists in the minds of most scientists to the effect that the social sciences cannot be basic. Indeed, there is little reason to believe that the result could have been altered even if this highly pertinent testimony had been eloquently presented on the Senate floor, in view of the tremendous dead weight of traditional views on this subject as revealed in all the testimony and the debate. Excellent statements and discussion on behalf of the social sciences were submitted by Wesley C. Mitchell, John M. Gaus, Robert M. Yerkes, E. G. Nourse, W. F. Ogburn, and E. E. Day. Dr. Gaus spoke specifically and ably in refutation of the point that the social sciences are "suspected of being largely the recording of partisan and prejudiced attitudes already present when the research was undertaken." Dr. Yerkes emphasized that "we are weakest at the present moment on the basic or relatively disinterested sort of inquiry."

Dr. Ogburn devoted some much needed attention to the vast confusion revealed in

the hearings regarding the relationship between social science and ethics. As Professor Ogburn correctly pointed out, it is not the province of the scientist to say whether the substance he makes shall be used for spraying fruit trees or for killing human beings:

DR. OGBURN: He can say that as a human being if he wishes to, but his science begins with making the product and making the gas and stops there He may choose as a hypothesis to work on a poison gas which will kill insects, and he does that, it seems to me, on a value basis. Social sciences were greatly confused by the mixing in of values with the consideration of knowledge I think if that distinction is kept clear, the role of the social scientist is seen very much better (769).

This point of view was reinforced by Monsignor John M. Cooper, Professor of Anthropology, Catholic University, in the following words:

FATHER COOPER: Although anthropologists, like their confreres in the social and other sciences, are more or less agreed on the philosophical assumption of the dignity and value of the human personality, they do not in the name of their science sponsor any specific philosophy of life . . . (778).

The Reverend J. Hugh O'Donnell, President of the University of Notre Dame, on the other hand, was worried about the threat which federal subsidy might constitute to the independence of higher education. He said he would have no objection to the inclusion of social sciences, provided that "any kind of agency that is established for social sciences be conducted by capable social scientists who have a philosophy that is basic, with a recognition of certain fundamental truths as they relate to society" (454).

5. *The atomic bomb should frighten people into effective social organization.* Closely related to the belief in education as of itself a solution to social problems was the recurrent idea before the hearings that, if the horrors of the atomic bomb could only be brought home sufficiently vividly to every hamlet and family, the solution of social problems, especially international problems, would somehow mysteriously be resolved. This appears to be merely another aspect of the same misapprehension that causes people to believe that the severity of punishment will prevent crime, although we know that when pickpockets were punished with death, other pickpockets operated extensively in the crowds that came to attend executions. The same erroneous assumption now finds expression in the notion that successful international organization will come about somehow by merely convincing enough people of the *need* for such organization. It is hardly necessary to point out that the epidemics that have swept over the human race have frequently been sufficiently terrifying to cause nearly everyone to go about in imminent fear of his life. People were certainly convinced of the need and the desirability of a remedy. Yet the need and the fear could not possibly have produced sulfa drugs except as the felt need actually found expression in scientific research. The following colloquy between Dr. Rabi and Senator Fulbright appears to proceed quite confidently on the opposite assumption. Dr. Rabi had just emphasized that to be fully impressed with the results of the atomic bomb one must see firsthand its actual consequences.

DR. RABI: It's like death. One doesn't really believe it until after it happens. . . . I think young people are not afraid of death because it is far off and no close member of the family and so on has died. After they have had some experience with that, then they feel differently. I think it is this very thing and I think it is as fundamental as that. Therefore I believe that actual demonstrations in some form that would bring the immediacy of the problem to the American people first of all and to the people of the world afterward, would lead to a real solution. In other words, they would get to a state of mind where they would not be satisfied with half-way measures. They would want to make sure that they

are not going to see this thing coming at them.

SENATOR FULBRIGHT: I certainly subscribe to that, but I am still puzzled as to how to do it, how to carry it into effect, how to do the demonstration and the mechanics of it.

DR. RABI: Oh, the Army could arrange that.

SENATOR FULBRIGHT: You think the Army could arrange that?

DR. RABI: Oh, sure, beautifully.

SENATOR FULBRIGHT: I shouldn't think a demonstration on the water would have that effect at all because it is too removed from the every day experiences of most people. I would think it should be as nearly like a city, like Kansas City or New York. Have you ever discussed this with the Army or with anyone? Are they contemplating any such demonstration, do you know?

DR. RABI: I haven't discussed this with the Army (996).

The reasoning seems to be that some of us are sure we know what the remedy is and that all that is needed is to shock enough people so as to agree with us. The idea that preventing war, and relieving the tensions that lead to war, might be technically as involved and difficult a problem as those that have to do with the development and the application of a comprehensive system of vaccination or a program of public health seems to have occurred to no one.

The only note of skepticism about this and other matters occurs in the testimony of Dr. L. Don Leet, of Harvard University, who was also connected with the production of the atomic bomb. Senator Fulbright had remarked that the real purpose of discussing the possibilities of the atomic bomb is to dramatize the effect which few people, he feels, fully appreciate:

DR. LEET: Yes; I feel that a larger percentage of the people are impressed than maybe you realize. You can talk to anyone on the street, and the first dozen words will bring the atomic bomb into consideration, and he turns pale.

SENATOR FULBRIGHT: Perhaps people are, but I see very few signs of anything effective being done about it in the international political field. That is the only thing that is important. If you have a few people on the street thinking it is a bad thing, it doesn't help any unless some action is taken in the political endeavor. Eventually that action is taken as the result of pressure from people in most cases. That is why it is important (1025).

Incidentally, Dr. Leet was the only physicist who suggested that perhaps the assumed effects of the atomic bomb had been somewhat exaggerated. But the general sentiment in the hearings was that if only enough people, and especially the physical scientists, would publicize their views as to the destructiveness of the bomb, much good would come of it. Thus, Dr. Harry Grundfest, Secretary, American Association of Scientific Workers, expressed great hopefulness over the fact that many scientists have finally become politically active for the first time. He mentioned that under the sponsorship of his Association a statement was signed in Philadelphia by more than 600 people.

DR. GRUNDFEST: It has grown even larger than that. Within a week and a half—the last report I had was on Wednesday—they had already had 800 signatures, and probably by now it is well over a thousand. The same thing is going on in Boston—over 500 people subscribed to a statement, essentially the same sort of thing, that you cannot keep this a secret; that you must have some form of international control . . . (1,028).

It is only fair to note that on the general subject of physicists' current advice on social organization, Dr. Oppenheimer had previously injected a word of caution. Commenting on the prepared statement of Dr. H. J. Curtis, of the Association of Oak Ridge Scientists of Clinton Laboratories, who was also prominent in the production of the atomic bomb, Dr. Oppenheimer made the following remark regarding the possible contribution of prominent physicists to the solution of social and political problems:

DR. OPPENHEIMER: But that contribution cannot be made by underestimating the difficulties, and I think the prepared statement which you read gives

an impression of political naivete on the part of the scientists which I would not like to see given, though it may correspond to the facts (328).

WE HAVE reviewed above the considerations that were urged against the inclusion of the social sciences in a comprehensive program of research in basic science as contemplated by the National Science Foundation. These views are significant because they are sincerely held by people of prominence and influence in science, education, and public affairs. A rough measure of the present status of the social sciences in the estimation of these people may, therefore, be secured from the statements reviewed.

Briefly, these views may be summarized as follows: (1) Man and his behavior are not a part of nature that can be studied as basic, "pure," natural science; the social sciences are inherently "applied" and concerned with ameliorative and exploitive techniques in the service of whatever tribal lore happens to be current. Social science, therefore, is a nondescript category consisting mainly of reformist and propagandist ideologies and isms. (2) The methods of the social sciences are so widely at variance with those of other sciences as to make it inadvisable to attempt to administer research in the social sciences under the same organization (*a*) for fear of discrediting the other sciences and (*b*) because people qualified to direct research in the other sciences would not be able to judge what constitutes valid or desirable social research. (3) Social research is especially in danger of falling a victim to pressure groups or of being corrupted by the government itself.[4] And, finally, (4) there is always in the background of the testimony reviewed, the traditional view that, after all, we know the solution of social problems through the historic pronouncements of seers and sages, past and contemporary, and all that is needed is more education to diffuse this lore and arouse moral fervor in its behalf. In connection with the last point, there is currently a widespread belief that if we only frighten people badly enough with the atomic bomb, they will forthwith become so changed in their nature and behavior as to insure the elimination of war.

The last of the above considerations is especially revealing regarding what is undoubtedly the dominant attitude at present as to the approach to social problems. It is a traditional position, the surviving supporters of which vaguely call themselves "humanists." At least one of their spokesmen frankly advocated that the notion of including social science in the National Science Foundation should be abandoned, and that instead a "humanities" division should be created, which specifically "would not engage in research" but rather "stir concern . . . for teaching . . . the accumulated wisdom of our civilization." In short, although research and the advancement of science have been the principal conditions for the improvement of our adjustments to the physical world, we already know the answers to social problems, or at least can find these answers by simply consulting seers and sages of ancient and contemporary times, including poets, playwrights, novelists, newspaper columnists, and radio commentators. This whole attitude and approach represents a far greater menace to civilization than does the atomic bomb. For the assumption that we know the answers to the basic sociological questions and that the latter are not amenable to scientific research, closes the door against the only approach, other than blundering trial and error, which can possibly avail.

The state of mind reviewed in this paper will indicate to social scientists the task before them:

(1) In the first place, if social scientists

aspire to the status and position and public estimation of other scientists, they must subject themselves to standards of the kind recognized by other scientists and by the public. That is, they must specify criteria that distinguish social scientists from that vast array of camp followers, reformers, propagandists, and social workers, which today dominate even most of the professional organizations of social scientists. The first step in this process is for social and other scientists themselves to make up their minds regarding the proper function of scientists as contrasted with the functions of citizens. Large numbers of both physical and social scientists are today not at all clear on that point. Many of them are firmly convinced that it is the peculiar function of social scientists especially, not only to describe reliably the costs and consequences of alternative courses of action, but also to dictate public policy. Indeed, this group is not infrequently scornful of the scientist who scrupulously distinguishes his scientific role from other interests which occupy him. They fondly imagine themselves to be the chief defenders of science. The hearings and the debate on the bill to establish a National Science Foundation clearly brought out that they are actually the worst enemies of the social sciences at the present time because it is precisely this failure on the part of social scientists to recognize their proper function as scientists which caused most of the objection to the inclusion of these sciences in the National Science Foundation. This objection must be recognized as a valid one, and the responsibility for the situation lies squarely at the door of social scientists themselves, who have been careless of their scientific reputation in a number of ways: Through lack of clarity or lack of intellectual integrity they have failed to make clear to the public when they have spoken as scientists and when they have spoken as propagandists and as citizens. They have posed as social scientists, and frequently claimed academic immunity as such, while actually engaging in ordinary pressure group activity. Finally, they have been careless in distinguishing between scientific research and special pleading.

(2) In the second place, social scientists must submit examples of research that are recognized as scientific research by other scientists. This they are in a position to do to an increasing degree.[5] The worry revealed in the hearings about the possible incapacity of physical scientists as administrators of a research foundation properly to appreciate and judge social research seems to me largely unfounded. On the contrary, I am satisfied that really scientific social research has a better chance for promotion under such administration than under a board composed largely of historians and humanists of the type whose testimony has been reviewed above and who not infrequently dominate the social research wings of important private foundations.

(3) In the third place, and incidentally, Section K of the A.A.A.S. should aim more strictly than at present to represent social science as science and to avoid giving grounds for the sort of criticism revealed in the hearings. It may be that the Association should have a section devoted to Ethics, Planning, and Social Policy and thus avoid the confusion which results from including these topics with the social sciences.

(4) Finally, education from the grades upward must be revised in the direction of a more comprehensive and thorough teaching of the nature of scientific method. There is ample evidence that in spite of the lip service to science and the increasingly dominating role which science plays in our time, no considerable proportion of the

population which goes through our present schools gains any adequate grasp whatever as to the nature of the method that is science. A recent investigation at the University of Iowa[6] revealed that the activity we call research is not well understood either by community leaders or by the students coming up through our schools; that research is not considered as an important function in society; and that research is not looked upon as an important method of solving social problems. Our proper concern with the importance of education and literacy has caused us to neglect the equally important consideration of the development of knowledge worth communicating through education. The importance of education must be measured not only by its extent, but by its *validity* and its *relevance* to the problems for which it is supposed to be a help. There is every evidence that much of present education is both invalid and irrelevant.[7]

Proposals to establish a National Science Foundation having again come before Congress, the above review should indicate the principal obstacles which they may encounter.

NOTES

1. *Hearings Before a Subcommittee of the Committee on Military Affairs*, U. S. Senate, 79th Congress, first session, Parts I–IV. Numbers in parentheses following quotations refer to pages in this report or in the *Congressional Record*.
2. The vote on Senator Hart's amendment to exclude the social sciences was 46 to 26. It was announced that had certain absent senators been present this vote would have been 47 to 29. The attitude of the additional 20 absent senators is not known. The following senators voted against Hart's amendment and therefore in favor of including the social sciences: Aiken, Barkley, Chaney, Downey, Ferguson, Guffey, Hayden, Hill, Kilgore, La Follette, Langer, Lucas, McCarran, Magnuson, Mitchell, Morse, Murdock, Murray, Myers, O'Mahoney, Pepper, Radcliffe, Taylor, Thomas, Tunnell, Wagner. The following absent senators were also announced as favoring the inclusion of the social sciences: Mead, Green, Fulbright.
3. For a brief popular exposition see LUNDBERG, GEO. A. Can Science Save Us? *Harper's*, December 1945. Also a book by the same title, Longmans, Green, 1947. For a more thorough discussion, the following books by the same author give the basic argument as well as extensive references to the supporting literature, including references to recent relevant research: *Foundations of Sociology*, Macmillan, 1939; *Social Research*, Longmans, Green, 2nd Ed., 1942. See also LUNDBERG, G. A. The Growth of Scientific Method. *Amer. J. Soc*, May 1945.
4. I have considered this question in my paper, The Social Sciences in the Post-War Era. *Sociometry*, **8**, 137–149, 1945.
5. For a review of some such research, see LUNDBERG, G A. The Growth of Scientific Method. *Amer J Soc*, **50**, 502–513, 1945.
6. OJEMANN, R H. The Cultural Understanding and Appreciation of the Scientific Approach. *Science*, **104**, 335–338, 1946. For a brilliant analysis of the failure of education to equip people with even elementary realism about social organization, see PELCOVITZ, N. A. World Government Now? *Harper's*, November 1946.
7. For specific suggestions for the revision of educational curricula, see LUNDBERG, G. A What to Do With the Humanities. *Harper's*, June 1943.

THE UNIVERSITY PRESSES:

THEIR FUNCTION

By ROLLIN D. HEMENS

The University of Chicago Press

AN ever-increasing number of persons are becoming aware that there is such an organization as a university press. This is due, in part, to the increase in the number of university presses—they have about doubled in recent years. More than that, the university press is now doing a professional job of publishing. The result is an increase in review attention, better and more extensive advertising, and a substantial growth in the number of people reading university press books. In spite of this, however, a vast majority do not know what a university press is and what it is supposed to do.

Frequently someone will say: "The University Press? Oh, yes, you publish the student daily paper and the alumni magazine." Others will begin talking about the technical problems of printing and of running a printing plant. One business executive indicated that he thought a university press was some kind of a machine punch-press operation connected with a school of engineering. Fortunately, most scientists think of the university press as the publishing organization that it is. Some scientists have definite ideas as to what a press should publish and are known to have alluded to a particular title as "a typical university press book." However, there is no generally accepted definition of this "typical" book. Usually it is referred to as sound—I suspect that the person really means "dull"—a book which will have a limited sale. One person recently answered the question in this manner: "It is a book which has been well handled; by that I mean well edited, well indexed, and well printed." He made no reference to subject matter, footnotes—or sales.

If we examine the activities of the presses to get an answer to the question, What is the function of a university press? we are likely to become confused. There are, roughly, sixty university presses in the United States and Canada. Many of them are small, their sole operation consisting of the printing of college announcements, class schedules, stationery, and such material. Others regularly publish books and magazines, which are produced in large and well-equipped printing plants owned by the same press. Still other university presses limit their activities to publishing; they do not operate a printing plant.

Thirty-four of these are members of the Association of American University Presses. Each publishes five or more books a year, and some also issue scholarly journals. A few of these presses have their own printing plants which do book work; Harvard, California, Stanford, and Princeton are examples. Others, like Columbia, North Carolina, and Minnesota, have all their printing done by commercial houses. Publication of books is the common characteristic of these thirty-four members of the Association.

An examination of the catalogues of these university presses in the hope of finding common characteristics which will help define their functions and indicate basic differences between them and commercial publishers proves disappointing. On Harvard's list will be found Chinese dictionaries and on Chicago's *A Dictionary of American English* and Smith-Goodspeed: *The Bible: An American Translation*. Yale has a series in poetry; Columbia, a one-volume encyclopedia; North Carolina and Princeton, fiction; and New Mexico, juveniles. There is

a large variety on each list, but in total there are college, elementary-, and high-school textbooks, medical books, Bibles, encyclopedias, art books, poetry, fiction, books on architecture, politics, literature, and so on through the entire gamut of publishing.

A scanning of the list of authors is not much more helpful. It is true, of course, that university professors are the largest single group represented. But there are, in addition, professional writers, businessmen, men and women in government service, lawyers, and others. This again exhibits a marked similarity to commercial publishing houses.

IF THERE is so much similarity, you may ask, between the university press and the commercial house—if the books published and the authors who write them are essentially the same—what is the unique function of the university press? How does it differ from the commercial house?

First, a university press is a nonprofit organization. It is either a department of, or a separate corporation wholly controlled by, the university whose name it bears. Its objective is education rather than profit. To be sure, many university press books are profitable, and some are selected because they are expected to yield a profit. Frequently a university press will seek profitable titles as a regular part of its annual publishing program. The surplus income is then used to subsidize important but unprofitable books and journals. Presumably, manuscripts which are expected to yield a profit are checked as carefully as the unprofitable ones to make certain that they are scholarly, competent, and a new contribution to human knowledge.

Second, the university press disseminates the results of scholarship and science. The sole purpose of the early university presses in this country was to complete the research of scholars and scientists. At the time, the universities found that, after spending thousands of dollars on research, there existed no adequate means of making the results available to more than a few of the scholar's personal acquaintances. It was difficult and time-consuming for a Harvard astronomer to know what a Chicago colleague was doing in the same field. It was virtually years before there was exchange of information between continents. Therefore, the university press was established to perform that final step of research and scholarship—publication.

Books in the category of scholarly writing are not necessarily limited to titles such as *The Genitive of Value in Latin and Other Constructions with Verbs of Rating* or *An Attempt To Frame a Working Hypothesis of the Cause of Glacial Periods on an Atmospheric Basis.* Nor are all titles as specialized as *A Reverse Index of Greek Nouns and Adjectives* or *A Study of the Spectra of 7e Aurigae.* Another type of scholarly work is represented by *A Dictionary of American English on Historical Principles* compiled under the editorship of Sir William Craigie. It is a monumental work planned for scholars. In preparing the *Dictionary*, the editors attempted to collect, select, and present "all that is really significant for the history of the American language," and it will be a steppingstone for research for many years. Moreover, the *Dictionary* has already proved its usefulness to writers in all fields. It reveals, for example, that the word "lumberman," meaning a man engaged in the lumbering business, was first used in American speech in 1817; therefore, it would be an anachronism in an authentic play of an earlier date.

Daring teachers frequently break with tradition and in their respective subjects develop methods of teaching which they believe better adapted to the needs of today's student. Often their ideas should be made available in book form for study and experiment elsewhere. Occasionally, the materials used for teaching should be

published, presumably, at first, in experimental editions. The New Plan of the College of the University of Chicago several years ago—and the series of New Plan textbooks in the physical sciences and in the biological sciences—is an example of this type of experiment which results in publication of new textbooks. The objective of the courses was to give all students, regardless of their intended specialization, a general introduction to the natural sciences. In developing the courses, the faculty discovered that textbooks for the traditional course were wholly unsuited for their use. Therefore, they prepared new books—and in some cases integrated sound motion pictures with them. There are good reasons why this experimental publishing is not undertaken by the commercial house but left to the university press. In the case of the New Plan textbooks, for example, there was no assurance that the books would be used elsewhere—or, for that matter, very long at Chicago. The investment in production was large, and the sales problems were unique. In this instance the experiment was so successful that the books are used in many of our colleges and universities. Some of the titles have been translated into Spanish or Portuguese or both, and one has been republished in Australia. Trade editions of several of the titles, moreover, have had a satisfactory sale to the general public.

Third, the university presses are pioneering in the field of adult education. Thus, publication for the academic and scholarly market is no longer the sole interest of many of them. Along with most universities, the presses have become interested in adult education. More and more, they are undertaking to select manuscripts which tell the lay reader what the research worker is finding in the many fields of knowledge. Because this activity is new to university presses, they are doing it less well than they are the publishing of scholarly books and journals. In addition, there appears to me to be some confusion as to objectives. A few of the presses have not undertaken the publication of any books in adult education; others have gone into the open market for authors and subjects which are thought to stand a good chance of being a Book-of-the-Month Club selection or of getting on the *New York Times* or *New York Herald Tribune* best-seller list.

There is nothing wrong about a university press book being selected by the Book-of-the-Month Club or the Literary Guild, or being on a list of best sellers. The recent selection of Paul Angle's *Lincoln Reader*, published by the Rutgers University Press, illustrates this quite well. However, in selecting manuscripts with both eyes on possible book-club selection, the university press enters directly into a field in which the commercial publisher has the advantage in experience and resources. But, more important, the university press, in so doing, turns its back on the opportunities and the responsibilities which are peculiarly its own.

As a nonprofit organization, bearing the name of a university, the press has a first responsibility to contribute to education and human knowledge. To do this it has access to the total resources of the university—resources not so readily available to commercial publishers. Almost every campus is a storehouse of jewels, most of them in the rough or buried deep in the dust of academic verbiage and "stuffed-shirtage." The director of a university press has a rare opportunity of easy access to this storehouse. Each jewel he unearths will need cutting and polishing. Some will be for the crown of science; others will be for the laborer's tool or for the immigrant's understanding of American life. Many will be found to have blemishes, and others may be chipped, but in the lot will be more than one stone of exceptional value.

The university presses have much to learn before they can be successful in pub-

lishing books informing any large section of the American public about the discoveries and research on university campuses. The subject matter of most of their trade books—that is, books expected to be sold through bookstores to the general public—is interesting and, many times, exciting. However, most of the titles sell only a few thousand copies. A sale of fifty thousand copies is considered exceptionally good; and a hundred thousand, stupendous. But these sales barely scratch the surface of the nonfiction reading public represented by the colossal weekly sale of such magazines as *Time*, *Life*, *Newsweek*, etc., and the monthly sale of *Reader's Digest*.

The problem of the university press is to reach that large group of our population which is intensely interested in knowing what is being thought and discovered on university campuses. The farmer, the mechanic, the saleswoman, the factory worker, the milkman, all want to keep abreast of advanced thinking. But each of them is too tired at day's end to read with a book in one hand, a dictionary in the other, and an encyclopedia in front of him. Advertisers and motion-picture and radio producers are acutely aware of this and take it into account when making their presentations. As a result, they undoubtedly have greater direct influence on the thinking of the mass population than do all the books ever published by all our university presses.

Fourth, the publishing of experimental or exploratory subject matter has long been considered a function of our university presses. I hope that someday one of them will have the imagination, the money, and the fortitude to forget tradition in preparing its books for the public. The University of Chicago Press went part way some years ago when it published *Animals without Backbones*, by Ralph Buchsbaum, and *From Galileo to Cosmic Rays*, by Harvey B. Lemon. But, on the whole, the presses hold to tradition—a tradition which gives first consideration to conformity to conventional patterns of bookmaking. If the lead of other industries were followed, the manuscripts would be considered as a collection of ideas. All modern techniques of style, illustration, typography, layout, color, and the like would be adapted to making the best possible package for delivering the ideas to the people to whom they are addressed. Such a program would likely mean adding to the staff of each press a person trained in the technique of advertising illustration and layout and a person, possibly a newspaper writer or an advertising copywriter, trained in writing for the lay reader. These persons would work with the publisher's regular editor, designer, and layout man in determining the design, style, illustration, and text best suited for presenting the author's ideas.

There are many other conceptions of the function of the university press. Some persons think of it as principally a publicity agency for the university; some believe that it should publish books to be distributed free by the author or by the college library; others feel that it should issue the writing of any faculty member provided he pays the cost of publication. This may be so. But it is my belief that the first responsibility of a university press is publication of scholarly writing for the advancement of scholarship and science. This publication would include books and magazines and textbooks as well as monographs. A further function would be the publication of books of inherent excellence which present the findings and thoughts of scholars for the education of the lay, or nonprofessional, reader. Another objective would be the maintenance of an attitude of experimentation and exploration in order that the presses may advance the frontiers of publishing as the scientists advance those of learning.

THE UNIVERSITY PRESSES
AND THE POPULARIZATION OF SCIENCE

By HERBERT S. BAILEY, JR.

Princeton University Press

A DISTINGUISHED mathematician recently told me that when he received a prize for his "Investigations in Hilbert Space," a newspaper reporter came to interview him. The mathematician obligingly received the reporter and patiently explained to him in simple terms what Hilbert space is. When he had finished, the reporter asked, "Does this mean, sir, that you do not believe in God?"

This extreme case explains clearly why scientists are so reluctant to talk to reporters. Newspapermen often will not take a "straight" story and print it; they must have an "angle"—something sensational. And the scientist who permits an interview is amazed to see in the morning headlines:

MATH WIZARD PROVES ATHEISM

A scientist who has the uncomfortable experience of being probed for sensational statements and of seeing his words twisted in the published report is likely to return to his laboratory vowing never to submit to an interview again. He will stick to his scalpel or his test tubes or his cyclotron; and the public, though it may receive the *material* results of his work, will not for a long time have the benefit of the *ideas* that he produces.

If the scientist does this, he is neglecting an important responsibility to society, for there is a real urge among the educated public to know the advances of science, and there is a real need for them to know. In the long run, moreover, it is at least as important foı scientists to have their activities understood by the public as it is for the public to understand science. Then if the scientist refuses to trust popular expression of his ideas to a professional writer, the only alternative is to write them himself.

But there is another factor which prevents scientists from expressing their ideas so that they can be understood by laymen, and this is the connotation of the word "popular." Owing to abuses such as those mentioned above, "popular" in the minds of many scientists has come to mean "vulgar" or "debased." This idea should be cast aside; there is no need to vulgarize or debase scientific knowledge in order to spread it widely. A few examples of successful scientific popularizers will suffice: Albert Einstein (with Leopold Infeld, *The Evolution of Physics*) and George Gamow in physics; Harlow Shapley and Sir Arthur Eddington in astronomy; George Corner and Carl Binger in biology and medicine. No scientist need be ashamed to add his name to this group.

I have said that the scientist has an important responsibility to explain his activities to society as well as to give the public the material benefits of his work. For readers of THE SCIENTIFIC MONTHLY this hardly needs elaboration, for the MONTHLY is devoted to presenting the ideas of one field of science to workers in another, and to presenting the ideas of all sciences to educated lay readers. But someone will object, "The educated lay reader is a far cry from the general public." True, but the process of spreading scientific knowledge and attitudes cannot be done all at once. It must be done in steps at different levels. Those best able to receive knowledge must get it first, and the ideas will spread through the population. The various degrees of popularization may be

seen first from *Science*, then THE SCIENTIFIC MONTHLY, and then such magazines as *Science News Letter* and *Science Illustrated*. Each of these is good in its own way and is useful in its special function.

Some will object that the broad general public does not really want to know about science. It is of course true that the general public does not have the same drive toward knowledge of the physical world that the professional scientist has, but there are a thousand evidences that the public is curious about science. Scientists must capitalize on this curiosity in order to present scientific ideas and attitudes that the public *needs* to know.

But does the public really need to know about science? Automobiles, refrigerators, electric lights, radios, and even atomic bombs are accepted and used by the public, who know not whence these miracles came. The housewife does not need to know about thermodynamics in order to appreciate an electric refrigerator.

The material benefits of science do not usually require scientific knowledge of the users, but the ideas and attitudes of science are at least as important as its material benefits. Witness the transformation of the world in the three hundred years since Sir Isaac Newton formulated his laws of motion. The modern world is built on scientific foundations; the machines used in the construction were scientific, and the ideas which guided the machines were also scientific. It was not necessary for new scientific ideas to have practical uses for them to cause great transformations. The general critical attitude of science toward all phenomena has driven witchcraft into the unenlightened corners of the world. Some of us may regret that we no longer have leprechauns and fairies, but in place of constant fear of supernatural events we now have scientific explanations of earthquakes, plagues, lightning, and the like. We can deal with these phenomena logically and, where we cannot control them, we at least can understand. Alchemy has disappeared and we have chemistry in its place. Astrology has fled before the increasing knowledge of astronomy in the public mind. No one who has heard a good description of the solar system can believe (except figuratively) that his destiny is controlled by Venus. The fact that many people still believe in horoscopes is a symptom of psychological maladjustment in the modern world, and the astrologers, and especially the newspapers who nourish this symptom by casting and publishing horoscopes, are greatly to be blamed. Astronomy has produced no refrigerators or automobiles, but its indirect effects through its descriptions and explanations of the universe have been tremendous.

Another example of an idea without specific practical applications which has had great indirect effects is that of biological evolution. Although technical knowledge of evolution, especially in the past ten years, has advanced greatly since its beginnings in about 1800, the basic ideas have not changed very much; and the false egotistical doctrines of the theory of special creation have fallen away as we have achieved a truer understanding of man's place in nature. The spread of the theory of evolution is an especially good example of the need for explaining science to laymen. None but the most stubborn can today deny the theory of evolution, and the idea has spread through every stratum of society; here we should mention the name of another great scientific popularizer, Thomas Henry Huxley.

Witchcraft, alchemy, astrology, and the theory of special creation have been killed by science; what false belief can science attack next? I submit that one of our gravest national problems which is directly vulnerable to the attack of present scientific knowledge is that of race prejudice.

If a skillful scientific popularizer could teach this nation to accept the ideas of genetics as it has accepted the theory of evolution, race prejudice would quickly die. Any man exercising racial discrimination would be uneasy in his heart, subconsciously knowing that the basis of his action could not be justified in fact, and he would be subject to the ridicule of his fellows. Note that the condition required to reach this state is not the advancement of science but the spread of scientific ideas through the population.

Popularization of science is needed for still another reason, for the good of science itself. Scientific research has been passing through a difficult period since the end of World War II because there is no government department primarily concerned with the advancement of science. Several National Science Foundation bills were presented to Congress in 1946, but none was passed, mainly because Congress, reflecting the opinions of the people, did not realize how necessary a National Science Foundation is. Fortunately the Office of Naval Research jumped into the breach and did a job it was not designed to do, supporting the efforts of pure science. It is hoped that a National Science Foundation will be established this year, but even then it cannot be fully effective unless it is understood and supported by public opinion, and this public opinion depends on general knowledge of science and the methods of science. All good popular scientific books help toward this end, and some, such as *Deserts on the March*, by Paul B. Sears (Oklahoma), an extremely influential book of a decade ago, are directly concerned with public policy. In the United States there is a traditional distrust of government spending; immediate practical results and frequent accountings in dollars and cents are expected. The public must be made to realize that scientific research is always a gamble, that results are slow and uncertain and often not immediately practical, but that science is worth supporting for the knowledge it gives us of ourselves and the world as well as for the unforeseen material benefits which come with this knowledge.

There is another more philosophical and more controversial reason for making every effort to give laymen of every country basic scientific ideas at a level that they can understand. F. S. C. Northrop has recently elaborated in *The Meeting of East and West* his belief that the world's ideological conflicts are based on different ontologies arising from largely outmoded science. He believes that the ideological disagreements can be straightened out only by re-examining the fundamental scientific assumptions on which the ideologies are based and by finding where they are obsolete or inconsistent and establishing a new common ground based on the best scientific knowledge. It seems obvious that, if Northrop is right, his plan can be effective only if the general public of the nations involved is well informed about science. But many scientists do not agree that science can lead to a unique ontology or that it can form a basis for ethical standards. Nevertheless, all agree that the epistemology of science is a great common ground between nations. On this ground scientists of all nations meet as friendly co-workers, and education of the public in the methods and attitudes of science may lead nations to do the same.

It is encouraging to note that the epistemology of science is spreading, though it has sometimes entered fields in which it does not belong. In addition to the physical and life sciences, we now have a whole group of so-called social sciences, with psychology as a bridge. These fields of study have adopted the name and some of the methods of science. Thus we hear of political science, the science of history, the science of economics, and even domestic science. Experts in the humanities may

resent this intrusion, but the spread of the critical attitudes and experimental methods of science has been a useful trend.

The ideas of science are important to everyone, so let the scientist speak forth from his "Ivory Lab," as Jacques Barzun calls it, and let us know not only what he is doing but also what he is thinking. For ideas inconsistent and incompatible with the truth as given to us by science must inevitably die.

I HAVE come a good way through this essay on "The University Presses and the Popularization of Science" with hardly a mention of the university presses. But writing comes before publication, and I am sure that all university presses will back me up in saying that the most important and most difficult part of publishing a good popular scientific book is to persuade a scientist to write it. Any geneticist who wants to refute this statement by writing a popular book on the nature of race will please step forward.

What, then, are the special abilities and special reasons for publication of popular scientific books by university presses? In discussing this topic I can speak only for Princeton University Press, but the principles under which most university presses operate are very much alike. Many people think of university presses as publishers of doctors' dissertations, textbooks, and esoteric tomes. This is a false picture. Most doctoral dissertations are not publishable as such, although they may contain the foundations of future good books. Many university presses, Princeton among them, do not publish textbooks except under special circumstances, leaving this task to the large commercial textbook houses, which are provided with scores of special textbook salesmen. University presses publish a fair number of esoteric books. It is their responsibility to publish these books, but these do not by any means monopolize their lists. The university press lists contain many nonfiction books of sound scholarship intended for the college-educated. Many of these books do not require special training on the part of the reader, but they are written at a level that requires more of the reader than the average book of the average general publisher. There are exceptions on both sides of this broad generalization.

The name of the university on the imprint of a university press book should be a guarantee of accurate and sound scholarship. I believe that for many kinds of scholarly books the university press imprints can be more effective than other publishers' imprints because a university is more than a name on the title page; it is a place whose reputation, and possibly whose halls, are known to the reader. University presses are organized in different ways, but nearly all have a committee composed of members of the faculty who decide purely on the basis of quality and without reference to possible profit or loss whether a book may bear the name of the university. University presses have split personalities; we do not let our right hand know what our left is doing. As soon as a book is approved on the basis of quality, we try to publish it in a way that will sell as many copies as possible. In their advertising and sales departments the university presses operate in very much the same way as the commercial houses.

Why don't the university presses go bankrupt? There are several answers. Most of the presses are subsidized by their universities. Some money-losing books are subsidized or underwritten by individuals or foundations, though this is not so frequent as is sometimes supposed. Some presses, like Princeton, have their own printing plants, the profits of which help to pay for money-losing books. A few presses have tried to operate on the prin-

ciple that any truly good book can stand on its own feet and pay for itself, but this is disastrous because it simply is not so. Many important books are money-losers, but it is the responsibility of the university presses to publish them anyway. The answer for most university presses is that some books lose money and some books make money, and we do our best to come out even in the end. Popular scientific books do fairly well for the university presses in this regard, and thus incidentally help to pay for the very specialized, money-losing scientific books that are necessary for the production of more knowledge, later to be popularized. But the possibility of profit from a popular scientific book should never be a justification for its publication, for a university press should not lower its standards of quality in order to admit a money-making title. Some people think that university presses should publish only money-losing books, but this would be impossible unless the presses printed money as well as books.

Because many people think of popular science as "debased" or "vulgar," it is frequently a real advantage to a popular scientific book to be published by a university press rather than by another publisher. The name of the university on the title page counteracts the suspicion of inferior quality. *Ourselves Unborn* (Yale), *Why Smash Atoms?* (Harvard), *Crater Lake* (California), and *How to Solve It, a New Aspect of Mathematical Method* (Princeton) might provoke suspicion as debased science, but their authors are respectively Director of the Department of Embryology of the Carnegie Institution, Assistant Professor of Physical Chemistry at the Harvard Medical School, Professor of Geology at the University of California, and Professor of Mathematics at Stanford University.

The wide and varied resources of the university are freely available to the university presses in their publishing enterprise. If we are publishing a book on mathematics that we feel should be read by economists, we ask the members of the economics department for advice. Where shall we advertise this book? Where should we send copies for review? Are there any special mailing lists that we should use for circulars and catalogues? What other special groups would be interested? The answers are readily forthcoming, and, in spite of the myth about impractical professors, they produce practical results. In this way we can reach a varied audience and help to bring about the cross-fertilization in the sciences that aids scientific progress. But this kind of cross-fertilization cannot ordinarily be done at the technical level used within each special field. Each scientist is at least partially a layman when he steps beyond his specialty. Interscience writing is a kind of popularization. The need for this kind of writing is shown by the importance of such journals as THE SCIENTIFIC MONTHLY and *The American Scientist*, and a good popular scientific book can perform the same function in more detail, making the ideas of one field of science easily available to all others.

It is generally recognized that the university presses are doing an important job for scholarship by publishing books for specialists, but it is not so often realized that the university presses are doing an important job for society by distributing the products of scholarship widely. Every university press reaches many thousands more readers every year than are contained in its university's enrollment. This is not to say that the university presses are disseminating more knowledge than the universities, but they are disseminating it more widely. In scientific fields they are doing a good and important job of popularization, along with their first responsibility of publishing the primary results of fundamental research.

OBSOLETE LIBRARY BOOKS

By CHARLES F. GOSNELL
The New York State Library, Albany

WHY books get out of date and what to do about out-of-date books is one of the most vexing concerns of the librarian. He is further vexed because, in terms of possible demands by the scholars he aims to serve, any book, no matter how obsolete, may be the essential link in a long chain of "documentation" for the "historical approach" to any conceivable subject.

Declining demand for older titles that have failed to achieve the status of classics is easy to see. In recognition of this declining demand the librarian can cast out duplicate copies. But the impulse to dispose of the last copy is usually countered by the certain knowledge that someday someone will have to have that book. Even the merest scraps of paper may someday be as vital as the Oxyrhynchus papyri. No librarian wants to be called, as some have been, "an enemy of books."

Yet no librarian wants to go on accumulating books indefinitely. It is a fundamental law of nature that indefinite accumulation must eventually lead to stagnation and collapse. Amid these many pressures, the librarian seeks through various forms of clairvoyance to foresee the relative intensity of future demands and to lay up treasure accordingly.

This is an account of an attempt that I made to get some figures on the actual rate of obsolescence of college library books. I wanted to plan for the balanced growth of the book collection and for the segregation and elimination of obsolete materials by replacing old with new.

Heretofore obsolescence has been considered principally with reference to individual and specific titles rather than to groups of books. A librarian could go through his stacks and pick out obsolete or useless volumes one by one. But no general library planning can be done, except on a quantitative basis, in the hundreds and thousands of titles.

Why could not a library full of books be treated like a group of people? An insurance actuary can turn to his tables and predict with striking accuracy how many will survive the coming years, and how long. Why not some similar analysis for books? Books are born, they grow old, and die, certainly as far as our interest in them is concerned.

Statistical bibliography is a relatively new or uncultivated field. But the astronomical proportions to which some of our libraries, their catalogues, and bibliographies in general are growing must force us to consider books as populations. A typical example of what can be done through this approach is the study of "certain biological properties" of literature by Wilson and Fred.

The causes of book mortality or obsolescence are many, varying from pure fad through extension of scientific knowledge and technological advances to fundamental changes in our civilization. The object of the present study is not to discover or to clarify these causes but to analyze their total effect. Deterioration or destruction of books is not true obsolescence. A book may be eternally bright and shiny, but this quality is often proof that nobody wants to *read* it. Hence, no consideration has been given to the physical lasting qualities of a book, but rather to its survival in interest and intellectual usefulness.

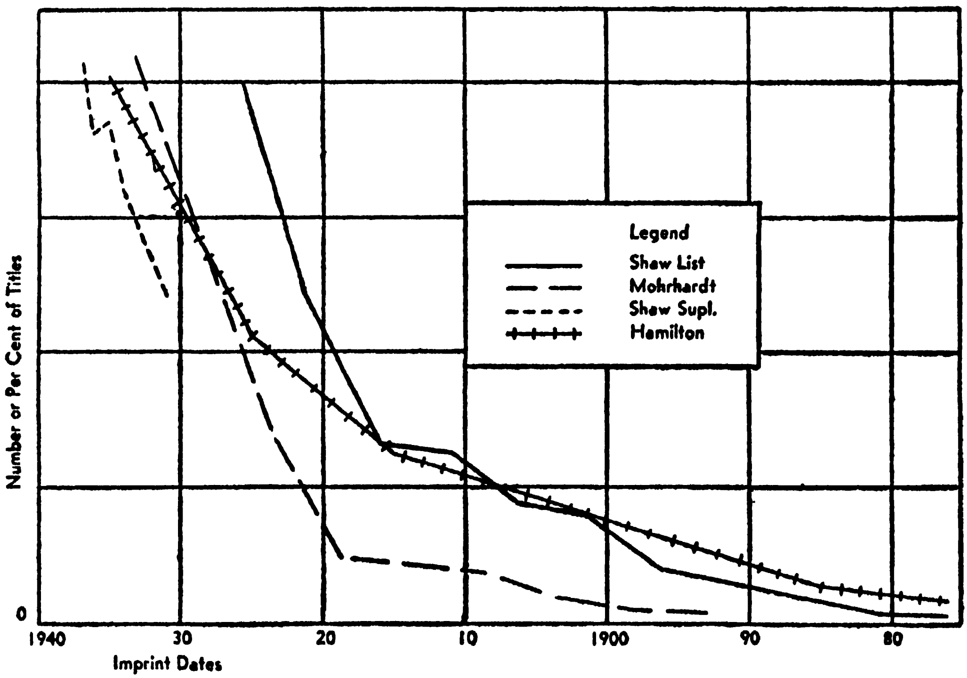
It is a common observation that most people prefer newer books to older and that most selective bibliographies accent the

more recent titles. That preference for newer titles or, conversely, that rejection of older titles is a common factor in many lists. I have sought to isolate it and put it into tangible figures.

WITHIN the past twenty years, college librarians have been fortunate in having had three comprehensive select lists of books prepared at different times. These lists were sponsored by the Carnegie Corporation and the American Library Association. Each roughly followed the same general canons of selection. These three lists or, properly, the publication dates of the books listed constitute the fundamental data of this study. The relative distribution of the dates suggests the rate of obsolescence.

Plotting of frequency curves for the imprint dates showed striking similarities for each of the three lists. First, it was evident that the compilers of the lists were a year or two slow in making their selections. There were few titles bearing dates of the year before each of the respective lists was published. The maximum number of titles per year did not occur until the third year preceding publication of the list. This lag in selection is presumably due to delay in appearance of reviews and lag in general acceptance of new titles by scholars. Accordingly, the figures for the latest year or two in each list were disregarded as without significance for obsolescence.

The diagram shows three curves for the three lists. The initial lag has been eliminated. Following the maximum years, there is a sharp drop in the number of titles per year appearing in each of the lists. This drop is rapid at first but becomes slower as the age of the remaining titles increases. It means that the older a book was, the less were its chances to be included. Many books that were included in the first list were dropped from the second. Many were included in the second and omitted in the



IMPRINT DATES OF BOOK TITLES

IN THE SELECT LISTS AND CIRCULATION AT HAMILTON COLLEGE.

third. The third list was intended to be a supplement to the first and hence had no tail to overlap the years represented by the first list.

The pattern of decline in preference for older books is repeated independently in each of these three lists and actually is found to be substantially the same in many other selections of books. It is not markedly affected by the rate of books produced each year. It is a function of the age of the books at the time each list was compiled.

From the fall in the curves, it is immediately evident that the older a title is at a given time of selection, the less is the likelihood that it will survive the selective process. Each preceding year back from the date of selection is represented by fewer titles. In one group it turned out that there were 100 titles twenty-three years old, 90 titles twenty-four years old, and 81 titles twenty-five years old. In the passing from age twenty-three to age twenty-four, 10 percent fewer titles remained. Likewise there were 10 percent fewer at age twenty-five than at age twenty-four. Therefore the rate of obsolescence for this group is 10 percent.

If a similar list of books had been prepared just a year later, it is evident that of the 100 titles in our first list, only 90 would have survived. For as the group of 100 became a year older, 10 percent of them would have lost their appeal to compilers of the new list.

It was desired to express the principle of obsolescence in a simple formula and to derive comparable coefficients for various subject groups of books. Pearson's criteria of moments, as well as the basic logic of the situation, indicated the type *X*, the exponential curve. Thus the curve of organic decay appears as the expression of obsolescence: $y_r = y_o b^t$. Where y_o is the number of titles at the maximum or initial point, with lag eliminated, b is a parameter expressive of the particular nature of the group of books, and t is time elapsed, then y_r is the number of titles that remains after passage of time t.

When $\omega = 1 - b$, *omega*, ω, becomes the annual rate of decrease in the curve, of the rate of obsolescence. Percent of decrease is expressed by 100 ω.

This rate is almost the exact opposite of the rate of compound interest. It indicates the rate at which the principal, or capital, is decreased or increased.

By shifting the equation to logarithmetic form, $\log y_r = \log y_o + t \log b$, the curve takes the form of a straight line. By the method of least squares, the straight line can be easily fitted to the date of imprint dates in logarithmetic form. And for simple graphic analysis and illustration, the imprint dates may be plotted directly on logarithmetic grids.

Through the method outlined, rates of obsolescence have been computed for the three select lists and nineteen subject subdivisions. For the Shaw *List*, including over 12,000 titles, the general rate of obsolescence was 8 percent. Individual subjects varied markedly in rate. Physical education was high, with 21.6 percent. Classics was low subject, with a rate of only 4 percent per year obsolescence. Chemistry and physics were in the upper group, with 12.9, astronomy and geology were toward the bottom, with a rate of 6.3, and mathematics was 6. The social sciences, except for history, were generally high, whereas the languages and philosophy were low.

There was substantial agreement in rates of obsolescence between the subject sections of the three lists. The rank correlation between the rates for the Shaw *List* and the Morhardt *List* was +.84. Further detailed analysis revealed no relationship between the total number of titles in a subject group and its ω, nor between the number of titles in its maximum year and the rate of obsolescence. The rate seems to be a property

peculiar to each subject and not a variable dependent upon some other continuous variable. No other ordinal relationship of the respective subject sections has been found to correlate with ω in either the positive or the negative.

There seems to be a slight tendency for some larger subjects to have a lower rate. Perhaps, conversely, a subject having a lower rate tends to accumulate more titles. This is true in English, history, and "general." Yet mathematics, music, and philosophy, all among the smaller groups of titles, fall low in ω.

Similarly, small subjects might be small because their titles do not long survive or because their rate of obsolescence was high. This may be the case with physical education and chemistry and physics. But geography, one of the smallest groups, has a medium rate. Mathematics, music, and philosophy are small in number of titles and have low rates.

In some fields, such as chemistry and physics, psychology, and mathematics, a large proportion of current research is published in professional journals rather than in books. Many of the books are either textbooks or handbooks and reference tools. The bearing of this fact on obsolescence is difficult to assess. From one point of view, the need for current publication of new material is met by the journals, and the output of new book titles is consequently reduced. Conversely, the rapid developments characterized by the journal articles must occasion frequent revisions and changes in textbooks and handbooks and thus should be reflected in a high rate of obsolescence.

It is quite possible that the omega for a given subject might change over a span of years. This change might be due to a general change in approach or in methodology in a subject; or to a sudden increase of interest or of expansion in the field. It has not been possible to isolate any such changes within the limits of the present study. The period of time covered is not long enough to show clearly any fundamental trends.

The possibility of a general shift in the rate during the period 1890–1920 is suggested by the yearly total for the Shaw and Mohrhardt *Lists* and in many subject sections. In the process of fitting the straight lines, it was found that nearly all curves rose above the straight lines for the period 1900–1910. In the succeeding ten-year period they fell below. This pattern, or cycle, is a function of contemporary conditions, not of the age of the titles.

The rise may be ascribed to a number of factors. There was an increased production of books in the period. There may have been an increased proportion of desirable books in the period; or some of the older books may have survived longer than usual because enough stronger titles did not appear in the war years immediately following. The drop in the next decade may be due to a reverse of these conditions. Certainly it was not due to a wartime drop in production, for there was a flood of publications in history and political science during the war years. It is more likely that the very urgency and immediacy of these publications in a critical period condemned them to short life.

In this connection, Sorokin has declared that "purely quantitative (astronomical) time cannot replace sociocultural time, and is inadequate for the study of sociocultural phenomena." As an example, he points out that "one year of existence of a modern social group is packed with more numerous and greater changes than are fifty years of existence of some isolated primitive tribe." Some investigators have studied a similar problem in the process of forgetting, where the nature and intensity of activities between the learning period and the retention test are factors.

Fitting the exponential equation to the data and computation of ω makes possible

certain generalizations regarding life expectancy and mortality of books. Of course no definite predictions can be made in terms of individual titles. But regarding a given group, predictions can be made with the same justification as they are in similar situations in life insurance and annuities.

The annual mortality for any given group is expressed by ω. The number of titles remaining in the group y_r after the lapse of time is given by: $y_r = y_o (1 - \omega)^t$.

It is possible to compute the time required to reduce an initial group to any given remainder and the average life, or life expectancy. There are many applications of the exponential equation which have been fully explored by workers in other fields and which need not be mentioned here.

The three select lists of books have been generally accepted as practical standards for college library collections. But no general principles which emerge from analysis of these lists can be accepted until they are compared and tested against actual library situations. To make these tests, samples were taken from the catalogues of five college libraries. The library samples showed generally lower coefficients, evidence that the libraries had not discarded old books as rapidly as the compilers of the ideal lists did. But there was strong agreement between libraries and lists insofar as the same subjects, such as chemistry and physics, were consistently high in rate of obsolescence, whereas history and the classics were low.

The select lists, and even the libraries, are made up in anticipation of need, but they are not actual expressions of need. There is good reason to believe that both the compilers of the lists and the librarians were practical choosers of books that faculty and students would want, but it is seldom indeed that an accurate check of reader demand is possible. Thousands of call slips are filled out daily, but they are never kept and classified or analyzed to find out the fundamental trends in reader demand.

An exceptionally detailed analysis of daily use was made over a three-year period by Lewis Stieg, librarian of Hamilton College. Among other items, he took the trouble to tabulate the publication or imprint dates of the books that college library readers asked for. A curve for one year's circulation has been plotted as a part of the diagram. The curves for two subsequent years are almost exactly the same. The chief difference is that in each succeeding year there is a slight displacement from the previous year, corresponding to what we should expect to happen as a result of a year's difference in time. The shape of the curve is the same, but it is moved over one unit, corresponding to one year.

Thus there is evidence that in actual use of a college library readers are interested in more recent books. The decline in interest per year, in the older books, falls like the decay curve which has been described. Loss of interest in older books is expressed as a process of exponential decay. It drops rapidly but never totally dies out.

From the Hamilton data it was possible to compute an ω, or rate of obsolescence, of 5 percent per year. This compares with 8 percent in the Shaw *List*. The lower rate for circulation of books at Hamilton may be accounted for by several circumstances. First, the collection there, nearly 200,000 volumes, was much larger than the lists and contained much more older material. Second, the instructional program of the college may have been planned to take advantage of greater resources by use of older materials beyond that contemplated by the compilers of the select lists. The compilers listed only 0.2 percent of books published prior to 1850, whereas at Hamilton 1.24 percent of the circulation was in titles published prior to 1850, including sixteenth- and seventeenth-century titles that are rarely found in a college library.

Some of the older titles may have been withdrawn because of interest in their physical form rather than for ordinary reading or reference use. For the purpose of this study of obsolescence, use of a Gutenberg Bible would not be significant. The Gutenberg Bible is noted as a physical specimen, a veritable museum piece. It is a rare scholar indeed who would or could read it because he wanted to read a Latin Bible.

But the fact remains that, when the librarians sat down to compile some ideal lists of books, they put less emphasis on older titles than a record of actual demand for these titles would justify. And, while they sought to concentrate on the latest and best editions, there is still a legitimate demand on the part of scholars and students for first editions and specimens of the earliest printings.

Obsolescence of books is a vast new field, and I am conscious of having scratched the surface of only one corner of this field. But I hope that I have turned up a few useful facts and some ideas. It seems worth while to list, for the librarian and scholar, some of the possibilities for future research and practical application.

The first task is to clarify and standardize the coefficients of obsolescence by wider application and study. My computation is presented as merely illustrative of the method rather than a final statement of fact. I do believe that many librarians can discard old books more freely than they have in the past. But what happens when a library does not discard old books is itself a problem for further study. By what curve or law the growth of libraries takes place and what may be expected in the future are open questions. There must be a saturation point for the largest libraries, but where it is nobody knows.

The distribution of imprint dates is a powerful tool for the analysis of book collections. Heretoforè, libraries have been compared with respect to size of their collections and annual additions or with respect to their holdings of specific titles. Now it should be possible to evaluate in quantitative terms at least one factor in the quality of book collections.

The extent to which the omega computed for a given library may differ from a generally accepted standard may be taken as an objective indication of its deviation from the norm. It must be remembered, however, that such figures as may be derived are no substitute for special expressed purposes or policies of a given library. If a library chooses to be different and has good reasons, therein lies the justification for deviation. But the deviation can still be accurately measured.

If, in a study of its collection of sociology, a college library finds an omega of .01, whereas the generally approved figure for comparable libraries is at least .08, there is an obvious deviation from the norm. The cause for this deviation may be that the library is not adding enough new books or that it is not discarding older material. If the generally accepted y_o for sociology is 40 titles within five years (that is, the maximum point on the curve) and the library has had only 20 titles at maximum, then it has not bought enough new books each year.

But if the library has been adding an average of 40 titles per year, the slight slope of the curve is due to failure to discard older materials. This failure may be examined in the light of the purposes of the library. If there is a deliberate policy of retaining deadwood for some definite purpose, such as "historical approach" to the subject, then no further justification is needed. But it must be remembered that the cost of housing and caring for the older material can legitimately be chargeable only to this special use.

An offer of a large gift of older material can be weighed with regard to what it will do to the present distribution of imprint

dates in a library. It might fill a gap left in the past or, more likely, it will increase the proportion of obsolescent material in the form of a camel's hump.

For the college library faced with the necessity of storing or setting aside the lesser-used portions of its collections, as suggested by many and practiced by few, the formula and rates may be used in planning what to segregate and in estimating the demand for the segregated material.

Assuming a rate of obsolescence of 5 percent, the minimum suggested by the circulation experience at Hamilton, the half life will be approximately fourteen years; that is, half of the useful collection will be in titles fourteen years old or less. Likewise three-fourths of the demand will be for books less than twenty-eight years old or certainly less than thirty years old. The average life, or life expectancy, would be about twenty years.

In many college libraries titles over thirty years old would comprise at least half of the collection. But only a small proportion would be in active or potential demand, probably not more than 10 percent. Thus 50 plus 10 percent, or 60 percent, of the collection might account for 90 to 95 percent of the circulation. Because the remaining 40 percent of the books would be used so little, these volumes might be removed to a less accessible place of storage with very little inconvenience; or they might be removed from service entirely, with a loss of only 5 to 10 percent of the total use of the library. For the undergraduate, a live and concentrated collection would be far more satisfactory. He would be spared much floundering among dead or misleading titles.

Within a library it may be desirable to compare several sections, with a view to determining relative need for book funds. In budgeting departmental purchases, it is important to know approximately how many titles are required each year in each subject, and to know that replacements in such fields as classics are needed less urgently than in certain of the sciences. On the other hand, the professor of classics may claim that money is more wisely spent on his books with longer life expectancy.

One of the least-explored areas of cost accounting is that for public and semipublic nonprofit institutions. Understanding and statement of the rate of obsolescence of library book collections in financial terms would be an important contribution toward the solution of this problem. Such information should also provide a useful lever in annual book budget requests. The librarian can readily demonstrate how many titles must be purchased annually to keep his collection up to the standards set for it.

The publisher has other phases of the accounting problem. He must decide when to dispose of unsold inventories of older books. His editors must decide when a book needs to be revised or even when a new book is needed in a given field.

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BOOKS—AGENTS OF WAR AND PEACE

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NATIONAL boundaries mean less, and books mean more, to scientists than to most men. The destruction of millions of books and the cutting of intellectual communications between countries during World War II present a vivid threat to American scientists, who closely share with their colleagues overseas a vital concern for the damage thus done to world learning. It seems fitting, therefore, that an American scientific publication should this year review the destruction of books throughout the world and American help in repairing the losses of this destruction and the equally damaging isolation of war years.

World War II differed from preceding wars in that it was very precisely tailored for the destruction of books and libraries. Its scale of operations was global; its theaters covered half the earth. Unlike the war of 1914–18, this was a war of mobility, and installations that were not ground to dust the first time the war machine passed over them often did not fare so well the second and succeeding times that they found themselves in the path of operations. It was a war of aircraft and aerial bombardment, in which the target was often indiscriminate. The concealed mine, the incendiary shell, the blockbuster, all were diabolical instruments to destroy the civilian target as well as the military objective.

The fact that this was a war which was inspired to destroy cultures accounted for the loss of more books than any other single factor. Books are not only the evidence but the very instruments of competing cultures, and as such they became not only weapons to wage war but also the enemy to be tagged for obliteration. This identification of the book as the enemy became apparent long before the beginning of hostilities when the first public burnings of books occurred in Germany in 1933. From then until 1945 the destruction of libraries and book manufactories had been a characteristic depredation systematically undertaken by both Germans and Japanese.

Under normal conditions the book is one of man's less perishable inventions. Even its precursors—the manuscript, the roll, the tablet—have often come through centuries in surprising quantity and in rather usable condition. The book and the library have been respected and safeguarded, even in war, as the very corporate mind of society. In 1939 man very literally turned upon himself and deliberately strove to destroy the physical instruments of that mind. He succeeded in a large measure.

The National Library of Yugoslavia in Belgrade, with its priceless collection of Serbian treasures, was destroyed by German incendiary bombs in 1941. In Italy the University and Pontiana libraries in Naples, the National Library in Palermo, the Civic Library in Turin, have disappeared in rubble. The archives of the city of Naples were burned by the Germans in reprisal; the great Columbiaria in Florence was blown up by the Germans when they mined the approaches to the Ponte Vecchio. The 700,000 volumes of

Charles University Library in Prague were stolen as a unit. In Russia casualties include the Ukrainian Academy of Sciences Library and both the State Historical and State University libraries of Kiev. Allied strategic bombing destroyed many of the great libraries of Germany, including the State Library in Berlin. The great publishing centers in London and Leipzig were obliterated.

These are but a handful of the great libraries which the world has lost. The complete list runs into hundreds—thousands—of libraries, large and small, in Europe and the Far East. Just what has been lost will, in many instances, never be known, for often the records of library and archive holdings were destroyed with them. Buildings can and will be replaced. The working tools of scientists and scholars, the archives that record the history of a city or a nation for centuries, the bibliographical treasures of entire cultures: many of them can never be replaced. Mankind will be forever poorer. Humanity will have to do some of its work a second time.

The entire subject of the destruction of books and libraries is a maze of paradoxical complications and contradictions. We must not forget that we ourselves, as the perfectors of aerial bombardment, are physically if not morally accountable for much of this devastation. Millions of volumes were burned by the enemy, were reduced to paper pulp to feed their propaganda machines, and during the freezing winters of eastern Europe became fuel for their invading armies. Both Germans and Japanese had organized commissions of specialists who were able to do a competent job of the bibliographical looting of the libraries of half the world. Libraries, hastily packed and transported to areas of supposed safety against aerial attack or looting, were destroyed by rain, mold, or rodents.

In France, German "inspectors of libraries" carefully protected the great public institutions, and, indeed, some very important bibliographical developments occurred in France during the occupation. The same German inspectors savagely pillaged, burned, and unspeakably desecrated great private collections. In Poland and the Balkan states where the Nazi hand was heaviest, the library resources of each country were "improved" through the creation of regional research centers and the consolidation of many smaller monastic and private collections. The Nazis undertook elaborate plans to rebuild and restock libraries that often they themselves had destroyed earlier.

In eastern Europe millions of volumes in libraries, private collections, bookstores, and government archives and offices were burned—often with some ceremony—in great public fires. Other millions of volumes were fed into pulping machines for reshipment to Germany. In Yugoslavia both the library of the National Institute and the stock of Getsa Kohn, the largest publishers of the country, were used as raw material for the production of new paper.

Two remarkable photographic studies of the treatment of libraries by the Germans show what happened to the University of Liége in Belgium and at the Polish Library in Paris. Books have been taken away—stolen, burned, dispersed—statuary lies in heaps of rubble on the floor, wainscoting and decorative woodwork have been pulled from their walls to be hacked to pieces, walls themselves have been destroyed.

The mobility of the war and the tremendoùs destruction from the air caught the world off guard. As the war fronts moved rapidly over areas of hundreds of miles, the custodians of libraries, archives, museums, and galleries frantically endeavored to protect their treasures. Their efforts were often pathetic, for they lacked equipment, personnel, transportation, and, above

all, a place which promised some degree of immunity from the devastation they could see on every hand.

In China great libraries were packed in boxes and shipped into the interior by horse and oxcart as the Japanese advanced. In France, when the great library of the Chapter of St. Thomas was threatened, it was hurriedly evacuated for storage in a rural area. There the books were destroyed as completely by rain, mold, rats, mice, and insects as they ever could have been by bombs or artillery fire. The original repository, a building at the University of Strasbourg, remained undamaged. In Holland the monks of the Abbey Van Berne distributed the early printed books of the monastic library among neighboring farmhouses. Virtually all were destroyed by natural causes, by theft, and by shellfire. The Abbey buildings are intact.

In the Philippines many of the public and university libraries of Manila were dispersed among private homes, where it was presumed that they might be safe. With the destruction of virtually the entire city, nearly all were lost.

In Italy the Germans first distributed artistic properties in rural castles, monasteries, and châteaux. As the Allied armies approached and as air raids became more frequent, they gathered up such material by train and truck—at a time when transportation facilities must have been very precious to them—for storage in the Vatican. Between November 27, 1943, and June 3, 1944, 664 cases of priceless books went to the Vatican in this way. The American Army took Rome on June 4.

This, then, was the situation as the war ended: Many of the most culturally significant countries of the world had seen great numbers of their libraries, public and private, destroyed as a deliberate or an incidental part of the war. All countries had been deprived of the normal exchange of publications and ideas. Scholarship was disrupted. The tools for printing books and magazines had, in many cases, been destroyed. Paper pulp was at a premium.

In spite of their pathetic primary needs—food, shelter, and clothing—foreign scholars who had suffered through the war and were still suffering long after its end were addressing very moving appeals for books and other printed materials to their more fortunate colleagues in other areas. Food for the mind is as important as food for the body, and in a technological age there is a direct relation indeed between research materials and the ordinary processes of living.

Nowhere in the world was there at the end of the war such a wealth of printed materials as America could provide. American medical journals with reports of seven years of progress unknown to many doctors abroad, engineering publications describing new techniques in building, educational books and magazines presenting the work of American schools, books, journals and pamphlets describing developments in the basic field of agriculture, descriptions of the vast strides made in the chemical industries—these are illustrative of the types of printed materials which were desperately needed and which were available in the United States and Canada.

Fortunately, many Americans did not wait until the end of the war to begin collecting such printed materials. Early in the war the American Library Association set up a Committee on Aid to Libraries in War Areas. This committee appealed to librarians throughout the country to collect all scholarly materials available during the war years and to store such materials for later shipment abroad. This appeal resulted in the collection of 600,000 periodical issues. An additional plan, presented by the International Relations Office of the American Library Association

and supported with funds from the Rockefeller Foundation, provided for the purchase of sets for the war years of 360 of the most valuable periodicals.

Books, as well as periodicals, have been purchased by the International Relations Office. The Rockefeller Foundation has supported the International Relations Office in its purchase of 35 sets of books published since 1939 for distribution to libraries in 24 countries. The Rockefeller Foundation has also financed the filling of specific requests from many libraries throughout the world. The Department of State, as a part of its cultural relations program, has supported the purchase of sets of books for China, specific titles requested for the Philippines, and very large purchases for Latin America.

The program for the collection of donated materials was broadened by the efforts of the Council of National Library Associations through its Joint Committee on Books for Devastated Libraries, made up of representatives of twelve American and Canadian library associations. An appeal for a large-scale program, coordinating the efforts of many individuals and many agencies, was made at a meeting called by the State Department in Washington February 28, 1945. The Joint Committee accepted that responsibility and sponsored the American Book Center for War Devastated Libraries. The Book Center has thus far collected almost one million volumes and has distributed 700,000 of those volumes overseas.

The Book Center sends its materials in all cases to committees in the various countries. These committees, composed of librarians from the various types of institutions, base distribution of the books upon firsthand knowledge of the needs and present resources of the libraries. In addition to the materials sent for distribution by the national committees, the Book Center transmits shipments collected by individuals or groups to specific libraries abroad.

The American Book Center has called upon, and has received the aid of, many other organizations. The Library of Congress has given space for offices and physical operations; UNRRA has made shipment of more than 2,000 cases of books to the countries which it serves. The State Department has facilitated arrangements overseas. Such professional organizations as the American Chemical Society, the Engineers' Joint Council, and the National Educational Association have made strong appeals to their memberships.

Most recently organized of the programs for aid to war-torn countries is the Commission for International Educational Reconstruction. This advisory office, set up by a number of educational organizations, the State Department, United Nations Relief and Rehabilitation Administration, and the United Nations provides coordination and stimulus for all programs interested in the shipping of educational materials overseas. The commission serves all educational levels with all types of materials. In its book program it has worked closely with the American Library Association and the American Book Center. The commission has been called upon by Dr. Julian Huxley, Director General of the United Nations Educational, Scientific, and Cultural Organization, to assume leadership in stimulating American voluntary organizations to supply educational materials to those countries that so desperately need them. The commission is now organized to assume this task.

In addition to the large-scale American efforts there are a great number of programs operating in special fields—programs which in many cases have produced notable results. Such institutions as the libraries of the University of Nijmegen and the University of Caen, both totally destroyed in the war, have benefited greatly from

programs organized to restock their collections. The efforts of single individuals have been, in many instances, remarkably effective. A large number of such enterprises have coordinated their appeals, their collections, and their shipping with the larger agencies which have been mentioned, and at the same time have retained the values of their specific programs.

The use to which American publications can be put in countries where English is not the official language is constantly surprising to American students. The welcome given to American publications is evident in statements such as this from Greece:

> The great work of civilization, reconstruction, and education which your organization has undertaken in behalf of Greece will. . .prove of great use in order to make the Greek people understand the way of thinking of the Americans and the spiritual movement of the United States of America.

Nearly every letter from abroad emphasizes the need and the usability of American publications. From Yugoslavia is written:

> The National Library was burned down by the Germans, who lit it to provide illumination for convoy loading during the final night of their occupation of the City. There is, apart from this, a great lack of American technical books and this impedes many of the processes of recovery as well as limiting a most important avenue of friendly liaison.

Medical literature has been given a high priority in all shipments.

> I would like to tell you [writes a Czech] how pleased all medical doctors and research workers in Prague were when I was able to tell them that the complete series of medical reviews and journals which you contributed to the Medical Library of the Charles' University in Prague were on their way to Czechoslovakia. There is a lot of hunger in my country, yet I can assure you that the desire to know about the progress in medicine and sciences is just as great. You can really be proud of what you have done in order to satisfy this desire. I am certain that your gift offered in the right moment will never be forgotten.

There is a continued need for American scholars to support programs designed to supply books to their colleagues overseas. The need has not evaporated with the coming of peace. It will continue for at least several years to come. Americans cannot supply all the materials of scholarship needed abroad, but they can supply many of the basic tools until reconstruction, exchange of currencies, and transportation facilities make possible once more a normal flow of books. The scars of war will never be erased, but the wounds need not remain open and festering sores. The rebuilding and the restocking of many libraries will help to heal the grievous wounds which civilized men have inflicted upon each other.

(If you wish to aid or if you wish information concerning the emergency programs mentioned in this article, you may write to any of these three organizations in Washington, D. C.: The International Relations Office of the American Library Association, % Library of Congress; the American Book Center, % Library of Congress; or the Commission for International Educational Reconstruction, 744 Jackson Place, N. W.)

Book Reviews

ETERNAL LANDSCAPE OF THE PAST

The Ancient Maya. Sylvanus G. Morley. xxxii + 520 pp. Illus. $10.00. Stanford Univ. Press. 1946.

AFTER forty years of field explorations and study of the Ancient Maya, Dr. Morley has finally produced the general book which has been eagerly awaited by all of those interested in the most intriguing civilization of the New World. There are few who will be disappointed, for here, conveniently arranged and clearly expressed, is a compendium of information covering all phases of the life and known history of the people who achieved the most conspicuous cultural advance in pre-Columbian America. It is doubtful if a more colorful story could be told of any single group in the history of mankind.

The semiliterate Maya left behind them a record in their inscriptions, art, and architecture that allows a much more complete reconstruction of their past than is usual among American aborigines. The documentation of their history during the first century of the conquest, by both native and Spanish historians, is unusually full. This documentation, joined with the archeological record, gives us the unique and continuous picture of thirteen centuries of achievement.

Morley's text is made more vivid by the careful selection of illustrations, which in many instances portray more strikingly than the printed word the unusual accomplishments of this gifted group. Of greater value to the student and professional are the tables which present chronologically practically all significant aspects of Maya culture; and comparative charts which equate them with other Middle American cultures. Of interest to lay readers will be Morley's table of fifty superlatives, comprising the "biggest," "most beautiful," "oldest," "latest," "highest," and so forth, as related to achievements in art, architecture, and the inscriptions.

In his preface, Morley divides the content of the Maya story into four major headings: the people and the region, history, manners and customs, and an appraisal of Maya civilization and its comparison with other aboriginal American cultures. It is a tribute to the even balance of the subject matter that hieroglyphic writing, arithmetic, and astronomy have been condensed into a single chapter.

In treating of the periods of Maya history, Morley still uses the classic terms "Old Empire," "Renaissance," and "New Empire," although he is careful to point out that these have no political significance. His breakdown of these periods equates them with the calendar system and the more recent subdivisions based on ceramic stratigraphy.

It is inevitable that theories will differ when dealing with periods as remote as those in which the early Maya flourished. The present incomplete status of archeological research in Middle America still leaves a wide field for speculation. There are many who feel that the emphasis on work in the Maya area during the past century has tended to magnify the accomplishments of the Maya at the expense of other groups. The conspicuous achievements of the Maya in architecture and their habit of carving elaborate hieroglyphic inscrip-

tions in stone were probably the factors which did most to bring about this emphasis. The picturesque jungle settings of the majority of the ruins intensified their romantic appeal—an appeal which has certainly never diminished with this reviewer.

In the present book Morley leaves no doubt that he represents the classical school, which looks upon the Maya as the principal fountainhead of Middle American culture. Early in the book he states this thesis in general terms.

> Owing to the almost unequaled isolation of the Maya country—surrounded on three sides by vast and, in those days, completely unknown bodies of water, and on the fourth by the lofty Cordillera, south of which Maya culture never seems to have penetrated—the Maya developed their unique civilization practically without influence from the outside. Its origin, rise, and first florescence in Old Empire times were exclusively due to the native genius of the Maya peoples, stimulated and conditioned by the fertile, happy environment in which they were fortunate enough to have lived. . . .This whole picture of unique geographic isolation, coupled with an outstanding indigenous civilization that developed in a region culturally so highly isolated and practically free from all alien influences, constitutes perhaps the best laboratory for the study of an early civilization to be found anywhere in the world.

Many of the specialists in the Maya area no doubt will agree with this, but there are others who will feel it to be a considerable oversimplification of the problem.

He discusses the theory that maize was developed in South America and rejects it in favor of the opinion that maize agriculture originated in the highlands of Guatemala and that with it was developed the early Mamon pottery phase. Mamon pottery then spread over Middle America with the diffusion of maize agriculture. When this early complex reached the Peten region, the rich tropical soil permitted intensive production and furnished the stimulus which permitted the Maya to originate and develop their arts and complex calendar system.

In presenting this thesis, he discusses briefly and rejects the theory that the Maya may have received the calendar from some group or groups lying to the West. The early dates on the Tuxtla statuette and the stelae of Tres Zapotes and El Baul he feels cannot be considered as contemporary because in the first place the decipherment on none of them can be considered as certain and in the second place all of them are non-Maya in style.

In accepting and using the Thompson correlation for converting Maya dates to the Gregorian calendar, Morley follows the practice of the great majority of present-day epigraphers.

Regardless of the stand taken on such controversial points, there will be little disagreement with the scholarly handling of factual material. In fact, the book brings together such a mine of information on all aspects of Maya life that no one, amateur or professional, interested in the anthropology of Middle America can afford to be without it.

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AIRCRAFT INSTRUMENTS

Dials and Flight. Assen Jordanoff. vi + 359 pp. Illus. $5.00. Harpers. New York. 1947.

THIS is not a book about dials. Nor is it a book about flight. It is a book about aircraft instruments, their construction, use, and maintenance.

The organization and content of the book suggest that the material may have been assembled in the preparation of a field maintenance manual for one of the armed services. There are no discussions of general flight principles, no introductory chapter, no general chapters. Rather, each chapter takes up a specific type of instrument and discusses the kinds of things which a

mechanically interested pilot or a general aircraft maintenance man would like to know about that instrument. The outline followed in each chapter is essentially the same. For this reason the content of the book is well illustrated by the following section headings taken from the chapter on the air-speed indicator: Purpose, Location, Description and Operation, Using the Air-speed Indicator, Calibrating the Air-speed Installation, and Servicing the Air-speed Indicator.

The book is in four parts. The first deals with flight instruments, the second with engine instruments, the third with navigation instruments, and the fourth with automatic pilots. The thirty-five chapters go far beyond the instruments found in private planes but, so far as commercial or military flight is concerned, are incomplete in their omission of all reference to radio navigation aids and radar equipment.

The content of the book is made easy to understand because of the fact that the text is supplemented by a great many handsome illustrations. Each instrument is shown in at least one half-tone, cut-away type diagram. Some two hundred line drawings cover additional details of instrument structure, instrument function in flight, and various aspects of instrument disassembly and calibration.

Instrument servicing and the detection of casualties in the various systems are outlined in a clear, step-by-step manner. Adequate warnings are posted whenever the repair work or testing is something which should be undertaken only by skilled instrument mechanics.

Jordanoff is the author of a number of other books on flying. The reviewer is advised by friends more familiar with these earlier publications than he that the present volume is not unlike the others.

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COMPLETE AS PALLAS

A History of the University founded by Johns Hopkins. John C. French. xii + 492 pp. Illus. $4.75. Johns Hopkins Press. 1946.

IN 1816 Tichnor remarked, "We are mortified and exasperated because we have no learned men, and yet make it physically impossible for our scholars to become such." Sixty years were to pass before a university was created, "complete as Pallas," which was to kindle the flame that would light our path to intellectual achievement. The absorbing story of that educational renascence and of the life of the institution which gave it birth is related with warm enthusiasm, yet with nice judgment, by Dr. John C. French, distinguished Librarian Emeritus of The Johns Hopkins University.

What the term "university" meant to the Quaker merchant of Baltimore, who rose from grocery clerk to amass a fortune, we do not know. Yet he desired to promote education and to establish a hospital as a part of the medical school of the university he was to endow. He was more specific as to the hospital, even to the grounds. These he would have "planted with trees and flowers and surrounded by iron railings." Possessing small taste for publicity, he seems never to have thought "the mastery of finance a warrant for his speaking with authority on such matters as education." He did, however, subject his university to the financial vicissitudes of a great railroad, but this, as Dr. French remarks, may "in the long run have been a source of more good than harm."

He was an acute judge of men. In choosing the original Board of Trustees, he was on sure ground. Seeing what these twelve men achieved, there can be little doubt about his judgment.

To those sagacious and tolerant merchants and lawyers—one was a physician,

a half were Quakers—fell the grave duty of creating a new American university. Unique in university history, they established a reading course for themselves in works about education, many of which reflected the controversies of the time. "Feeling their way toward a radically different concept of university education," they sought advice. President Angell of Michigan cautioned them "not to go and erect another college like four hundred already existing in the land, but to strike out boldly at once and make a great graduate university."

They called Daniel C. Gilman to the presidency. He would create a university which "should extend its influence far and wide and would make it the means of promoting scholarship of the first order."

To this end came Gildersleeve in Greek, with his literary taste and skill and a nimble, caustic wit; Sylvester in "our divine algebraic art," maker of mathematicians, who founded the first of the learned journals, and whose teaching "broke every rule of pedagogy;" also came the systematic Remsen in chemistry, whose administrative abilities "later brought him responsibilities from which he should perhaps have been spared;" and then Martin in biology and Rowland in physics.

What these men wrought may be found in the careers of the twenty fellows who joined that first faculty. "Probably no expenditure of ten thousand dollars in American education has ever had so large and so enduring a return from the investment." There was Brooks in the biological sciences; Craig in mathematics; Morse in chemistry; Royce in philosophy; and Adams in political economy and finance, significant for his attack on the social doctrine of *laissez faire*.

These men alone from the literary and scientific department were justification of the trust imposed by Mr. Johns Hopkins, but there were still other far-reaching influences on the intellectual life of America. There came the genius of William H. Welch, first Professor of Pathology, who, together with Osler, Halstead, and Kelly, formed the core of the medical faculty with its hospital, which was from the beginning a teaching hospital with students at the bedside, "designed both to relieve suffering and to advance medical knowledge." Later the medical disciplines were augmented by the School of Hygiene and Public Health and by the Institute of the History of Medicine. Again it was the vision of Welch that triumphed. Medical education was emancipated from the method of the old schools with their four or five months of lectures, heard twice during two successive years, and remote contact of the student with the patient.

The wisdom of Gilman and the trustees was evident in the influence of the university on American education. It "stirred to vigorous life impulses toward advanced courses in institutions in which the college tradition had been all-powerful, and it encouraged new foundations devoted primarily to university work."

But these achievements did not come without opposition. There were those who would have acquired "elm shaded lawns and formal quadrangles" instead of the plainness of the Howard Street site, as there were those who thought it was the plain duty to provide an institution that would "benefit the largest number of Baltimore boys, perhaps with emphasis on agriculture and the mechanical arts." Others there were who opposed the obvious leaning toward science with the "fear its influence would be hostile to religion." And one, Colonel Scharf, criticized the trustees for "wasting the income on a few graduate students, each of whom cost $700 a year to educate," and for failure "to come down out of the scholastic clouds."

This is an honest book that might well be read again and again by those who find in

their hands the destiny of our educational institutions. Its reading would give renewed hope and assurance, and point a way to the future. For the university still must blaze the way. Problems transcending even those already met in the physical and biological sciences remain unsolved. They dwell in the relations of man to man, and of nation to nation.

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WHAT'S PAST IS PROLOGUE

The Path of Science. C. E. Kenneth Mees (with John R. Baker). xii + 250 pp. $3.00. John Wiley. New York. 1947.

IN THESE times of intensive specialization it is good to read a book by a physical scientist whose interests are broad enough to include the humanities and the social sciences. The author, Vice-president in Charge of Research at the Eastman Kodak Company, deplores the cleavage between the humanities and the sciences. He writes: "The humanists must understand what the scientists have done in the past, are doing now, and may do in the future; while the scientists must see their work in the light of history and in relation to the effects that its application to social conditions will produce." In writing this book on the "development of modern science against the background of history" the author has carried out this point of view.

The early part of the book is devoted to a discussion of various cycle theories of history, from the early Greeks through Spengler. The author points out that while there have been ups and down in the history of art, literature, and architecture, the path of science has "step by step advanced through the ages." He compares the pattern of the historical process to that of a helix, or coiled spring, "in which the vertical component represents the growth of scientific knowledge, which increased rapidly after the sixteenth century and then became the dominant factor in the history of civilization."

In the chapters which follow the author discusses the nature of science and the development of scientific method. He traces the growth of science through its beginnings among the Greeks, its collapse in the Middle Ages, and its rebirth with the breakdown of feudalism and the rise of capitalism. The story of the growth of physics, chemistry, and biology brings together a mass of material on three of our most important scientific disciplines and makes fascinating reading.

In discussing the present organization for scientific research and the developments likely to occur in the future, Dr. Mees outlines the growth of research laboratories in industry, endowed technological institutes, and government-operated research laboratories in the United States and in other countries. Despite these new and important developments, Mees believes that the scientific department of the university is "the basic institution upon which everything else depends. . . .It is from the universities that the bulk of the new ideas by which science is advanced are likely to come. . . ." He is skeptical of the possibilities of planned research in the field of pure science and he emphasizes the importance of personal liberty for progress in scientific research. "The increase in efficiency of operation achieved by planning is balanced by the loss of independent thought, with the consequent diminution in the trial of ideas."

Dr. Mees closes on an optimistic note about the future of science and society. He believes that scientific method can and should be applied to the study of society; but he does not view scientific

method as a substitute for fundamental moral principles. He writes: "Truth and justice, mercy to the weak, and understanding for the erring are principles that require no formal justification. These are not the principles of science; they relate to spiritual rather than natural laws."

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AERE PERENNIUS

Banting's Miracle. Seale Harris. xx+245 pp. Illus. $3.00. Lippincott. Philadelphia. 1946.

SO MANY hearsay stories have been circulated about Banting since his discovery of insulin that the present volume based on accurate historic researches of the author is not only timely but highly appreciated by the medical historian and those who have ever treated diabetics. Although Dr. Harris collected the material for this biography in Canada, as his numerous acknowledgments indicate, he also states frankly and to his credit that he "was obligated most of all to Frances Williams Browin, one of Lippincott's editors, for revising and rewriting the entire manuscript which she transformed into a carefully arranged, chronologic biography."

It appears to this reviewer that Banting lived on the whole an unhappy, embittered, and frustrated life except when at short intervals after his international honors he betook himself with oil and brushes to scenic spots in Canada which he tried, with some tutelage, to record on canvas. He seems also to have suffered great disappointment in that he failed to add to his fame by a second Nobel prize-winning discovery—a goal most difficult to attain.

Naturally the author of this biography did his utmost to glamorize his Canadian hero, and properly so. But some of us who knew Dr. J. J. R. Macleod as a very personable and delightful scientist and gentleman are inclined to question, in part at least, the very despicable light in which Dr. Macleod is portrayed, since he is rather definitely accused as a scientist who attempted to filch *in toto* the monumental medical discovery of Banting, Best, and Collip. This is difficult for some of us to believe.

In one instance, which could be checked (p. 169), the author is partly unfair with respect to Macleod's article in the *Encyclopaedia Britannica* on insulin when he writes "that anyone reading that definitive article would never have dreamed that two men named Banting and Best were generally recognized as insulin's discoverers," since the article states in part: "In 1921, however, Banting and Best working under the direction of J. J. R. Macleod [sic!] found that extracts of partially degenerated pancreas contained the hormone." However, in the appended bibliographical references to the article, Macleod does not list any articles of Banting and Best, which surely is a serious and discreditable reflection on Dr. Macleod. As a matter of scientific accuracy, it should be pointed out that the author-clinician is not aware of the fact that depancreatized dogs may live more than a month without insulin, a fact which every laboratory man knows who has had any experience in this type of laboratory research. But this minor criticism is not sufficiently important to keep the biological scientist and layman from reading this intensely interesting and well-written book about a great, versatile medical hero and his trials and tribulations, one who was snatched from life by an airplane accident just when he was about to lead the tranquil and satisfying home life for which he longed so much.

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THEORY AND PRACTICE

What Industry Owes to Chemical Science. A Symposium. viii + 372 pp. Illus. $5.00. Chemical Pub. Co. Brooklyn. 1946.

IN THIS collection of fifty-three short essays on modern (British) practice in various industries, there are many specific references to the work of chemists, but by and large the impact of chemistry on industry is obscured and not illustrated by the details which have been included. There are in the collection some essays of quality, such as those on metals, dyestuffs, rubber, and drugs, and, notably, that on heavy chemicals. But the vast majority of chemical research, which I regard as a fascinating and exciting story, is omitted, or glossed over, or simply told in an uninteresting manner. A hint of national pride which was expressed in the introduction (". . . . British men of science, often in the face of small encouragement, have played their part in industrial development") may have contributed to decreasing the validity and interest of some of the essays.

It seems a pity to discuss aluminum, for example, without recalling that its manufacture by Hall and by Héroult (neither of whom was mentioned in this connection) was made possible by Davy's fundamental work on electrochemistry. In the chapter on dyes, August Kekulé is briefly mentioned, but the structural theory of organic chemistry, for which Kekulé was largely responsible, is nowhere outlined. Yet it was this theory which permitted scientists to determine the arrangement of atoms in dyes and drugs, foodstuffs and vitamins, solvents and fibers; it is upon this theory that the successful syntheses in all these fields are based. In particular, although some plastics were invented on a semi-empirical basis, the vast modern development of the field has been the direct result of an extension of structural chemistry. The simple theory of polymerization was largely developed by Carouthers (who is not mentioned); his patent for nylon is a magnificent example of the application of theory to technology.

The exact way of preparing the wood for lead pencils has its place in human knowledge (in some technical encyclopedia); the essence of the structural theory of organic chemistry underlies much of chemical industry. It is because such fundamental principles are neglected for the minutiae of industrial practice that I find *What Industry Owes to Chemical Science*, in general, unrewarding. I must, however, admit that I do not know an ideal book on the subject; the best that I can suggest is Alexander Findlay's excellent history, *A Hundred Years of Chemistry.*

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NOT SO BAD AS IT SEEMS

The Gardener's Bug Book: 1,000 Insect Pests and Their Control. Cynthia Westcott. xvi+590 pp. Illus. $4.95. American Garden Guild and Doubleday. Garden City, N.Y. 1946.

IF YOU know the name of the bug, you will find it listed alphabetically, with the plants that it attacks and the methods of control. If you do *not* know the name of the bug, find your plant in the list, and if it is a pest of that plant it will probably be there, with remarks on the injuries it does and sometimes a prescription for a remedy.

Starting with a general discussion of insects, the damage they do, and also their value in industry as scavengers, as plant pollinators, and as predators on, or parasites of, other insects, Dr. Westcott gives clear instructions on how to make the garden unpleasant for the bugs and pleasant for the plants. A great variety of garden chemicals

are treated concisely, and mechanical methods of using these as sprays, dusts, or fumigants follow in another chapter.

Brief remarks on the natural history and classification of insects are followed by a list of a thousand or so species regarded as pests, each with short though ample directions on control. Besides an excellent listing of most of the insects injurious to flowers, vegetables, and trees of North America, there are notes on mammals,[1] centipedes and millipedes, mites, salamanders, toads, sow bugs, slugs, and snails, a list long enough to appall almost any gardener; but an optimistic chapter, entitled It's Not So Bad As It Seems, gives a little cheer. No one has all these plants to worry about, and hence not all the insects, and as they come at different seasons, they can be worried about a few at a time. The Plant Doctor's Calendar tells you when to do the worrying.

No one, except that kind of entomologist, will read about two dozen kinds of thrips or a hundred aphids, but this manual helps you to identify the one that is bothering you, and spray it.

A bibliography and a glossary help make this an unusually satisfactory book on the subject, and one which should be appreciated by any gardener. The discussions here and there are written in a sprightly manner and make good reading, though the book will be used mainly as a reference encyclopedia. There are thirty-eight colored plates, some of them showing different stages of insects and the damage they do, and more than a hundred line drawings of pests (some also of mechanical apparatus) —all helpful to the information-seeker.

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[1] The mole can be eradicated, a cat being one of the best exterminators, but trapping or gassing will work. Rats, because they cannot vomit, die from red squill.

ONE MAN IN HIS TIME

The World Expands. George Howard Parker. vii + 252 pp. $4.00. Harvard Univ. Press. Cambridge. 1946.

PROFESSOR PARKER well knows that much can be learned by a careful examination of the complete life cycle of a single animal, facts that are common to the species and facts that are peculiar to the individual. Moreover, much is to be inferred concerning the environment. When a particularly excellent specimen of a species that is fast receding elects to dissect himself and does so in excellent English prose, the product is an elegant contribution.

The World Expands is the autobiography of George Howard Parker, distinguished student and Emeritus Professor of Zoology, Harvard University. The first four decades of the current century of American zoology were marked by the leadership of a remarkable group of men, Parker, Wheeler, Harrison, Coe, Wilson, Morgan, Calkins, and later Woodruff, Conklin, Davenport, Jennings, Mast, McClung, F. R. and R. S. Lillie, Child, Wilder, Donaldson, and others. Most were American-born, all were able, energetic, and driven to prodigious effort by the tremendous challenge of the expanding fruits of experimental analyses. Among this group, the like of which may not be seen again, Parker is outstanding. Parker is then contemporaneous with the rise of experimental zoology. Lacking present-day instrumentation and awaiting a more adequate chemistry, experimental results were largely descriptive; mechanistic interpretations required shrewd analyses or were deferred. The reviewer well remembers a seminar with Professor F. R. Lillie in which occasionally the experimental findings seemed to admit of no rational explanations. When such situations arose Professor Lillie took prompt evasive action and ascribed the results to

"a property of the colloid matrix." Parker was a tireless investigator throughout this period and published prolifically. Unfortunately, his book treats of his many and varied researches sparingly.

Professor Parker and his contemporaries had much in common. Most of them struggled with personal finances when they were students and most of them studied or traveled in Europe. They took their work seriously, gave it dignity and substance, and created an important branch of learning in America. Unfortunately, on occasion they took themselves too seriously and from time to time the scene was enlivened by small war. In effect, Parker's autobiography is an account of the development of the intellectucal and personal life of one of these American zoologists who attained eminence. It is detailed, clear, and, since he follows the advice of Albert Campion that in preparing an autobiography one should "not allow any damned modesty to creep in to spoil the story," it is complete. There are the usual financial problem and its solution, the European study period with its exciting new scenery and old ruins, including a handshake with the skeleton of Johann Sebastian Bach, the contacts and friendships formed with European leaders, Leuckart, Lang, Wiedersheim, Lancaster, and others, all told in a style so engaging as to command interest in a reviewer to whom many of the names are no longer persons but landmarks in the development of modern biology.

Europe, especially Germany and Italy, and, later, Japan were visited before the seeds of self-destruction bore visible fruits in those now unhappy lands. Korea, China, Hawaii, Alaska, and the West Coast of the United States, Yosemite, and the Grand Canyon are the subjects of pleasant personalized sketches. Harvard under Eliot and Lowell, Woods Hole, and Bermuda are the framework within which most of the story occurs. And here is the linkage in personal and humanized terms between today and the days of the Agassiz', Asa Gray, and other eminents both in America and abroad. It is, by the way, comforting to learn from this book that Harvard once was worried greatly over a deficit of sixteen thousand dollars.

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WISDOM IS THE PRINCIPAL THING

Science, Its Effect on Industry, Politics, War, Education, Religion, and Leadership. D. W. Hill. v + 114 pp. $2.75. Chemical Publ. Co. Brooklyn. 1946.

THIS book contains seven essays (The Scientific Outlook, Science and Industry, Science and Politics, Science and War, Science and Education, Science and Religion, Science and Leadership) on our progress and our failures in applying the scientific method to all the pressing problems facing man of today. The author is a man with experience in, and understanding of, the scientific method who has stepped down from the ivory tower, with clarity of vision and a wide grasp of man's past and present problems. These essays are a challenge, particularly to all men of science, to all educators, to all industrial and political leaders in all lands today. Not that all these people will at once agree with the author as to the primary significance of the scientific method as the guide to action in all phases of life. But Dr. Hill's facts and logic will challenge both our traditions and our complaisancies. To this reviewer the strongest chapters are the ones on Science and Education and Science and War; the weakest, the essay on Science and Religion. Dr. Hill insists that "there is no outstanding problem between nations that the method

of science could not solve at a hundredth of the cost of a major war." He insists that a thorough training in, and a complete grasp of, the scientific method is a *must* in the education of every man and woman in every land. In the chapter on Science and Leadership the author appears somewhat off his *main course* in this one statement: "Scientists, in their writings, are too truthful and that makes them dull. In their modesty, they have no use for hyperbole" (p. 101). For are not hyperbole and the scientific method incompatibles?

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THE PEOPLE

The Navaho. Clyde Kluckhohn and Dorothea Leighton. xx + 258 pp. Illus. $4.50. Harvard Univer. Press. Cambridge. 1946.

THIS account of the largest tribe of American Indians in the United States is the third of a series of monographs resulting from a joint project of the Committee on Human Development of The University of Chicago and the United States Office of Indian Affairs. The purpose of the total project is to present data which will suggest means for more intelligent administrative planning and implementation of policies by the Indian Service. To say that this book is the best general report on the Navaho yet published is no exaggeration, and this is no small praise indeed for a book about a people living in a region so publicized, so well travelled, so thoroughly investigated, and so much written about as the southwestern United States! The story of "The People," as the Navaho call themselves, is so told and analyzed that for the nonspecialist it is not merely another collection of Indian "lore," but it has "meaning for anyone who is interested in human beings."

The first brief chapter on the past of The People is a model of clarity and conciseness. That an understanding of the history of any people is necessary for comprehension of their view of life is trenchantly brought out by the remark that one can no more understand the Navaho without knowing about his captivity at Fort Sumner in the middle of the nineteenth century than he can "comprehend Southern attitudes without knowing of the Civil War." The second chapter on land and livelihood presents the problem of making a living, nearly half of which is dependent on livestock, in a crowded and overutilized land in which life is hazardous to begin with. Attempts to solve these problems by shifting the basis of economy are frustrated by the emotional value and prestige value of the Navaho's sheep. Survival, therefore, depends upon delicately adjusted and maintained social cooperation, and chapter three tells how this is achieved. Social organization is primarily along the lines of kinship. "The importance of his relatives to the Navaho can scarcely be exaggerated." Biological family, extended family, "outfit," and clan are the important units, for The People are just beginning to develop a tribal consciousness.

The section on The People and the world around them is the longest and in some ways the most important in the book. It is an unbiased and objective description of the nature and results of contact between the Navaho and traders, missionaries, government employees, and other white persons. Considerable space is devoted to education, with an account of the reform in the Indian school system since 1933 and numerous pertinent suggestions for further improvement. The history of the Navajo Council might be called an experiment in partial self-government, and the chapter closes with suggestions as to how a more truly representa-

tive and workable government might be achieved by and for The People.

Three chapters are devoted to the relation between The People and the supernatural, or what we would call Navaho "religious life," although they do not have a term for religion in their language since they do not think of it apart from the rest of their life as we do. Supernatural beings and powers, ghosts, folk tales and myths, taboos, rites and ceremonials, and the misuse of such techniques, or witchcraft, are described. The analysis of the economic and social advantages of the religious system, for even the belief in witchcraft affords certain positive benefits, is an illuminating summary of Professor Kluckhohn's masterly treatments of such subjects in his previous monographs on *Myth and Ritual* and *Navaho Witchcraft*.

The chapter, The Tongue of The People, with its ingenious cartoons, is one of a very few—if not the first—semipopular discussions of the compelling force a people's language has upon their thinking. A sore need of English-speaking folk for successful living in our "One World" is a greater realization that differences in basic assumptions and attitudes, real or apparent, are often conditioned by differences in language. Americans are peculiarly resistant to linguistic analysis. The remarks on dealing with interpreters should be of much practical value to field workers in any foreign language area.

Finally, the Navaho "philosophy of life" is summarized in terms of ethics, values, and unstated premises of thought. Reflective study of this chapter should go far toward helping anyone, even mere readers of the daily international news, to begin to see things as others see them. Mention should also be made of the twenty splendid photographs.

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GENUS HOMO

The Human Frontier. Roger J. Williams. vii + 314 pp. $3.00. Harcourt, Brace. New York. 1946.

IN THIS book a scientist distinguished in one field outlines the need for another—*humanics*, a science to solve the problems of human living. In successive chapters he shows how present sciences may contribute to the new science, and the problems it may attack. After examining the role of metabolism in character, the relation between the senses and social behavior, the physiological and endocrine basis for behavior, and the basic psychological traits and capacities, Professor Williams moves on to topics such as: man and society, tolerance, mental hygiene, religion, education, marriage, criminology, medical research, heredity, leadership, employment, and international relations.

Professor Williams' excursion from chemistry into humanics is not random or cursory; he has read widely in the psychological and social, as well as the physical and biological, sciences. In his opinion our major scientific task is the study of man. Until we know man as completely as we know other scientific material, we will be our own worst enemies. Only knowledge of man will set us free.

The two basic faults of modern science are:

1. The high degree of specialization which causes the physiologist to study one, the anatomist another, the psychologist another, and the biochemist still another aspect of the human being without viewing the whole. "Men are too complex to be adequately studied by any one type of scientist" (p. 169). The case for a more inclusive and general science is well stated.

2. Science considers man as though all men were alike and could be treated alike. The robot—the statistical man, or "Mr. Average Man"—is the one about which

science generalizes. What is needed is knowledge of the individual—not of man in the abstract.

Here the criticism of present-day science is not on such firm ground. Not only are modern scientists quite as much interested in variability as in trends, but they are adding enormously to our understanding of the individual. Statistical techniques give increasing attention to the variability within populations, and many scientists are concerned with individual differences. Modern differential diagnosis using laboratory techniques in medicine or batteries of tests in guidance and placement make thorough explorations of the individual.

Professor Williams implies that many differences in personality and adjustment arise from differences in metabolism, or other physiological functions. The accuracy of such statements, which are common nowadays, is difficult to determine. When metabolic functioning goes below a certain critical level, marked changes in adjustment occur. But above this floor it is difficult, indeed, to demonstrate relations between metabolic make-up and behavior. Many of the generalizations made on the basis of extreme deficiencies produced experimentally in animals have little bearing on the normal functioning of humans.

The proposed science of humanics is not a science in the strict sense so much as a form of engineering designed to apply the knowledge gained in fundamental sciences.

> This science of human beings (which has for its purpose improvement in social control) we may call humanics. Only by learning its basic truths, teaching them to our youth, and by extending greatly the boundaries of our knowledge can we cope with numerous social problems: education, marriage, health, employment, charlatanism in politics and elsewhere, crime, alcoholism, group bigotry (whose name is legion), and war (p. 5). Humanics, as it will develop, will fall into the category of an industry because labor and capital will be involved in the production of a special type of valuable service (p. 277). The purposes of humanics. . .are related to those of the scientific psychologist but still they differ fundamentally. The psychologist. . .as a thorough-going scientist, must have no special bias or purpose other than to find and understand the truth. Such an attitude is absolutely fundamental to progress and the type of study which we are advocating has everything to gain from a strong and well-developed psychology. But the humanicist, if we may call him such, has an ax to grind; he is interested in the type of psychology and other knowledge that can be *used* and *applied* to social betterment (p. 125).

Williams does not consider the alternative for coordinating the sciences which proved successful with the atomic bomb, namely, the formulation of a major problem and the organization of a team of scientists from different fields with funds and facilities to attack it. Here integration is achieved by mobilizing resources on a problem rather than by developing a new specialty.

The discussion is best in terms of the need for integration and cooperative effort; it is poorest in indicating the avenues and devices by means of which the results desired are to be achieved. We ought to do something as scientists about society, but what? Williams' answer is a new practical science. But this answer ignores the fact that the application of scientific knowledge, even within limited areas, depends not upon one but upon a large number of professions and vocations. How are these professions and the public to be educated? Wants must be created within the population and basic information made available in some degree to all. How is humanics to produce its effects on society? The chapter on education, to my mind, was the most unsatisfactory and incomplete one in the book.

Professor Williams implies that a new profession of human engineers must carry the load of humanics and take the responsibility for substantial reorganization of our living. If such reorganization covers every phase of living, it would involve

complete guidance of the individuals who make up society. One wonders how well human beings would accept such complete controls and what the implications of such controls would be in terms of the organization of the state. I thought immediately of Aldous Huxley's *Brave New World*, which it might be well for some of us who propose Utopias to reread occasionally.

The book is stimulating, especially if one views it as a survey of the possibilities of an applied social science made by a keen and intelligent observer who sees many deficiencies in our present approaches to human problems.

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NATURAL HISTORY

The Wolf in North American History. Stanley Paul Young. 149 pp. Illus. $3.50. Caxton Printers. Caldwell, Idaho. 1946.

THE wide and varied public that may be expected to be actively interested in Mr. Young's account of *The Wolf in North American History* includes hunters and trappers, fur dealers and wearers of furs, western stockmen and farmers, historians and economists, and, most directly of all, naturalists and conservationists. The book is essentially a condensation of the author's share of *The Wolves of North America*, by himself and Major E. A. Goldman, published by the American Wildlife Institute in 1944.

Mr. Young's vast knowledge of the techniques and practices of wolf destruction has involved him in the study of the whole natural history of wolves. That his conclusions should be quite opposed to the complete extermination of the wolf is of fundamental interest to the naturalist and the private citizen who may be paying the bills for wolf destruction without being either consulted or, if they protest in the wolf's behalf, heeded. Our best interests may well be at variance with those of the western livestock men who graze their stock on public lands and obtain the diversion of national funds for what they frankly promote in private as a campaign of extermination, whereas in public they euphemistically refer only to predator control.

There is a chapter on the quite distinct red wolf of eastern Texas and Louisiana; a chapter on the development of the steel wolf trap, and a history of the gray wolf—the timber wolf—in America. This is essentially the record of its progressive elimination from the settled parts of the United States and Canada. The role of the wolf in European and American legend supplies the source of much misinformation about wolves, and especially the background of the transmitted heritage of anthropomorphic superstitions regarding their "viciousness" and "malevolence." We are indebted to the author for making accessible some of the notable American wolf stories of colonial times, especially the story of Israel Putnam and a renegade wolf of Connecticut, dating from 1739.

Mr. Young has been so closely identified with the "predator control" program of the Bureau of Biological Survey, and his book includes so little about the protests of naturalists against that program, and so little about the examples of major calamities to wild game, to natural vegetation, and ultimately to soils, resulting from the destruction of predators, that even his evidently sincere bow to preservation of the remnants of the North American wolf populations comes late and seems somewhat like a lack of knowledge on the part of the right hand of the left hand's efforts. Many naturalists regretted, and sometimes protested against, the transformation of the Biological Survey from

a government agency engaged in the investigations in field biology that laid a foundation for further ecological study of our continent into a corps of animal destroyers. The reviewer shares this regret but, like other naturalists, agrees that the gray wolf must be exterminated in settled areas. Mr. Young's testimony that the wolf need *not* be exterminated in wilderness areas will greatly strengthen our position. His book will remain a most interesting account of one of the most characteristic predators of the Northern Hemisphere.

KARL P. SCHMIDT
Chicago Natural History Museum

THE GOOD SOCIETY

Science and Freedom. Lyman Bryson. xi + 191 pp. $2.75. Columbia Univ. Press. New York. 1947.

THE scientific method should be applied to social science as well as to the physical and biological sciences. This suggestion, as well as the contrary one that social science is not a science and that the scientific method is inapplicable, have been frequently heard. For those who would consider this question with care (and what more important business do we have these days than a better understanding, if possible, of the science of human relationships!), this profound and provocative book by Lyman Bryson, Professor of Education at Teachers College, Columbia University, and Counselor on Public Affairs to the Columbia Broadcasting System, is strongly recommended.

The title of the book does not at first suggest a study of how our present society can evolve toward a "good society." It is nevertheless an appropriate title, for the two terms, as defined in the first two chapters and used with univocal precision, are the basis for his thesis: that the way of working which has led us to our present state of understanding, or mastery, of the world of nature aside from man, at least deserves an earnest trial in seeking to understand, and master, ourselves.

Freedom is assumed to be an example of a "good," and then Mr. Bryson devotes his first chapter to describing precisely how he is using the term. His definition emphasizes variety of choice and variety of normalities. "A society or social group that treats with the same, or nearly the same, friendliness a wide range of behavior patterns is providing freedom in some real degree for its members." His concept of "economic democracy" and, later in the book, his description of three stages in the evolution of a free society are pertinent to much of the discussion these days about "democracy" and "free enterprise."

The chapter on What is Science? uses less space in defining science than it does in enlarging upon and clarifying the thesis of the book. For example, he deals quickly with those who object to scientific statements about human behavior because of the necessity for abstraction, by reminding them that the entities with which the physical and biological scientists deal in their advanced stages are always abstractions. The entity "triangle" as used by a mathematician is not subject to sensory observation. Thus, the "economic man" is also an ideal construct, defined arbitrarily for purposes of study.

Before suggesting what specific kinds of things a scientific humanist would do in working toward the good society, Mr. Bryson occupies himself with further prolegomena to clarify his concept of how social change occurs (peacefully where diversity is permissible; i.e., in a free society), the meaning of culture, the "person" as an entity of social science, the institution as a "set of habits shared by two or more persons in complementary relations . . . ," and the somewhat frightening term, "the social engineer." In his chapter on Education, however, it is

made clear that the social engineer would seek by all means at his disposal to "build in as many persons as possible, in the 'normal' person, the habits of free action . . . [and] loyalties to the institutions of freedom." (He is careful not to make the common mistake of insisting that men be free to do prescribed things and of being more concerned about the content rather than the conditions of freedom.) There is also a challenging discussion of the old problem of relating knowledge to action in this chapter on education.

For an evaluation of his brief nod to a "philosophic basis" for his argument, this reviewer refers you to the philosophers. His description, however, of three "patterns in the mental content" as *practical, absolute,* and *scientific* seems to me to be particularly helpful in understanding some of the basic controversies of modern life, and the slow emergence of the "scientific" pattern suggests a measure of cultural growth toward a good society. His approach to the definition of this good society is essentially pragmatic, but of special interest.

Mr. Bryson writes with precision and compactness. The book is not easy reading, but instead deserves a second reading. His frequent use of well-chosen examples to make his points clear is especially helpful.

As one who is oppressed on every hand by emphasis on material values, it is deeply refreshing to read a book which from the outset is implicitly and explicitly concerned with human values. The entire concept of freedom, set forth as a good, is apparently derived from a democratic belief in the importance of individuals having opportunities for self-realization. He describes a democratic government as "one that has for its purpose the creation of such conditions as will best keep and develop the intrinsic powers of men."

PHILIP N. POWERS
President's Scientific Research Board
Washington, D. C.

MANKIND IN A TEST TUBE

Human Destiny. Lecomte du Noüy. xxi + 289 pp. $3.50. Longmans, Green. New York. 1947.

THIS is a very stimulating, altogether worth-while, and extremely timely book, but I am afraid the publishers have done its author a disservice by announcing in their "blurb" that it "presents a brilliant new interpretation of evolution and expresses a startling theory of Man's place in the universe." Actually, it gathers together in highly commendable fashion the widely held concepts of emergent evolution that have been current at least since Bergson's day and focuses their philosophical implications upon the crisis faced by humanity at the present time. This involves, of course, the consideration of the relations between science and religion, as well as the challenge to educators and all who are concerned with the training of youth.

Portions of the book are marred by an unfortunate use of foreign idiom, and the author is certainly not well versed in the vocabulary of American geologists. I think he overplays the gaps in the known paleontological record and at times seems to base certain conclusions on the absence of knowledge rather than upon inferences from verifiable data. But these are merely minor flaws in an otherwise praiseworthy production, and his fundamental thesis is sweet music to my ears.

Dr. du Noüy is one of the foremost biophysicists of our time. For several years an associate member of the Rockefeller Institute and then head of the Biophysics Division of the Pasteur Institute, he was serving as a director of the École de Hautes Études at the Sorbonne when France was submerged beneath the Nazi flood. He and his American wife escaped to the United States in 1942 and are now living in California. His scientific

and philosophic attainments have brought him many honors, including an award from the Franklin Institute of Philadelphia and the Arnold Reymand Prize of the University of Lausanne.

From this background of experience and knowledge, Dr. du Noüy reaches the conclusion that the methods which have proved so effective in interpreting the inanimate world fail utterly to explain, or account for, the origin of life or even the appearance of the highly dissymmetrical molecules which seem to be required as the basis for living organisms. But once life appeared, the course of organic evolution appears to have direction.

> Everything always takes place as if a goal had to be attained, and as if this goal was the real reason, the inspiration of evolution. . . .The laws of evolution are teleological, whereas those of the transformation of each species simply tend toward a state of equilibrium with the surrounding medium.

Acceptance of this concept of "telefinalism" helps Dr. du Noüy to penetrate the otherwise "absolute mystery" of the appearance of moral and spiritual ideas. With the emergence of man, a new phase of evolution is made possible. "Evolution continues in our time, no longer on the physiological or anatomical plane but on the spiritual and moral plane." With "the birth of conscience," when man

> asked himself the question as to whether an act was "good" or whether another was "better," he acquired a liberty denied to the animals. . . .When this occurred, man took another leap and increased the gap which already separated him from the other primates;. the new orientation of his evolution was clearly indicated. Henceforth, contrary to all the others, in order to evolve he must no longer obey Nature. He must criticize and control his desires which were previously the only Law.

I agree heartily with Dr. du Noüy "that no progress will be made unless the ultimate solution is sought in an extension of the concept of evolution to the whole of nature, including man and his intellectual and moral development." Indeed, I wrote an article many years ago entitled "The Evolution of the Soul." But there are implications in this book with which I cannot agree. The sentence quoted above, with its statement that man "must no longer obey Nature," his reference to the "intervention of an Idea, a Will, a supreme Intelligence," and the suggestion that intelligence "will in fact almost always be opposed to moral and spiritual development" indicate a philosophical dualism that I cannot accept. Instead, I interpret the observable facts of evolution, past and present, to mean the constant and continuing presence within nature of an administrative Intelligence operating in different ways on different "planes," but always and everywhere consistent with itself. And certainly it requires all the intelligence that men can muster to discover the goal of moral and spiritual development, or even the paths that might possibly lead us a little way toward that goal.

KIRTLEY F. MATHER

Department of Geology
and Geography
Harvard University

Comments and Criticisms

IN THE NEGATIVE

The article "In Accentuation of the Negative," by T. V. Smith, (SM, December 1946) certainly fulfills the promise of its title. My reaction to it was decidedly negative.

Dr. Smith is presumably attempting to establish a philosophical basis for the theory that "that government is best which governs least" (a magnificent generality which is certainly true in a very restricted sense, and utterly false in many others). That is my interpretation of his position after reading his article; but he is so deliberately abstract and "general" in his discussion that I cannot really be sure of what specific realities he is concerned about.

Interpreted naively, his "rules"—modifications of the so-called golden rule—are simple truisms. But we can be quite sure that Dr. Smith does not so interpret them. For us to know exactly what he does mean by these rules, however, it would be necessary for him to state a few specific examples of how he would apply them to the real affairs of the world. This he has not done. I believe he is somewhat furtively attempting to support a position with which I disagree very strongly. But since he does not openly define his position, I cannot attack his arguments directly.

As a professional philosopher, Dr. Smith ought to have learned that words are unreliable tools for the conveyance of meaning on the level of the abstract and general, and must be aided in their task by frequent reference to specific things: events, persons, places, dates—in short, "referents." But evidently he has not learned this. When he does use "examples," they are only remotely related to present reality. Either Dr. Smith is really dwelling in that "ivory tower" so often mentioned in connection with philosophers and scientists, and is thus unwilling to degrade himself by specific reference to mundane realities, or else he does not quite have the courage to make a bald statement of his position.

I have a complaint also concerning the author's pedantically obscure style, which I believe has no place in a scientific journal (although it lends itself very well to the technique of concealing one's exact meaning). It is difficult enough to understand complex subject material even when an effort has been made to achieve clarity of expression. Here, however, there seems to have been almost a deliberate attempt to employ phraseology which requires intense mental effort for interpretation. I believe this practice grows out of a belief that any thought which is expressed intricately is necessarily the product of a sophisticated mind. (I am here using "sophisticated" in its best sense, as an antonym of "naive.")

This attitude, I am convinced, has done as much as anything else to prejudice scientists as a class against philosophy and philosophers. This is the more regrettable because it is so unnecessary. I am convinced that any worth-while thinking (and philosophers have done a great deal of it) can be expressed understandably and accurately without resort to grandiose and unfathomable phraseology. I concur with A. N. Whitehead's condensed statement of the situation (quoted in E. T. Bell's *Men of Mathematics*, p. xxi): "It is a safe rule to apply that, when a mathematical or philosophical author writes with a misty profundity, he is talking nonsense."

L. V. Blake

Washington, D. C

THE BIG TEN

It is not often that I find time to read a scientific periodical from cover to cover, but your recent issues have been good, and well worth serious study. As a member of the Association for the past 22 years, though never taking much active part, I have been an interested onlooker. Surely we cannot but advance as a science when we have at the head of affairs such men as Chas. F. Kettering. As he remarks, it is truly alarming when our Monthly only aggregates 15,000 copies monthly. I believe that one writer put his finger on a good point when he said there should be more writing for the average man: after all, the real test of an educated man is that he can use simple language; tongue twisters, whilst very impressive, are seldom understood even within the one single specialty.

I have been interested, too, in the discussion relative to the ten most influential books of science. A recent critic has said that Pasteur was greater than Jenner, and hence his writings, rather than Jenner's, should have been included amongst the

ten. As one, however, who has written to the medical press reporting at least one big smallpox epidemic, which vindicated vaccination, let me point out that Pasteur, great though he was, was not the first, as Jenner was, to prove that specific immunity to an infectious disease could be built up by artificial means. Roger Bacon, too, of course, was important not because he classified most existing knowledge, but because he took men's minds away from argument and senseless philosophic discussion which led nowhere, and directed them to study nature—the book of life. It was this which was the spirit of the Renaissance. Freud, too, may seem important to some, but not more so than Adler and Jung; and there are some who do not class their work as of any too great importance anyway. Let's keep national animosities out of our discussions, Mr. Editor, seek to become less caustic, and treat matters on their scientific merits as true scientists should

ROBERT KERR DEWAR

Fort William
Ontario, Canada

IMPONDERABLES

Mr. Thomson King's suggestive paper prompts me to supplement it with the message that I sent to my colleagues and students at the University of Virginia who gave me a reception when I retired last September. This message follows:

The prophet and his disciples seek life and are religious and risible. The scientist and his students seek ponderable entities and are apprehensive, wearing but a very sad smile in the face of free atomic energy.

Today the scientists' rather than the prophets' aspiration prevails. Some biologists speak of the terms "living" and "life" as meaningless. Life to many biologists is but a by-product of protoplasm's activity—life being secreted by protoplasm in much the same manner as are enzymes secreted. These biologists look to the day when morphology and physiology will be reduced to terms of physics and chemistry. This is a worthy problem for scientists. Joseph Needham is justified in making the following statement, "The aim of *Entwicklungsmechanik* is thus the reduction of the phenomena to the smallest number of causal processes." Someday the problems of morphology and physiology will be solved in physical and chemical terms.

But the contemplation of life or biology as an intellectual discipline carries us beyond form and function. Biologists must recognize the *use* of form and function. This fact has been ignored or slurred over by most biologists. Even those biologists, the psychologists, who should deal most intimately with life, seek to reduce their phenomena to ponderable terms. They have said, for example, that sensation varies as the logarithm of the stimulus; but they must leave behind all mathematical terms when confronted by beauty, truth, goodness, and a mother's love for her child. These are values that men, through use of their protoplasmic machines, interpret into an otherwise meaningless, monotonous universe. In the presence of these values, "a modern psychologist might squirm at being confronted with the etymological meaning of his science. . . .At the risk of shocking some people, I should assert that Pascal, for example, or Shakespeare has not been surpassed as a genuine psychologist" (Albrecht-Carrié, René. 1946. "One Scientist to Another," *American Scientist*, **34**, 472). So likewise, we biologists should "squirm at being confronted with the etymological meaning" of biology.

In this, my final greeting, may I remind you that life carries us beyond the ponderable. There can be, therefore, no science of life As scientists, we biologists have our problems of form and function. As biologists, however, we have the fact of *life* and the manner in which it *uses* form and function. Biologists, in overlooking this fact, remind me of Elizabeth Barrett Browning's lines: "Every common bush is afire with God [Life], but we [biologists] sit around and pick blackberries."

I assert that Jesus Christ has not been surpassed as a genuine biologist.

WILLIAM ALLISON KEPNER

Charlottesville, Va.

HOW ABOUT MIND?

The article "On Life as a Separate Entity" in your February 1947 issue, by Thomson King, impressed me deeply. I also read, with great interest, Carlson's "Science and the Supernatural," appearing in August 1944, and contributions by Anna Rosenberg and Arthur H. Compton in later issues.

Thomson King postulates: (*a*) that there are two fundamental entities, matter and energy, in the universe, and that all phenomena are due to these, separately or in combination; or, (*b*) that there are three fundamental entities: matter, energy, and life—and that all phenomena arise from them.

From experience and by experiment we know that matter and energy are fundamental; everything that exists is due to the activity of matter and energy; yet there must be something else to account for phenomena: let us suppose that it is life and examine the last assumption.

It is generally accepted that there was a time when no life existed on earth; the condition of the stars and of stellar space is believed to be inimical

to life, although it is quite possible that life does exist on some planets. Thus life can hardly be regarded as a fundamental entity, like matter and energy.

What else is there that matter and energy cannot account for? How about Mind? Is life the cause of mind, or is mind the cause of life as well as of matter and energy? If we admit the existence of a Universal Mind and concede that it is the cause or substance of mind in all living things, are we not on a more secure footing?

In the light of recent development in atomic physics it is high time that a better understanding of the nature of matter and energy should prevail Atoms are not abstractions, but are forms of energy, possessing weight and mass; occupy space, vibrate, and are three-dimensional, consisting of particles finer than themselves, made up of electric charges, negatrons and positrons, which, in turn, are created by gravitational energy that pervades all space Like little bullets they can be fired into atoms either to disrupt them or build them up into higher atomic weights.

The conclusion seems to me to be inescapable that mind is a form of energy which cannot be destroyed and in consequence exists and persists after the so-called death of the body.

Motion is the only thing that enters the brain from the outside world; mind absorbs this motion and becomes aware of it. This would seem to prove that mind is a form of energy and, if so, it is indestructible.

I am sure that a large percentage of your readers are profoundly interested in these basic problems and it is to be hoped that more informative articles along similar lines will be forthcoming in the not too distant future.

HARRY LA VERNE TWINING

The Scientific Forum
Los Angeles

CARBORUNDUM

In reading an article entitled "Early Chinese Jade," by A. G. Wenley, published in the November 1946 issue of THE SCIENTIFIC MONTHLY, I noticed the following sentence on page 343: "The whole process is extremely time consuming and requires great skill and patience, although in recent times the use of carborundum as an abrasive and also the diamond drill has to some extent hastened the process."

My reason for bringing this sentence to your attention is that *Carborundum* is the registered trade-mark of The Carborundum Company and is used by this company to indicate the source of its abrasive products. *Carborundum* is not the name of an abrasive material but is a trade-mark which we use on all of our abrasive products, including those made of flint, emery, garnet, silicon carbide, aluminum oxide, and diamond.

I realize that it is often easy to slip into the use of a trade-mark in place of the correct name of the product or material to which we are referring. I am therefore taking this opportunity to ask for your assistance in avoiding misuse of our trade-mark *Carborundum* in THE SCIENTIFIC MONTHLY. I would greatly appreciate your cooperation with us in this matter.

W. G. SOLEY

The Carborundum Company
Niagara Falls, N. Y.

TO WRITE OR NOT TO WRITE

In his article on "The Principles of Poor Writing"[1] published in THE SCIENTIFIC MONTHLY for January 1947, Dr. Merrill failed to state the most important principle: *Have nothing important to write*

HOWARD B. HOLROYD

Bucknell University

UNDISMAYED

"Henry Adams and the Repudiation of Science" (Charles I Glicksberg, SM, January 1947) prompts comments upon the meaning of the universe. The essence of the article implies that if humans cannot understand all the laws of nature then quit trying and retreat to religion—comforting after failure.

Henry Adams' worries can be attributed to farsighted vision, seeing much of the known, unable to reach yet undiscovered laws but entirely overlooking the closest of all facts.

The first law of nature should express the number of possible laws or, in a crude expression, $\frac{dN}{N} = \frac{N}{\infty}$, two imaginary ratios where the infinitesimally small number of laws now known bear a ratio to the number that ever will be known as that number bears to infinity.

As a corollary, N must be a summation of discontinuities, such as Newtonian mechanics, mental processes, history, and other observations. Religions (plural to avoid any denominational selection) are merely a small factor in mental processes but unfortunately the present age has not yet discovered that there are many terms that cancel each other, leaving simple (?) philosophy as a result.

In summation, work with what we have, search for more, but do not waste eight pages trying to relate dN with ∞; the impossible is obvious.

A. W. CLEMENT

Galion, Ohio

The Brownstone Tower

This issue should make its appearance at a propitious time. The editor will have returned from a short vacation and, full of spring and new hope, will attend a meeting of the A.A.A.S. Publications Committee. This new committee is now responsible for the supervision of policy of the SM and of *Science*. Headed by Dr. K. F. Mather, the committee may be expected to guide, stimulate, and support the editors in any innovations that seem promising and feasible to them. It is heartening that capable officers of the Association are now taking an active part in the development of the two journals.

As the leader of a committee meeting, Dr. Mather probably has few equals. On February 23 I watched him conduct the organization meeting of the Inter-Society Committee on Science Foundation Legislation. His performance was a masterpiece of precision and firm but good-humored control. Standing before a roomful of representatives of many scientific societies, Dr. Mather guided the election of officers of the committee and obtained expressions of opinion on some of the controversial features of the pending Science Foundation bills. By the time he had turned over the chair to the newly elected chairman, President Edmund Day, of Cornell, he had won the admiration of the whole audience. (For a report of this meeting and of developments in Science Foundation legislation in Congress read recent and current issues of *Science*.)

For the sake of economy in the face of rising costs all issues of the SM since February have consisted of 96 pages between covers. The first 32 pages are of coated stock suitable for half tones; the last 64 pages are of book stock. Thus the cost of manufacture per page is reduced to a minimum. In the past the article chosen to lead each issue was always one that the editors regarded as particularly significant for one reason or another. Under the present arrangement, however, the leading article must be illustrated, and it may or may not be as good as some of the unillustrated articles that must follow it. We have noticed that the readers will find the best articles wherever they are placed in an issue.

Partly as a measure of economy and partly because wrapping paper is scarce, we have mailed the SM unwrapped since the beginning of this year. Although many national magazines have adopted this practice, we were fearful that numerous complaints of damage would be received. To date, however, with three issues delivered, only four complaints have come to my attention. My personal experience with unwrapped deliveries is good; however, if some readers find their copies are not as crisp as they were last year, we ask their forbearance. But if any copy is badly damaged in transit, we want to be informed.

Much credit is due to Mr. Christensen and Mrs. Keener for working out the present Spring Book Issue. Many plans are incubating in our office for new features. "*Der Mai ist gekommen; die Baüme schlagen aus. . . .*"

This paragraph is added as the May issue goes to press. The coming of spring has been saddened for us by the sudden death of Dr. W. L. Valentine, editor of *Science*, on April 5. Sharing my office with him, I was impressed by his executive ability. Guided by the opinions of readers, he made many innovations in *Science*. His editorial career was brilliant. *Vale* Val!

F. L. Campbell

THE SCIENTIFIC MONTHLY

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EXPLORATION OF THE UPPER ATMOSPHERE BY MEANS OF ROCKETS

By H. E. NEWELL, JR

Naval Research Laboratory, Office of Naval Research, Washington, D. C.

AT THE present time the upper regions of the earth's atmosphere are of special interest to scientist and layman alike. For the first time in history the portions of the atmosphere above the 32 kilometers (19.8 miles) attained by balloons are accessible to direct observation. Rockets and rocket-borne instruments already have reached altitudes of 175 km., and it is simply a question of time before even greater heights are a matter of course.

To the scientist, the advent of the rocket as a vehicle for high-altitude research is a welcome thing. In the past a tremendous amount has been learned about the upper atmosphere above 30 km., but only by inference from a wide variety of observations made from outside the region under investigation. By piecing together information from studies of meteors, the aurora, the propagation of sound and radio waves, and other phenomena, it has been possible to obtain a fairly good picture of the state of the atmosphere at least up to 120 km. There are, however, many questions that remain unanswered. Now, by the use of rocket-borne instruments, it is possible to provide some of the answers.

The rocket does not, of course, displace other methods of investigating the upper atmosphere. It merely supplements them. The principal advantage of the rocket is that it actually gets into the region under study. But there are also certain obvious shortcomings. First, at least for the present, a rocket flight is relatively short. The longest V-2 flight to date took only between nine and eleven minutes. Thus the period of time available during a flight for the making of measurements is limited. Also, there are definite limitations imposed upon the sizes and weights of experimental equipment. The performance of the vehicle itself, vibration, rolling, and supersonic speeds introduce experimental difficulties. Experiments requiring observations over an extended period of time for the determination of such things as diurnal and seasonal effects are at present ruled out. In spite of such limitations, however, the rocket can provide a valuable service in high-altitude research. In fact, a good beginning has already been made in the use of the V-2 for this very purpose.

For the past several months German V-2 missiles have been employed in an intensive program of upper-atmosphere research. The

Naval Research Laboratory Photo

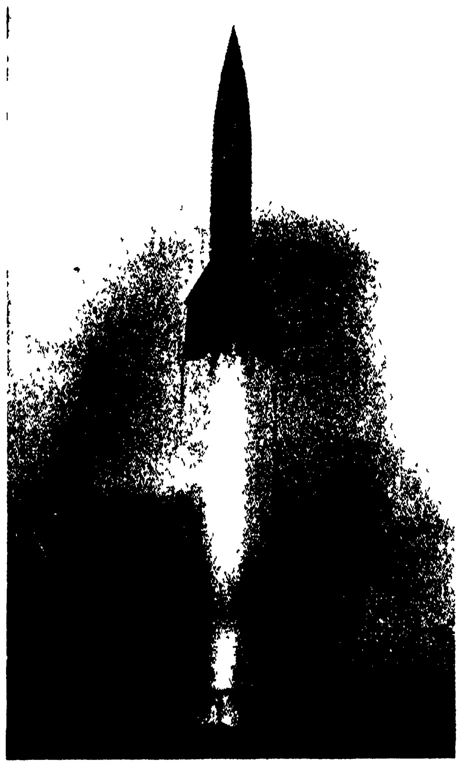
FIG. 1. V-2 ON LAUNCHING STAND

missiles were captured in Germany at the close of the war in Europe and are now being fired at White Sands in New Mexico by the Army Ordnance Department.

The high-altitude research is actually one phase of a three-purpose program: to obtain experience in the handling of large rockets; to obtain ballistic and aspect data for the missile; and to perform upper-atmosphere experiments. The first phase is being conducted by the General Electric Company under contract to Army Ordnance. The ballistic program and general tracking of the missile are being carried out by personnel from the Army's Aberdeen Proving Ground. Several agencies are taking part in the instrumentation of the V-2's for exploring the upper atmosphere. Their efforts are coordinated by a V-2 Upper Atmosphere Panel, the chairman of which is E. H. Krause, of the Naval Research Laboratory. The Panel was formed at the invitation of the Army Ordnance Department some months prior to the first firing at White Sands on April 16, 1946.

The agencies actively participating in the upper-atmosphere program are: the Naval Research Laboratory, Washington, D. C.; The Johns Hopkins University Applied Physics Laboratory, Silver Spring, Md.; Princeton University; and the Army Air Forces through the University of Michigan and the Watson Laboratories in Cambridge, Mass. Princeton University dropped out of the program early in 1947, and the Army Signal Corps has since entered into the work.

The firings at White Sands occur at the rate of one every two weeks. They are grouped by fours into cycles, each agency instrumenting one missile per cycle. Seventeen missiles were launched in 1946. Up to



Naval Research Laboratory Photo

FIG. 2 V-2 TAKES OFF

March 15, 1947, a total of 21 had been fired, and it is expected that the final total will be at least 50 V-2's fired. At the current rate of firing every two weeks, this means that the V-2 program will extend at least into the summer of 1948.

So far the Naval Research Laboratory has provided the upper-atmosphere research equipment for three V-2 flights, the most successful of which occurred on October 10, 1946. Figure 1 shows the October 10 rocket as it rested on the launching platform prior to the firing. The missile stood a full 46 ft. 11 in. above the top of the platform. Its diameter was 5 ft. 5 in. Some idea of its huge size can be obtained by comparison with the men in the picture.

The second figure shows one of the earlier rockets just after take-off. The thrill which the sight of such a launching brings can hardly be expressed in words. At the moment of take-off the jet exerts just enough force to lift the missile, which rises gently and gracefully. The full thrust of 28.3 tons is turned on the moment the missile starts to move, however, and within a few seconds the speed of the missile exceeds that of sound. On October 10, at the time of fuel burnout, the rocket was at an altitude of 25 miles, or 40 km., and was moving with a speed of nearly a mile per second.

Some of the principal features of the October 10 flight are shown graphically in Figure 3. Plots of altitude and velocity versus time appear on the right, and the trajectory itself is given on the left. The horizontal motion of the rocket was small in comparison with the 175-kilometer height attained. The maximum altitude was reached at 238 seconds after take-off. The shower effect was drawn in at the end of the trajectory to indicate that the missile broke up after about 410 seconds of flight.

The air burst was caused intentionally by exploding charges placed just behind the warhead. Two methods for detonating the explosive were employed. One used an

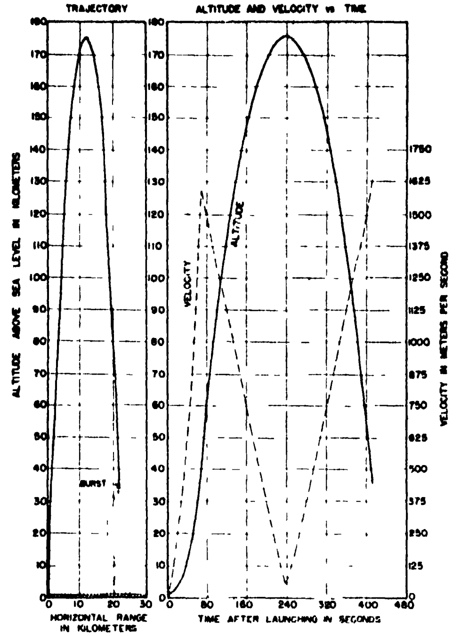


FIG 3 V-2 TRAJECTORY, OCT. 10

automatic timer set to operate at 330 seconds after take-off. The other used a remote-control device, the radio signal for which was transmitted at 335 seconds after take-off.

Parts of the rocket were strewn over an area extending from 19 to 27 km. from the launching site. There is, accordingly, no doubt that an air burst occurred. Since, however, radio signals were received from the missile for 410 seconds, it appears that the explosive merely weakened the missile structure at some time between 330 and 335 seconds after take-off, and complete disintegration occurred some 75 seconds later. At this time, as the graphs of Figure 3 show, the missile's altitude was 35 km., and its speed was 1,640 meters per second, more than 1 mile per second!

The air burst was caused to facilitate recovery of equipment from the missile. On previous flights in which the rocket struck the earth intact, tremendous craters were formed. One such crater is shown in

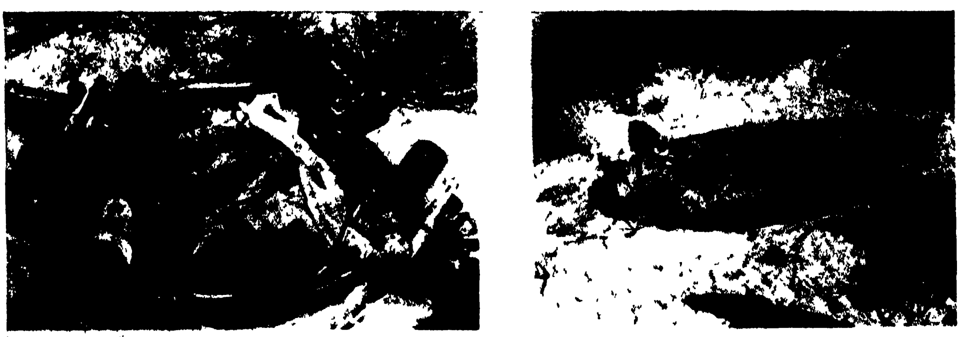
Naval Research Laboratory Photo

FIG. 4. CRATER FROM V-2 IMPACT

Figure 4. That resulting from the impact of the June 28 rocket measured 80 feet across and was more than 30 feet deep. In spite of an intensive search involving more than a week of excavating the crater and sifting through the sand, nothing of value was recovered. On the other hand, the afterbody of the July 30 missile, for which the attempt to cause a breakup in midair was successful, was only slightly damaged. Attached equipment, if appropriately protected, could have been recovered. Figure 5 shows portions of the October 10 rocket which were found after the flight.

A schematic diagram of the instrumentation of the October 10 V-2 appears in Figure 6. The seeds, none of which were recovered after the flight, were provided by Harvard University for the purpose of determining the effect, if any, of cosmic radiation at high altitudes in producing mutations. The cameras were installed primarily to study the motions of the trailing wire antennas which were attached to the fins. Five of the cameras mounted on the venturi ring shown in Figure 5 were recovered with the films undamaged. The spectrograph was moved from the warhead location used on June 28 to the fin position shown in the figure, because it was felt that the change would increase the likelihood of recovering the instrument after the flight. The warhead was designed at the Naval Research Laboratory specifically for use in upper-atmosphere research and was constructed at the Naval Gun Factory in Washington, D. C. The explosive for causing an air burst is shown installed in the control compartment just behind the warhead. Also in the control compartment was the telemetering equipment used for relaying data to the ground by radio.

The Naval Research Laboratory telemetering equipment has been used on all V-2 flights at White Sands. The airborne unit, when in operation, continuously transmits groups of one-microsecond pulses. Each group consists of an initial pulse followed by a sequence of 23 pulses. The last



VENTURI RING ROCKET ENGINE

Naval Research Laboratory Photo

FIG 5. PORTIONS OF THE V-2 RECOVERED AFTER THE OCTOBER 10 FLIGHT

pulse of a group is always separated from the initial pulse of the next group by a very much longer time interval than ever exists between the pulses within the group itself. In this manner the different groups of pulses are clearly distinguished. The groups are transmitted at the rate of 180 per second, the time interval between the initial pulse $(n+1)$th pulses is $50 + 30V_n$ microseconds. Thus, the spacings between pulses within a group furnish a measure of the voltages existing at the inputs to the various channels at the time of transmission of the group.

It is plain that if the voltages at the inputs of the channels are themselves measures of physical quantities, then radio

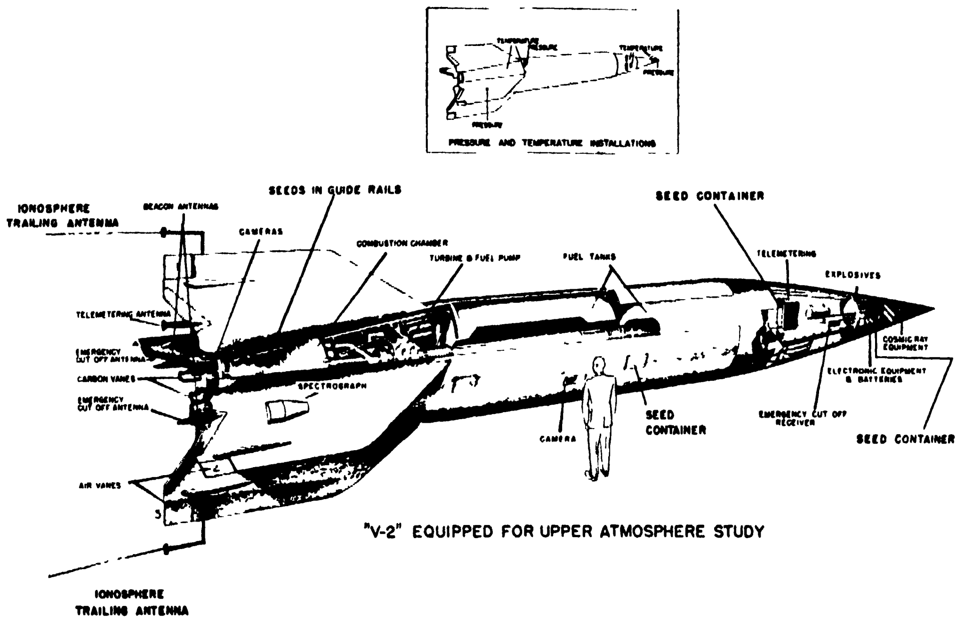


FIG. 6. SCHEMATIC DIAGRAM OF V-2 INSTRUMENTATION

of one group and that of the next group always being exactly 0.00555 second.

The time spacings of the pulses within a group correspond to the various channels of the system. Thus, the spacing between the initial pulse and the second pulse may be said to correspond to channel No. 1 of the system; that between the second and third pulses, to channel No. 2; etc.

If, when a group of pulses is formed for radio transmission to the ground, a d-c voltage V_1 exists at the input to channel No. 1, the spacing between the first and second pulses is $50 + 30\ V_1$ microseconds. Similarly, if V_n exists at the input to channel No. n, the spacing between the nth and the transmission of the appropriately spaced pulses described above is a means of measuring those physical quantities from a distance.

The ground stations used at White Sands contain equipment for receiving the pulses transmitted from the rocket and for translating the pulse spacings back into the original voltages. The recovered voltages may then be applied to a recording oscillograph which provides a photographic record of them suitably separated into channels for subsequent analysis.

For example, in the October 10 rocket, a bellows gauge was coupled to a potentiometer which controlled the magnitude of the

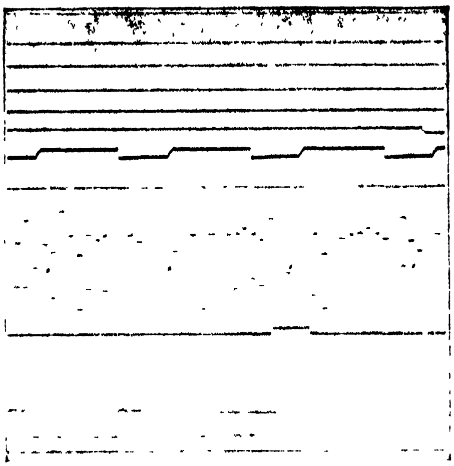


FIG 7. TELEMETERING RECORD

ARROWS INDICATE RISING VOLTAGE CORRESPONDING TO DECREASING PRESSURE IN BELLOWS GAUGE.

voltage applied to a specific channel. One end of the range of pressures to be measured was made to correspond to a zero voltage input to the telemetering channel, and the other end to 5 volts d-c, these limits being chosen since all voltages applied to telemetering channels were required to be between 0 and 5 volts d-c. A calibration curve of voltage versus pressure was obtained for the gauge before the flight and was used after the flight in the interpretation of the telemetering record. A portion of the telemetering record obtained for the gauge on the October 10 flight is shown in Figure 7.

The experiments which used telemetering for obtaining measurements were those on cosmic rays and on pressures and temperatures. In addition, various quantities connected with the rocket performance, such as motion of the carbon vanes used to direct the jet and thereby the missile, were telemetered.

The most exciting record from the October 10 flight, however, was not obtained by telemetering. The solar spectroscopy experiment depended upon recovery of a photographic film for getting data. Both the spectrograph and the film were recovered after the October 10 firing. Subsequent development of the film revealed the first spectrograms ever obtained from above the ozone layer in the earth's atmosphere.

A schematic drawing of the spectrograph which on October 10 made its historic journey into the upper atmosphere is shown in Figure 8. The diagram is to a large extent self-explanatory. As shown, the illuminators are small beads. The purpose in using bead illuminators was, of course, to obtain a wide-angle system to eliminate the need for tracking the sun, and tests showed that the use of such beads provided a greater illumination, requiring shorter film-exposure times, than did a conventional slit of comparable width together with a diffuser plate. The beads were 2 mm. in diameter and were made of carefully polished lithium fluoride, which is the most transparent substance known for ultraviolet light. The grating was ruled at The Johns Hopkins University. It was 4 cm. long by 3 cm high and consisted of 15,000 lines to the inch ruled on an aluminized spherical surface of 40 cm. radius of curvature.

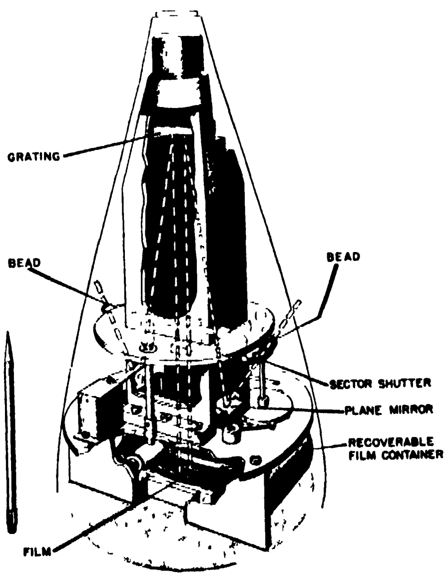
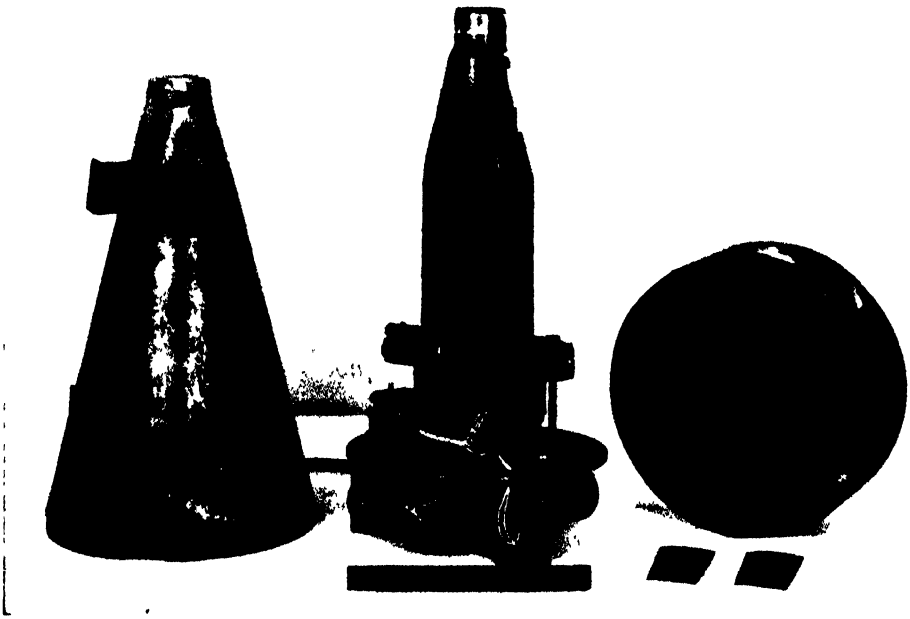


FIG. 8. SPECTROGRAPH, SCHEMATIC

Eastman 35-mm. 103-0 ultraviolet sensitized film recorded the spectra.

The purpose in using two beads was in effect to combine two spectrographs into one. The instrument was installed in Fin 2 of the rocket so that at the time of launching one bead looked south and the other looked north. Both beads were directed forward at an angle of 45° to the longitudinal axis of the rocket, and each was capable of directing the sun's light into the spectrograph even when the sun lay as far as 70° off the principal axis of the system. Since the rocket rolled after fuel burnout, when the stabilizing effect of the jet vanes was removed, there were periods during which the spectrograph film was insufficiently illuminated to produce useful spectrograms. The use of two beads in a single instrument considerably reduced the amount of wasted time. To avoid smearing of the spectrograms, the light from one bead was directed to one edge of the film, and that from the other bead was directed to the other edge.

The spectrograph itself, which was constructed by Baird Associates according to the Naval Research Laboratory design, is shown in Figure 9. This might easily be taken as a "before" picture. Actually, the photograph was taken after the instrument had been returned to the Naval Research Laboratory, following its recovery from the missile wreckage. Two small dents can be seen in the housing on the left in the figure. Except for slight pitting of the illuminator beads, presumably due to weathering in the desert, this is the only damage suffered by the spectrograph. Advantage is being taken of the excellent condition of the recovered instrument to run new calibrations on it in addition to those made before the flight.



Naval Research Laboratory Photo

FIG 9 SPECTROGRAPH RECOVERED AFTER OCTOBER 10 FLIGHT

Figure 10, which was published originally in *The Physical Review*,[1] shows some of the spectrograms obtained on October 10.

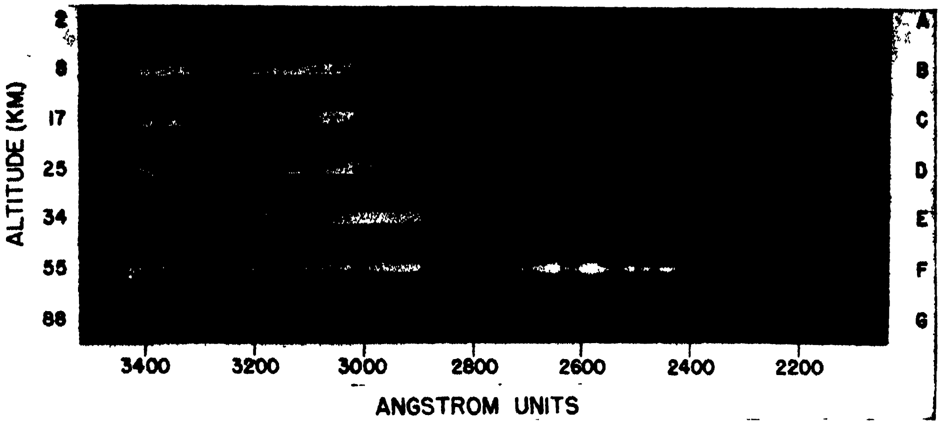


FIG 10 SPECTROGRAMS OBTAINED ON OCTOBER 10

The altitudes at which they were taken are shown at the left. One observes a progressive extension of the spectrum into the shorter wave lengths as altitude increases. At 34 km. the ozone absorption is still sufficiently strong to block out the wave lengths in the central region of the Hartley bands on either side of 2,550 A. Some radiation, however, does come through in the region below 2,200 A. On the other hand, it appears that very little ozone remains above 55 km. The spectrogram taken at that altitude shows a strong influx of radiation in the central portions of the Hartley region. By the time the rocket had reached 88 km., the sun was well off the optical axis of the spectrograph, and there was insufficient illumination at the shorter wave lengths to photograph. This explains the faintness of the spectrogram taken at 88 km. Malperformance of the spectrograph prevented the taking of spectrograms to the highest altitude attained by the rocket.

The taking of solar spectrograms from above the ozone was not the only "first" associated with the October 10 rocket. Cosmic-ray data obtained in that flight provided the material to complete the well-known Pfotzer curve of vertical intensity of cosmic radiation versus altitude, or, as it is usually plotted, versus atmospheric pressure. The first reliable determination of the point on that curve corresponding to zero pressure was made. Zero pressure was taken in the reduction of data to mean all pressures below 2 mm. of mercury, which in terms of altitude means any height in excess of approximately 40 km.

A schematic drawing of the counter array used in the cosmic-ray experiment appears in the upper right-hand corner of Figure 11, which was first published in *The Physical Review*.[2] The numbered circles denote Geiger counters arranged in the form of a multiple telescope which, as indicated, looked out at 45° to the vertical through a thin steel window.

The information telemetered during the flight was: (*a*) the coincidences $1\cdot2\cdot3\cdot\bar{4}\cdot5$; (*b*) the coincidences $1\cdot2\cdot3\cdot\bar{4}\cdot\bar{5}\cdot(6+7+8)$; (*c*) the coincidences $1\cdot2\cdot3\cdot\bar{4}\cdot5\cdot6\cdot7\cdot8$; and (*d*) the coincidences $1\cdot2\cdot3$. The notation used here is analogous to that of point set theory. The dot can be read as "and," the plus sign as "at least one of," and the superscript bar as "not." Thus a coincidence $1\cdot2\cdot3\cdot\bar{4}\cdot5\cdot(6+7+8)$ means a count simultaneously in counters 1 and 2 and 3 and at least one of 6, 7, and 8, but not in 4 and not in 5. Actually, the word "simultaneously," as used above, does not have the meaning "at one and the same time."

Rather, counts in a group of counters are considered to be simultaneous if they occur within such a small time interval of each other that the probability of their being caused by separate or unrelated particles is very remote. For the counting rates to be expected in the upper atmosphere it is safe to regard counts which occur within about 20 microseconds of each other as coincidences. The coincidence resolution employed in the October 10 experiment was about 5 microseconds.

A coincidence $1\cdot2\cdot3\cdot\bar{4}\cdot5$ must be caused by a ray or shower coming from a small solid angle about the direction of the line of centers of counters 1, 2, and 3. The recording of counters 4 and 5 in anticoincidence with 1, 2, and 3, that is, the requirement that 4 and 5 do not fire when 1, 2, and 3 do, eliminates all coincidences $1\cdot2\cdot3$ due to showers of particles entering from the sides of the telescope. Thus the number of coincidences $1\cdot2\cdot3\cdot\bar{4}\cdot5$ per unit time divided by the solid angle of the telescope gives the total cosmic radiation intensity from the direction of the steel window.

In a similar fashion, the coincidences $1\cdot2\cdot3\cdot\bar{4}\cdot\bar{5}\cdot(6 + 7 + 8)$ per unit time can be used to determine the intensity of the hard, that is, the penetrating or highly energetic, radiation from the direction of the window. Such a coincidence can be caused only by a particle or shower of sufficient energy either to penetrate 15.2 cm. of lead or to set off some sequence which persists through that much lead. In all this discussion the term "particle" is taken to include photons as well as electrons, protons, mesons, etc.

The coincidences $1\cdot2\cdot3\cdot\bar{4}\cdot\bar{5}\cdot6\cdot7\cdot8$ provide a measure of the hard shower count. Since in this case all of 6, 7, and 8 must fire in coincidence with 1, 2, and 3, a shower of at least 3 particles must emerge at the bottom of the lead.

Finally, the coincidences $1\cdot2\cdot3$ per unit time diminished by the coincidences

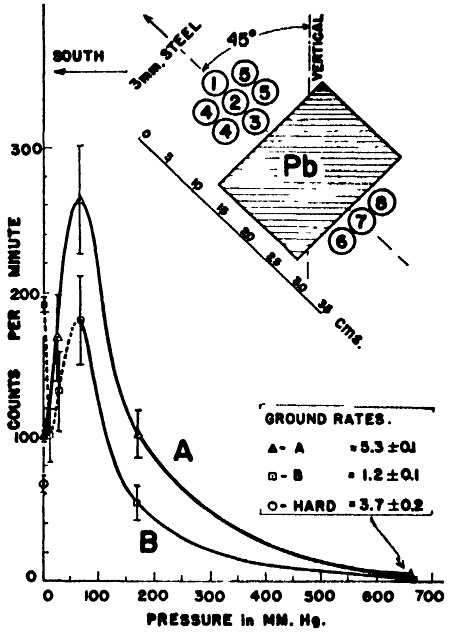


FIG. 11. COSMIC RAY INTENSITY

$1\cdot2\cdot3\cdot\bar{4}\cdot\bar{5}$ per unit time is a measure of the total shower rate.

The curves in the lower left-hand corner of Figure 11 show the results obtained on October 10 by measuring the coincidence rates discussed above. Curve A gives the measured total intensity of cosmic radiation from a 45° angle to the vertical. This is the curve which corresponds to the Pfotzer curve mentioned earlier. The Pfotzer curve, which gives vertical radiation intensity versus atmospheric pressure, has until now lacked the portion corresponding to altitudes above those attainable by balloons. In the previously known portions, the correspondence between A and the Pfotzer curve is good if appropriate account is taken of the fact that the two curves refer to different directions relative to the vertical. It follows that the free space point of A serves in effect to complete the Pfotzer curve. In fact, if, as seems likely, the cosmic radiation in free space is essentially isotropic, then A and the Pfotzer curve should come together at the zero pressure point.

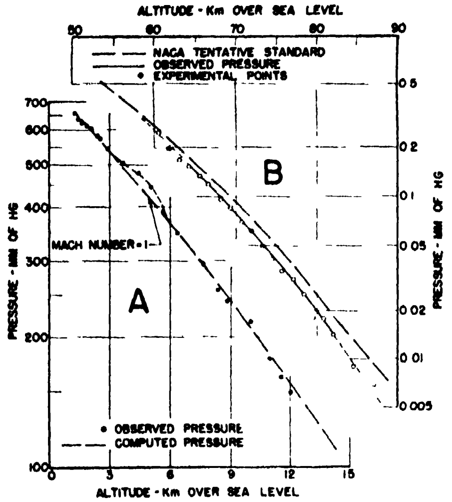


FIG. 12. PRESSURE CURVES, OCT. 10

The circle on the zero pressure axis gives the hard count in free space. The solid angle of the hard-count telescope was only .37 times that of the total-count telescope, and there were not enough counts at the altitudes below 2 mm. of mercury to provide reliable data. If it be desired, the ground point, determined by careful calibration prior to the rocket flight, can be used in conjunction with the curve already known for the altitudes reached by balloons, to fill in the hard-count curve. As can be seen, only about 70 percent of the radiation in free space is penetrating.

Finally, Curve *B* is a measure of the shower count. It was obtained as described above by subtracting the counts $1 \cdot 2 \cdot 3 \cdot \bar{4} \cdot \bar{5}$ from the counts $1 \cdot 2 \cdot 3$. The dip at 12 mm. of mercury is felt to be questionable. The high shower rate in free space appears to be reliable, however, and is surprising. Presumably the showers are formed in the warhead. But how? That is the question.

The curves of Figure 12, which also was published originally in *The Physical Review*,[3] show some of the data obtained on pressures in the upper atmosphere. The measurements were made by means of gauges installed in the tail of the October 10 rocket at points where German wind-tunnel data indicated ambient pressures would be measured.

The circles of Curve *A* denote pressures recorded by means of a sylphon bellows gauge, the telemetering record for which was shown in Figure 7. The broken-line curve was computed from balloon sounding data taken at El Paso on the morning of the flight. The fact that the observed and computed pressures agreed so well outside the ever-troublesome region of Mach number 1, when the missile's speed was in the neighborhood of that of sound, indicated that at the location of the bellows gauge the pressures were actually ambient pressures.

It was therefore assumed that the readings of a Pirani gauge similarly located in the tail of the V-2 gave ambient pressures directly These observed pressures are plotted as the circles in *B*. The broken-line curve is the tentative standard of the National Advisory Committee for Aeronautics, inserted for comparison purposes. It is seen that the measured points exhibit a curvature similar to that of the NACA standard. This curvature corresponds to a negative temperature gradient. The measurements may, therefore, be taken as partial confirmation of the existence of an atmospheric temperature which decreases with altitude in the range from 60 to 80 km.

The inflection in the NACA curve just beyond 80 km. appears to exist in the curve of observed pressures, but there are in this region too few measured pressures from which to draw any firm conclusions.

The October 10 rocket also carried equipment for measuring ion density in the ionosphere. As it happened, however, the loss of one of the trailing wire antennas at launching prevented successful performance of the experiment.

Other experiments, installations, and measuring equipment were included in the October 10 V-2 instrumentation. Temperature measurements were made in addition

to those of pressure. A timer was included to detonate the explosive for separating the warhead from the afterpart of the rocket. Auxiliary measurements were made for the various experiments. For example, the motion of the spectrograph film was telemetered to provide a means for determining the altitudes at which the various spectrograms were taken. A Naval Research Laboratory ejection device, for the recovery of special equipment and records, was tested during the October 10 flight. The Army also included an ejection device of its own.

It is interesting, and perhaps ironic, that the only things of any real importance that were not recovered from the October 10 flight were the recovery devices. All evidence points to the fact that the ejection devices did operate properly, but, since the containers were ejected long before the missile broke up, they presumably do not lie in the area over which the missile wreckage was strewn. Thus, the failure of the ejection experiments appears to be due to the difficulty of locating the containers after they have landed, rather than in getting them safely to earth. One can readily think of many schemes for overcoming that difficulty, but for the present the ejection experiments are being curtailed owing to the highly gratifying success so far attendant upon the use of the air-burst method.

A highly detailed and technical account of the Naval Research Laboratory's participation in the V-2 program appears in two specially prepared reports.[4, 5] These cover the Laboratory's accomplishments in upper-atmosphere research through the October 10 firing.

It is plain that the performance of as many experiments as described above, all at once, and in the same rocket, requires considerable manpower and very careful coordination of effort. The Naval Research Laboratory has at present a large group of professional men engaged solely in upper-atmosphere studies. E. H. Krause directs the Laboratory's participation in high-altitude research, a major part of which at the present time is associated with the V-2 program. For the October 10 flight G. J. Perlow directed the cosmic-ray work. C. V. Strain and R. Tousey maintained cooperative direction of the experiment in solar spectroscopy. The temperature and pressure measurements were conducted by N. R. Best and R. J. Havens. The work on the ionosphere experiment is progressing under T. R. Burnight. C. H. Smith is in charge of the electronic instrumentation and rocket services. He is assisted by M. W. Rosen and J. T. Mengel. The author directs the work of a theoretical group.

All this, however, is not the whole story. To get a complete picture of the effort being put forth on the upper-atmosphere program, one must count, in addition to the men listed above, shop personnel, machinists, sheet-metal workers, glass blowers, draftsmen, and outside contractors. And to these must be added those engaged in assembling, servicing, and firing the rockets, and those engaged in tracking and photographing the missiles during flight.

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A LAYMAN LOOKS AT SCIENCE

By JOHN L. CHAMBLISS
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NOT long ago my young son, who is in the sixth grade, came home from school with the announcement that he had learned about the origin of the earth. "There used to be just the sun," he explained, "and then a star came along and came too close to it, and by the force of gravitation it drew out a sort of streamer that cooled and made the planets and the earth."

I said that was the theory, though of course it hadn't been proven.

"That's why all the planets are going around the sun."

I said yes. "But don't forget it's just a theory. It's what the scientists call a hypothesis."

My words, I am afraid, made little impression on him. He had learned something new—how the earth had originated—and the distinction between "theory" and "truth" was too much for him to make.

I came across the following in a newspaper editorial recently, written on the occasion of the quadricentennial of Tycho Brahe: "He was the pioneer in the affirmation of the supreme value of data. The impact of his career upon the existence of people now living cannot be exaggerated. He was, indeed, one of the principal architects of the world of today."

Though one may be pardoned for thinking that the great Danish astronomer might find the last remark a rather dubious compliment, there is no denying that science considers itself, and justly, as the principal architect of the twentieth-century world, and by this is usually meant that the material conditions of life today are directly or indirectly traceable to science.

There is, however, a more fundamental way in which science is the "architect" of our world: what might be called its subjective creation as distinguished from its objective creations in the world around us. For the layman who chooses to think about such things, it is, perhaps, more important than the visible evidences of everyday life. I refer to that *world-picture* which science has given to modern man, his "cosmological environment" which is the result of the discoveries of the past four centuries.

When my eleven-year-old son learned in school that the earth originated in such and such a way (and because of his years failed to distinguish between "truth" and "hypothesis"), he was adding all unknowingly to his "cosmological environment"—that picture of the universe which he will carry the rest of his life.

It may seem an impertinence for a layman to express himself on scientific matters, even those of a general nature which come home to him most closely. Science in recent years has taken on almost the status of a priestcraft in the minds of many laymen: we are made aware that its discoveries, which overshadow our lives, are comprehensible only by the few.

On the other hand, every man has the right to philosophize on general questions, and by "general questions" I mean that picture of the world and life and God which is the foundation of our very existence.

That a man is conditioned by his environment we all know, and it should be equally plain that men in general are affected by their larger environment, that "cosmological environment" in which they share as partakers of the same civilization. It should be obvious, I think, to any reasonable man that if the men of any age picture themselves

as living in a universe in which they do not really belong, in which their God is an abstraction, their earth the result of a cosmic accident, and they themselves and life itself but fortuitous phenomena, having no real status in the universe as a whole: if this be their world-picture, it is bound to be reflected in their lives and in their civilization.

Man is distinguished from the lower animals by the fact that he knows he is a Man, and it is this self-consciousness which, in a sense, *makes* him a Man. The first Man was he, perhaps, who first made this discovery, and by that act *became* a Man, and it is worth remarking that the tribal name of primitive peoples is simply "Men."

But the corollary of this self-consciousness, which has lifted a good portion of mankind from savagery to civilization, is man's belief in his own worth and dignity. Destroy that belief, convince men in general that they and all their works are of no ultimate consequence, and though man may continue to exist as a higher animal, he will in time return to savagery. For we may lay it down as a general proposition, I think, that the first necessity of creative man, individually and collectively, is a belief in his own importance.

Among scientific writers of the past thirty years or so it has been the fashion to stress the vast extent of the universe, the insignificance of the earth and the solar system in this scheme of things, and the corresponding insignificance of man. Scientists, it would seem, have conceived it as their duty to inform the rest of us that the human race is not nearly so important as men have supposed in the past, and that even life—which for us is the basis of all perception and thought—is but a sort of by-product in the greater universe.

It may be that good intentions lie behind these teachings, and the scientist would doubtless say that he is merely presenting the world as it is; on the other hand, the human urge to dramatize probably has something to do with it, nor should we forget that, in painting this picture, the scientist has recourse to theory as well as fact, while over it all lies his own personal interpretation.

I should like to illustrate this difference between fact and interpretation, since it lies at the heart of any discussion of our modern world-picture. I quote from a book published twenty years ago but still on the shelves of many libraries, Bertrand Russell's *Philosophy* (chap. ix, The Structure of the Atom):

> The main point for the philosopher in the modern theory is the disappearance of matter as a "thing" It has been replaced by emanations from a locality —the sort of influences that characterise haunted rooms in ghost stories. As we shall see . , the theory of relativity leads to a similar destruction of the solidity of matter, by a different line of argument. All sorts of events happen in the physical world, but tables and chairs, the sun and moon, and even our daily bread, have become pale abstractions, mere laws exhibited in the successions of events which radiate from certain regions

We have here, it will be seen, certain statements and certain inferences drawn from those statements. According to the theory of modern physics (now abundantly verified) the atom is of such and such a nature: a solar system in miniature, we will say, whose "sun" and "planets" are mere radiation points. Very well, says the layman; such is the nature of the atom. Now let us follow our scientist-philosopher as he proceeds to develop this theme.

Matter, we are told, has disappeared, as a "thing." It has been replaced by "emanations from a locality the sort of influences that characterise haunted rooms in ghost stories." And further on we read that "tables and chairs, the sun and moon, and even our daily bread, have become pale abstractions," and so on.

It will be said that these are mere figures of speech. Nonetheless, let us analyze the meaning behind these statements, which are not untypical of the sort of thing the lay-

man reads today and which are instrumental in fashioning his world-picture.

Matter, we read, has disappeared, as a "thing." We ask therefore what constitutes a *thing*, in the eyes of the scientist. Is a "thing" any less a "thing" because we have learned that its ultimate constituent is a system of energy?

What did the scientist expect to find when he started out to investigate the properties of matter? Surely he did not expect to find that a piece of bread was always a piece of bread. Or that a chair was always a chair. Why, then, having discovered that the fabric of the universe is a *unity* which we call energy, does he assume that the visible world is any less "real" than it was before?

Plainly, we are dealing here with something more than the bare data of science. Nor can the scientist, who thus adds his own personal interpretation to his data, honestly claim to be presenting the world as it is. What science has proven in the past thirty years, it would seem to a layman, is that the visible world of multiplicity is composed of an invisible world of unity, and is itself the *form* of that invisible: a discovery foreshadowed two thousand years ago in the Greek philosophers. But this does not imply that the visible world is any less "real" than it was before; nor does it mean that we are all characters in a ghost story.

These features are the addition of the individual scientist in interpreting his data —an interpretation not uncharacteristic of the way present-day science explains the world to the layman.

It will thus be seen that our modern world-picture is only in part the result of scientific data, that it depends also on the way those data are interpreted. And it is this act of interpretation with which I am concerned.

The unscientific layman who would accept as a matter of course any number of scientific discoveries is not necessarily bound to accept with equal faith the scientific interpretation of those discoveries. We may ask, indeed, whether there is any such thing as "scientific interpretation." One might as well speak of "scientific appreciation" or "scientific enthusiasm." Interpretation, one supposes, is largely a matter for the individual mind, unless we allow the authority of religion or philosophy.

Modern science, however, has long since abandoned religion as a means of interpreting its data, and it would seem that even philosophy has been put aside, if we mean by "philosophy" *common sense and genuine seriousness*—qualities not notably present in the quotation given above. Instead, science has pinned its faith to "the supreme value of data," with the result that today —although the storerooms of science are bulging with facts—the layman has difficulty finding a philosophical scientist who can adequately interpret those facts.

One thinks of Arthur Eddington and Sir James Jeans in this connection, along with other noted scientists who have addressed themselves to this task, but it seems to me that even they fail to appreciate the *seriousness* of what they are doing: like Bertrand Russell, they like to dramatize their material and, if possible, throw a scare into the reader.

They tell us, in substance (as we all know), that the universe is apparently the work of a Great Mathematician, for whom, it would seem, the earth and life and mankind are matters of the extremest inconsequence. They give us, in other words, a world-picture the very opposite of that postulated by any religion, wherein the importance of mankind in the eyes of the Creator is a primary requisite.

It follows naturally, if we accept their scheme of things, that there is no argument for virtue but expediency, no meaning whatever in such words as "love," "honor," and "sacrifice;" and even science is left with

little on which to stand, since there is no reason for supposing that in such a cosmos man can get even a glimpse of the truth.

These things, of course, we cannot accept; even the scientist balks at drawing these inevitable inferences, and so it is precisely at this point that he bows out of the picture, as a scientist, leaving the layman to work out his own salvation.

We have, therefore, as an essential feature of the twentieth-century mind, a new type of *dualism.* For on the one hand we have this scientific world-picture, wherein man and all his works are of no final significance; and on the other hand, varying with each individual, we get a personal compensatory creed, which may range all the way from humanitarianism to dogmatic revealed religion. And this new dualism is no less a characteristic of the scientist than it is of the layman. No less than the layman, the scientist must have something to believe in, over and above this world which he explores; he may be an atheist, but even as an atheist he is forced to believe in the essential worth of his own quest of the truth.

With all this, it would seem to any reasonable, intelligent man, there is something fundamentally wrong.

The world is not divided into two compartments, Science and Faith, or whatever we choose to call them; and if it *is* so divided—or, rather, if that is what we believe - then it is time we realized it.

We are told today, by both scientists and religious leaders, that there is no "real conflict" between science and religion, because each has "its own field." The truth is rather that there has never been a greater conflict between the world-picture of science and the teachings of religion; only they are now so far apart that there is no longer any contest.

If this scientific world-picture were solely a matter of incontrovertible fact, there would be no choice for the rational layman but to accept it unreservedly. But, as we have seen, it is not purely a matter of fact, for it depends also on the way facts are interpreted, which is largely a function of the present scientific outlook.

I will cite one other example: that theory of the origin of the earth mentioned at the beginning, which my young son recently learned in school.

According to this hypothesis, which is known, I believe, as the Tidal Disruptive Theory and which was propounded some years ago by Sir James Jeans as a variation on the Chamberlin-Moulton planetesimal hypothesis, the solar system had its origin in the chance approach of a passing star which, coming too near the sun, drew out from that body a gaseous filament which in time cooled and solidified to form the planets, including our earth.

With the technical aspects of this theory I am wholly incompetent to deal, nor am I interested in them. I am interested in the state of mind behind this conception, whereby the earth and the solar system are conceived as the result of an *accident.*

Here, it would seem, is one of the great problems of science: to account for the origin and the history of the earth. And in this hypothesis by one of the world's most noted scientists, we have, surely, a fair example of today's scientific outlook in its attempt to explain the universe.

The result, as we can see, is a hypothesis in which there is no slightest suggestion that there is a *purpose* in the universe, or that this earth on which we live is the result of any *plan.* Instead, we have a theory of which the scientist asks but one question: Does it conform to known data?

I am not so naive as to expect the hand of God in a scientific theory (though, after all, it is as easy to believe in a miracle as to believe in an accident), but I do expect such a theory to be reasonable and to take some account of other data besides the bare bones of physics and mathematics. And this theory is not reasonable. Taken as one aspect of the

modern world-picture and apart from the limited field of science, it is merely the obverse of the Book of Genesis, carried to the point of absurdity, and is equally unacceptable by any sensible man.

For if we thus account for the origin of the solar system and of this earth we live on, then the idea of a Guiding Intelligence behind the universe goes up in a puff of smoke. Our belief in an order and a reason in the universe likewise goes up in smoke, for we say in effect that *by virtue of this accident, we now possess all the order of the universe.*

Science presupposes an Intelligence in the world; otherwise there could be no science. But Intelligence is not merely Intelligence: Intelligence operates toward an *end.* It cannot be abstracted from the will and made into a thing-in-itself, because it is an instrument of the will. The very concept of Intelligence, whether human or divine, implies a *plan* and a *purpose.*

But in this Tidal Disruptive Theory of Sir James Jeans which is being taught my young son, there is no plan, there is no purpose, there is no meaning and there is no sense. It is purely the attempt of a physicist and mathematician to devise an explanation for the origin of the solar system that will conform to certain scientific data.

I did not set out to deliver a blast against science, for I do not conceive myself as a David to tackle such a Goliath. Moreover, the evidence of every man's debt to science is too apparent and ever-present to be questioned by a sensible person.

But it seems to me that if science, as a *logos* of the world, is to continue to command the respect of the thoughtful layman, it must take a new stock of what is called today "the scientific outlook."

We have come a long way from the days when Tycho Brahe, Bacon, and Galileo, in reaction against Scholasticism, affirmed "the supreme value of data." Science, it would appear, is now endangered by a new scholasticism—the Scholasticism of Empiricism, wherein overspecialization and quibbling over data lead to results no less barren than the "concepts" of the Schoolmen.

The first job of science is *to explain the world to man*—not to give him radios, airplanes, and atomic bombs. And in the long run this explanation must command his inward assent, not merely a blind submission to the omniscience of science.

The layman who takes it upon himself, as a rational human being, to examine the present scientific world-picture in its relation to his personal credo (whatever that credo may be) is led inevitably to the conclusion that, for all its remarkable discoveries, science has yet to work out a reasonable picture of the whole.

Such a picture, of course, must rest upon facts.

But there must be also a philosophy behind those facts, and established principles in the light of which to regard them.

CARTOHYPNOSIS*

By S. W. BOGGS
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MANY primitive societies are quite unaccustomed to maps. For them, territorial and boundary questions are relatively simple and radically different from those of map-conscious nations. For example, when two tribes in a certain region near the Indian-Afghan frontier find difficulty in agreeing upon a common tribal boundary they sometimes have recourse, as recounted by Colonel A. H. McMahon, to laying down a boundary "by oath." A leading man of one side is prevailed upon to undergo the ordea and is accepted by both sides. Holding the Koran firmly on his bare head, the soles of his feet being bare and cleansed of every particle of his own tribal soil, and having taken every precaution to save his soul from perjury, in a scene full of excited tribesmen he steps out, and the course he follows becomes the unquestioned boundary line. It may unexpectedly diverge widely from both claims, but salient points are sometimes found to be marked by crumbling rock cairns of great age whose existence had long been forgotten. Boundary-makers of many nations wish their tasks were as simple and easy!

Map-conscious people, however, usually accept subconsciously and uncritically the ideas that are suggested to them by maps. In part because maps appear to represent facts pertaining to mother earth herself, veracity and authority are frequently attributed to them beyond their deserts. In what may be called "cartohypnosis," or "hypnotism by cartography," the map user or the audience exhibits a high degree of suggestibility in respect to stimuli aroused by the map and its explanatory text.

Sometimes self-hypnotism and illusion occur quite innocently. And frequently a sort of mass hypnotism is practiced by men who attempt to delude the public. Maps may also be used effectively to dehypnotize people; we should therefore consider what maps may be made, and how they may be used to awaken people to an intelligent understanding of the world and the problems of our times.

Illusion and confusion. The innocence of some people's illusions when they look at maps uncritically reminds one of a four-year-old child's question: "Why do I see things when I shut my eyes that aren't there when I open them?" People often suppose that maps reveal "facts" which, if they were wide-awake to the maps, they would realize are not shown at all. An example of illusion and confusion, arising from use of the over-familiar Mercator projection, is shown on the accompanying map (Fig. 1*a*) on which there is added a long straight line indicating the true compass course known to mariners as "east by north." On the Mercator map every continuous true compass direction is a straight line,[1] whereas on the earth all such

* From a paper presented at the Fourth Assembly of the Pan American Institute of Geography and History, Section II, Caracas, Venezuela, August 29, 1946.

[1] The Mercator map projection is one in which the parallels of latitude, represented by straight lines, are mathematically spaced in such a way that, at any point on the map, the north-south and east-west scales are identical. In consequence, every true compass direction or course is a straight line of indefinite extension; this property makes it especially useful to navigators in plotting courses at sea. The projection is *not* projected onto a cylinder from the center of the globe, as is often depicted—a very different and quite useless map.

lines are spirals—except the meridians (great circles) and the parallels (small circle). The $E \times N$ line on Figure 1*b* illustrates how such a spiral goes around the earth an infinite number of times without actually reaching the North Pole.

Illusion may occur when men use world maps instead of globes in seeking to understand some of the world relationships of our times. Observe, for example, a world map (Fig. 2*a*) prepared by a brilliant geographer, the late Professor Mackinder (Sir Halford), to illustrate his famous paper on "The Geographical Pivot of History," which he read in 1904. The map was made on the Mercator projection; its limiting border is an ellipse; and parts of North and South America are repeated at the left and right sides of the map. The "pivot area," or "heartland," in north-central Asia (which was for many centuries a region of horse and camel mobility insulated from the continental margins in large degree by deserts) is shown on the map as being bordered by an inner or marginal crescent of land accessible to ships, paralleled by an outer crescent of continents and islands festooned across the map. That

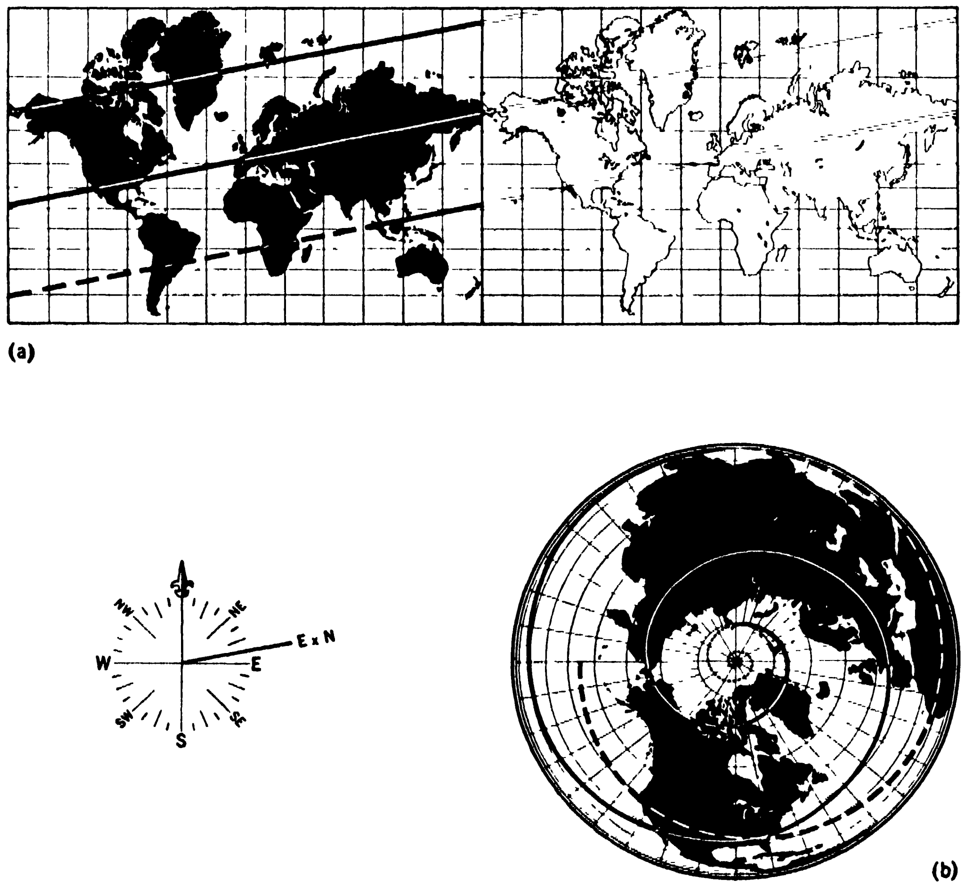

FIG. 1. "EAST BY NORTH"

THE COMPASS COURSE KNOWN TO MARINERS AS "EAST BY NORTH" (N. 78° 45′ E) IS A SPIRAL THAT NEVER QUITE REACHES THE NORTH POLE. ON (*a*), THE MERCATOR MAP, IT IS A STRAIGHT LINE, WHICH WOULD BE CONTINUOUS IF THE MAP WERE REPEATED INDEFINITELY. ON (*b*), THE NORTHERN HEMISPHERE MAP, IT APPEARS AS A TRUE SPIRAL—WHICH IS DOTTED IN ITS POLEWARD EXTENSION BEYOND THE MERCATOR MAP LIMIT. IN THE SOUTHERN HEMISPHERE ON BOTH MAPS THE COMPASS COURSE IS CONTINUED AS A BROKEN OR DASHED LINE.

is how it appears on a particular map. When the Mackinder map is traced on a transparency and wrapped around a cylinder (and the Mercator map is, of course, developed mathematically as on a cylinder), the repeated areas will overlap (Fig. 2*b*). But what is it like on the earth itself? As seen on the globe (approximated by the circular world map, Fig. 2*c*), the left and right portions of the elliptical Mackinder world map correspond to a single lens-shaped area embracing that portion of the Americas which appears twice. To the north and south there are loops enclosing the polar regions which are altogether missing from the map. The remainder of the earth's surface appears on the Mackinder map once and once only. The so-called outer crescent, whose ends overlap in the Americas and which traverses Australia and southern Africa, is seen on the globe as a belt obliquely encircling the earth; also, the Arctic area is seen in its spherical compactness in normal relationships to Eurasia and North America.

Professor Mackinder's own concept of "global realities" was clearly revealed in these words: ". . . We shall . . . have to deal with a closed political system . . . of world-wide scope. Every explosion of social forces . . . will be sharply re-echoed from the far side of the globe." However, it would seem that the Mercator map suggested to its author the concept of an outer crescent, instead of what in reality is approximately an oblique circle; it seems to have suggested, also, an oversimplified generalization in a sort of geometrical pattern of historical relationships. Much of Mackinder's paper, with the salutary critical comment that followed its presentation, is almost forgotten. In any event, the map subsequently exerted a hypnotic influence on many thousands of people, for it was reproduced at least four times in the Nazi literature of so-called geopolitics, with perversions of the author's original intent which were designed to serve malevolent purposes in propaganda.

Delusion by design. Maps are often deliberately employed to "sell" ideas—to individuals and nations. In every continent maps have been used, and are now being used, to disseminate mischievous half-truths and to obfuscate the thinking of men. They are employed as graphic devices—subtly to suggest an idea, to inculcate a prejudice, or to instill patriotic fervor. Such maps may be true in every detail, but in their omissions and their perverse emphases they may be socially poisonous—as chlorine by itself is a poisonous gas but an essential element in common salt.

In an article entitled "Magic Cartography" (*Social Research*, September 1941), relating to the uses of maps in propaganda, Hans Speier observes:

> The use of maps in propaganda is dependent upon highly developed techniques of map making and reproduction, a certain minimum of mass education in reading cartographic symbols and a specific organization of society. This organization may be characterized briefly as one in which the individual's functional dependence and loyalties extend far beyond the area of his immediate experiences.
>
> . . . [Maps] may make certain traits and properties of the world they depict more intelligible—or may distort or deny them . . . . They may give information, but they may also plead. Maps can be symbols of conquest or tokens of revenge, instruments for airing grievances or expressions of pride. Indeed, maps are so widely used in propaganda and for such different purposes that it is difficult to understand why propaganda analysts have paid so little attention to them.
>
> Propagandists . . . rediscover . . . symbolic values in maps, and by exploiting them, turn geography into a kind of magic. . . . The progagandist's primary concern is never the truth of an idea but its successful communication to a public.
>
> Entirely new possibilities in the use of maps for political propaganda are revealed by the *film*. The German propagandists have realized that . . . [when they produced] moving maps.
>
> . . . [Maps] are essentially scientific. The propagandist who uses them borrows the prestige of science and at the same time violates its spirit.

Chimerical cartography was effectively employed in the propagation of ideas by the Nazi geopoliticians. Dr. K. Frenzel, addressing the German Cartographic Society in Berlin, October 22, 1938, declared: "Every map has a suggestive force! Man is an ocular creature. He reacts to that which he sees and can take in at a glance." The private cartographic industry was declared to bear a very heavy responsibility as a mediator between science and the people, and between the policies of the

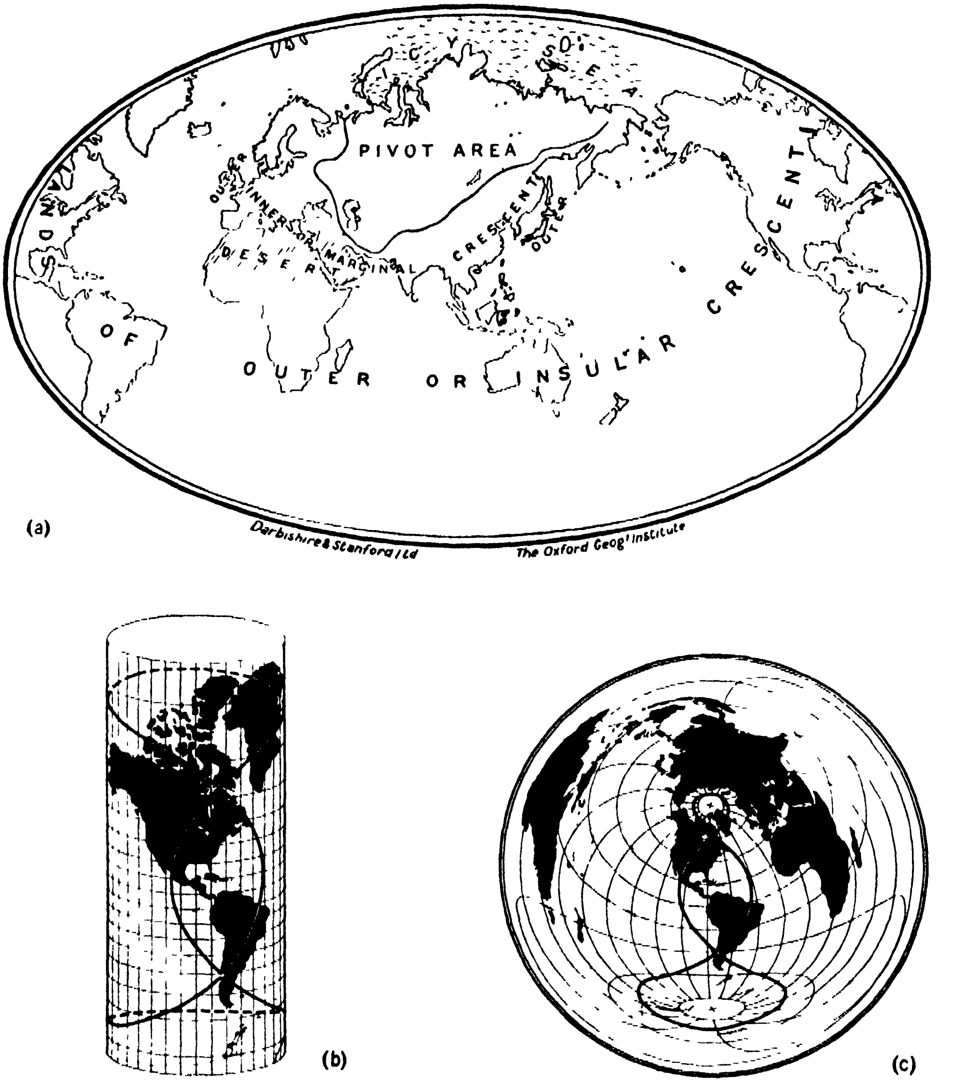

FIG. 2. MACKINDER'S MAP, "THE NATURAL SEATS OF POWER"

(*a*) FACSIMILE OF A MAP BY THE LATE PROFESSOR HALFORD MACKINDER TO ILLUSTRATE A LECTURE ON "THE GEOGRAPHICAL PIVOT OF HISTORY" IN JANUARY 1904. ALTHOUGH ON THE MERCATOR PROJECTION (WITHOUT PARALLELS AND MERIDIANS), IT IS IN THE FORM OF AN ELLIPSE. A PORTION OF THE AMERICAS IS REPEATED AT LEFT AND RIGHT. (*b*) THE ELLIPTICAL LIMITS OF THE SAME MAP SHOWN ON A MERCATOR MAP WRAPPED AROUND A CYLINDER. (*c*) THE LIMITS OF THE SAME MAP SHOWN ON AN AZIMUTHAL MAP. THE LENS-SHAPED AREA IN THE AMERICAS APPEARS TWICE ON MACKINDER'S MAP; THE LOOPS AROUND THE NORTH AND SOUTH POLES ARE WHOLLY MISSING ON THE MACKINDER MAP; THE REMAINDER APPEARS ONLY ONCE ON MACKINDER'S MAP.

government and the people. Every map had to be submitted before publication to all government departments that might have an interest in it. An obligatory organization of several large publishers of school atlases was created in order that unified school atlases would be published for the whole Reich.

Special symbols and devices were developed and standardized by frenetic propagandists, adapted to a minimum of mass education in reading maps, in order to convey ideas of threatening forces, attack and resistance to attack, hostile encirclement, and the like. Posters in railway stations and other public places utilized maps that had a powerful effect upon the uncritical mass of the population.

In Italy cartography was employed by Mussolini to stimulate an urge for territorial expansion. Most striking, perhaps, was the series of maps on a wall in Rome, erected on the Via Imperiale, a new boulevard cut through from the National Monument to the restored Forum. On these maps, which were executed in choice marbles of selected colors, the growth of Rome from a city-state to the empires of Augustus and Trajan was artistically depicted. The purpose was obvious, the method artful. The dominion of Rome once encircled the Mediterranean. Modern Italy's destiny seemed manifest; *Mare Nostrum* was again used with the present tense. No critical appraisal of the lack of pertinence of the extent of Trajan's conquests to the role that can or should be played by Italians in the twentieth-century world was ever tolerated.

The map of Hungary, in a park in Budapest, delineated in a pattern of flowers and foliage which portrayed Hungary's former and current territorial extent, was for years a striking example of cartographic propaganda. Surrounded by stirring words of a famous Magyar poetess spelled out in the foliage and with the national flag nearby always at half-staff in perpetual mourning for territories lost after the first world war this map never allowed the people to accept the imposed territorial changes.

Intelligent use of maps. In a distraught world, whose teeming millions sometimes hesitatingly follow their leaders and would-be leaders as they pick their way among the rubble of shattered cities and ideas, honest and critical thinking about maps is important. Men, women, and even children should all be more critical of the maps they see in daily papers and in periodicals, in books and atlases, and on the screen. They need to be taught to *read* maps (an art in itself) and not merely to *consult* maps—frequently only for location of a single city or point, or regarding a route of travel. Economists, historians, political scientists, and others need to cultivate a keener sense of earth distributions of resources and of peoples and their activities—which necessitates development of ability to read distributional maps.

Cartohypnosis is no more common, however, than delusion and confusion of the mind by subtle uses of words and phrases—but it is perhaps more difficult for the average man to protect himself against the use of mischievous maps. Even the phrases of the most honest men are sometimes inadequate, for, as Whitehead remarked in *Adventure of Ideas*, "The success of language in conveying information is vastly overrated, especially in learned circles."

The map user who desires to guard against becoming the victim of cartohypnosis should keep in mind three things:

(*a*) that it is the actual situation on the earth that is significant;

(*b*) that maps have definite limitations as well as certain unique capabilities; and

(*c*) that map makers are human.

(*a*) It is what one would find on the ground, in all its complexity, and not simply what one finds on a map, that is significant. In looking at a map one may

well ask: What the map shows may be perfectly true, but what is the whole truth? What is on the ground- including peoples and their customs, their ideas, and prejudices? What other types of information are pertinent to the subject?

A small-scale map in a newspaper, designed to indicate territorial transfers and boundary changes, cannot reveal the bilingual populations and the economic and cultural ties between peoples throughout the region. Men could not be so sanguine of solving some of the present perplexing political problems by means of shifting international boundaries, in areas in which boundary changes are ardently advocated (always in other people's territory), if some of the mappable data regarding economic interdependence and cultural transition zones were adequately visualized on maps.

(*b*) Maps have definite limitations as well as unique capabilities. Like an aerial photograph that reveals a pattern, perhaps of archeological origin, almost erased by time and imperceptible on the ground—or like an X-ray photograph—a map may disclose patterns of great significance which are not discernible on surface inspection. Many maps based on statistical data thus reveal pertinent invisible transitions which, if even suspected, would be only vaguely perceived on visiting the area.

A map is unique in its capacity to represent with fidelity literally millions of observed facts, accurately generalized and artistically presented, conveying to the mind a vivid, true picture of the distribution of certain phenomena on the earth's surface that could not be obtained in any other manner. Large-scale topographic maps, for example, if they are highly accurate, belong to this category.

But the limitations of a map should be borne in mind. One of the most important is that a map cannot be more accurate and reliable than the data upon which it is based. A map printed in beautiful colors may be of little value and may mislead the uncritical, especially if it be a work of art. On the other hand, a crudely executed map compilation may be highly accurate and of the greatest importance.

People seldom consider that a map is like a single chapter in an encyclopedic compendium; one map cannot present the results of an inventory of geology, natural vegetation, and water resources. Any map that attempts to show too much is of little use.

Use of the Mercator projection for world maps should be abjured by authors and publishers for all purposes. The world is round. No man ever saw or will ever see a world that has much resemblance to the Mercator world map, and the misconceptions it has engendered have done infinite harm. A map that makes Greenland look larger than all South America, instead of smaller than Argentina, is not suited to portray world relationships. The Mercator is ideal only for navigation, each chart covering a relatively small area. Discrimination should always be exercised in selecting map bases for world maps, the choice depending upon the data or the relationships to be represented.

In this so-called air age, in which men glibly talk of "global relations"—which are misleadingly visualized on *all* world maps, polar and otherwise—one ventures to suggest that the phrase "global geography" should be restricted to those aspects of world relations that can be rationally comprehended, without geometrical acrobatics, only with the aid of globes. I find that transparent plastic hemispheres, some with geographical patterns, and others with geometrical patterns imprinted, which can be moved into any position upon a globe resting only in a cup or ring, provide the best means of comparisons between one part of the globe and another. Map projection distortions and differences of scale are completely eliminated. Having seen a

situation clearly on the globe itself, a map projection may be selected which is adapted to the special requirements of visualizing that particular set of data. There are, to be sure, many types of data which may be grasped when presented on maps even better than on globes. But there are other categories of highly significant relationships, notably the longer ocean-trade routes, air routes and distances, radio and other wave propagations in the field of electronics, and problems relating to the peaceful development of atomic energy for the benefit of all mankind, which require the use of globes and certain types of accessories and actually deserve the appellation "global geography."

(c) Maps are made by men, and, as Dr. John K. Wright has remarked (*Geog. Rev.*, October 1942), "Map makers are human." Scientific integrity, painstaking accuracy, and cartographic skill are essential qualifications of the maker of the maps upon which the map user can rely.

Maps that ought to be made and used. Maps are not an end in themselves. If maps can be used as weapons, as Napoleon intimated, they can also serve the needs of peace. Maps can play a unique part as aids in the analysis and solution of complex problems and as tools in planning on community, national, and world scales. One of the most important uses to which maps can be put is to dehypnotize people, to wake them up to the facts and phenomena of the mid-twentieth century, and to educate them to world understanding. Where words utterly fail, maps can sometimes portray, vividly and memorably, some of the freshly and sharply etched, but as yet dimly perceived, lines of interplay between peoples in a world which in many areas is scarcely reminiscent of the conditions upon which our thinking is largely premised. We should bear in mind that until the nineteenth century there were no "world problems."

As a specific example among hundreds that could be suggested: In animated motion-picture maps the areas to which goods produced in Canton can be transported at equal increments of cost can be delineated. As the expanding waves of motion sweep across the oceans and along the railroads of Europe, the Americas, and Africa, they creep almost imperceptibly across China beyond the meager pattern of railroads and motor roads, revealing that villages less than five hundred miles from Canton are much farther away, in terms of transport-cost, than Omaha or Jerusalem. If four-fifths of the people of China cannot trade with each other they cannot trade with the United States and other countries. To see a number of such examples for different continents on animated maps is to grasp the relationship between the lack of modern transport and the "timeworn misery" of low levels of living in large areas of the world.

The world needs maps that visualize economic interdependence of countries and regions; that locate the principal natural resources and their volume of production; that correlate the volume of commerce with decreasing costs of production and transport and that reveal the increases of trade over both short and great distances; that reflect trade balances and international balances of payments; that depict the rapidly expanding patterns of communication in terms of both total and per capita volume; that record the rapidly changing levels of living; that trace migrations of peoples in all parts of the world in recent decades; that disclose the areas in which disease constitutes a threat to health in distant lands—and many other types of maps, including some "maps" on curved surfaces (part-globes) for special purposes.

Resources of governments and of well-supported institutions are needed to underwrite the vast amount of research required in compiling many of the maps that ought to be made. Coordinated programs of map

production are essential if different series of maps are to be readily comparable, and if wall maps, atlas maps, lantern-slide maps, and animated motion-picture maps are to supplement one another. Important technical advances in the science and the art of cartography are desirable and possible. Very significant work will of course be done by individual geographers and cartographers not employed by governments or large institutions. Their contributions will be greater if they associate themselves with economists, historians, demographers, sociologists, political scientists, engineers, and other specialists of many nationalities.

People can seldom be hypnotized against their will. Cartohypnosis can be eliminated as a threat to sane and wholesome development of the world in the interests of its human inhabitants if people look at maps critically and honestly and demand an abundant supply of accurate maps to show them what are the geographical relationships between peoples and their activities.

GENESIS OF WORDS

In restlessness of vague, chaotic thought
In which the self first knows self-consciousness,
Where hope of ecstasy depends on hope
Of words within a wordless wilderness,

For what a satisfaction undefined,
In what despair of hunger unexpressed,
The self, as in a claustrophobic dream,
Cries wordlessly and would resume its quest

For some fulfillment of its vague desire!
What paradox with every hope denied,
That words cannot exist alone for want
Of thought which, waiting for the words, has died!

Here has the groping search of man begun;
Here mark the hope for structure and design—
Hope for the social heritage preserved,
The wise man's book, the poet's singing line.

Here mark the hope for science yet to find
An evidence of self-sufficient cause
And for philosophy to yet record
Some final harmony of cosmic laws.

DOUGLAS E. LAWSON

THE FUTURE OF THE SCIENCE OF ANIMAL BREEDING

By SYDNEY A. ASDELL

Laboratory of Animal Nutrition, Cornell University

MANKIND has one thing in common, an interest in food, how to get more of it of better quality and cheaper. In recent years we have heard much about balanced diets, how to get the best out of the food we have, and of the transport and distribution of food. But we have heard little about the producer's end of it: what science is doing for him and how he looks toward the future. This is especially so in the field of animal breeding and production. To most of us, the domestic animal is like Topsy, it just growed. But there is more to it than that.

Let us take the milch cow as an example. How many of us realize that the average milk yield of a cow in the United States is about 5,200 pounds a year, or considerably less than that of the world's champion goat? What does that mean to the farmer and to us? It means a lot. For the first two years of her life the cow is growing—costing money and returning nothing. It does not cost much more to grow a good cow than it does to grow a poor one, and the cost of maintenance and management is also very little more. Obviously, milk will be cheaper if this overhead is spread over a lot of milk instead of over a little, and the farmer will get a better living and have less work to do because he will have fewer cows to look after. The problem, then, consists in breeding better cows. What is being done to achieve this object? Quite a lot!

A good deal of work is being done to find out the best systems of breeding which, if followed, are likely to result in the best cows. Pedigrees of record-breaking cows have been analyzed to see whether any consistent thread appears in the type of breeding. As a matter of fact, there is no consistent thread. Any system of breeding, except one, produces good results provided the parents are good. The exception is close inbreeding, which is definitely detrimental. Some of the best cows have come from lines in which the degree of inbreeding has been reduced. Similar work with swine, following the work with corn in which many of the best commercial strains are the results of crosses between two inbred lines, has shown that excellent pigs can be bred by crossing inbred lines, but—and this is most important—the inbred lines have to be very good themselves to produce good results when they are crossed. In other words, we have to find and use to the greatest possible extent the best breeding animals. It is easy enough to judge a good cow by what she yields, but it is not so easy to judge the bull because he only expresses his worth in his progeny.

The question narrows itself down, then, to this: how to find the good bulls and how to make the most use of them when they are found. So far, the only way to find the best bulls is to breed a lot of them and determine the milk yield of their daughters. If the daughters are good, the bull is good. By the time we know all this the bull is getting on in years and may not be useful for many years longer. So we are looking for ways to prolong his usefulness and, by better management, to prevent him from becoming vicious in his old age. So far, psychologists have not been called in to help in the latter problem, but I believe they might make a real contribution if they would tackle it. More than two hundred men on American farms are killed each year by bulls.

Increased use of the good bulls when they are discovered is being made by the new

techniques of artificial insemination. By this means, instead of siring 60 calves a year, a bull may sire 500 or more. This gives a much greater sphere of usefulness for the proven sires, and it also speeds up the testing and sorting out of the younger ones. These techniques have passed the experimental stage and are now in practical use. More than 10,000 cows were artificially bred in New York State alone last year, and the number is increasing very rapidly. The introduction of these methods raises a crop of new problems, not the least of which are to keep the bulls fit under this regime and to spot and correct rapid fluctuations in their efficiency.

We are learning how to make more use of the good bulls when we have found them. Can we do the same for the cows? Experiments are in progress to determine whether it is possible by the use of the hormones of the anterior pituitary gland, ovarian stimulants, to cause a cow to shed more eggs than usual and to produce twins and triplets as a rule. This has been done in England under the stress of war necessity, but it is complicated by the fact that a female twin so produced is nearly always a freemartin. Can we transfer the eggs of good cows to less valuable cows so that the latter become internal foster mothers of the calves of the former?

Along somewhat similar lines efforts are being made to induce breeding in sheep, goats, and fur animals outside the normal breeding season and thus to speed up the reproductive processes. Two possibilities suggest themselves: the injection of ovary-stimulating hormones; and the physiological conditioning of animals by modifying the amount of light to which they are exposed so that they live, in effect, in a perpetual spring or fall, whichever their normal breeding season may be.

Besides this work, all directed toward more efficient breeding, the physiologist has not been idle in attempts to increase the milk yield of the individual cow beyond its inherited level. This involves a study of the factors which cause mammary development and secretion, and much progress has been made along these lines. Hormones have been isolated which do what we want, but, so far, they are too expensive for general use and they often do things to other organs which are not so desirable. A cheap synthetic drug, stilbestrol, has proved very interesting as it can be fed instead of injected and it can cause a large increase in milk yield or bring a cow into milk without her calving. But stilbestrol damages the reproductive function rather badly. It does not, however, do this to the goat, which suggests that we may be able to find another synthetic drug that will be as effective in the cow as stilbestrol, but without its undesirable qualities. The hunt is on for such a substance, but we may be up against an inherent peculiarity of the cow.

Another method of approach to the same problem is to find something that will raise the general metabolism of the animal, including that of the mammary gland. Thyroxin is such a substance, and it is the one provided by the animal for its own use. Again, it is expensive, but a cheap substitute has been found and is being used experimentally. One wonders, however, whether the cow will be able to stand up to the strain of an increased rate of living.

THIS last question is very pertinent, and it brings us to another problem that is engaging the attention of the physiologist, namely, the length of productive life. It is unfortunately all too true that the average life of our cows is only a little over six years, of which two are used in growing up. Thus, the cow is a milk producer for only four years instead of for ten, which should be her effective life. Again, the overhead is spread over a short period and is, therefore, unduly great. Also, the short life means that most of the heifer calves born have to be drafted

into our herds as replacements, and we cannot do the culling of poor producers that we should be able to do. Some recent work with smaller animals has suggested that by causing them to grow more slowly their length of life may be increased, but we do not yet know whether this method increases the length of productive life, which is the important point. That is the next thing that will have to be determined. On the other hand, there is good reason to believe that slow growth, or perhaps I should say unbalanced growth, injures the reproductive function, and sterility is the cause of huge losses to our farmers. Perhaps we shall have to strike a balance; we may be trying to combine incompatibles.

Sterility in animals is something that has always troubled us and probably always will to some extent. Recent research in this field has suggested that not much can be done to cure it. When a cow is sterile, or nearly so, the damage has been done and it is too late for really effective treatment in a majority of cases. It seems that we shall have to study more closely the effects of different methods of raising calves and of managing them so as to get the best results. Also, there may be a field for the prevention of the diseases that lead to sterility. We may hope that in the future many of these diseases will be wiped out by the development of vaccines. The field is an interesting one, almost untouched, and much work has to be done in it. Naturally, results obtained here may have applications in the human field and vice versa, and so the animal and human physiologists have a common ground and can learn much from each other.

To return once more to the important problems of growth. A rate of growth that is best for prolonging life and productive and reproductive efficiency may not be best for the production of meat. There we want to bring the animals to market condition as rapidly as possible so that the least amount of feed is used up in body maintenance, unprofitable to the producer and consumer alike. Also, we want to make the carcasses such that there is least wastage in slaughter and dressing, and we want the proportions of lean, fat, and bone to be those best suited to the varied market requirements. That means a study of consumer preferences, and in the past it has meant changes in breeding methods to produce the carcass best suited to the market. This the producer has been reluctant to do because to modify an animal by breeding takes many generations of selection. By the time the desired result has been attained, consumer preferences may have again shifted, and the work has gone for naught. Are there short cuts by which we can gain the desired results in a generation? This involves an analysis of the mechanism of growth. When does an animal grow most bone, when most muscle, and when most fat? Can we modify the order of growth in different cuts so that the most profitable cuts grow more in proportion to the less profitable ones? A partial answer to this question has shown that by underfeeding or overfeeding pigs at certain periods of growth the carcass quality can be modified. But the results have been obtained by careful hand feeding where everything is under control. Can we apply such methods so that they can be used practically in nass feeding?

Another approach to the problem is by modifying the activity of the glands regulating growth. This has been done from time immemorial by castration. Can we find other, similar, methods using the experience that has been obtained by the doctor and by the dog breeder? One approach that is being tried is to depress the activity of the thyroid gland, thus making the animal less active, so that he can use more of his food for growth and fattening. By the use of thiouracil for this purpose the fattening of hogs can be brought about more readily, and quite remarkable hams can be grown on small pigs. But the balance is delicate, and

we do not yet know how to adjust the dose and the time of its application to produce these hams without unduly stunting the pigs. How far can we go along these lines?

Another field that is being developed is that of prediction. We already know that, disease aside, a female that comes in heat early and that reproduces readily and frequently at first will usually do better over her lifetime than a poor initial reproducer. How far can we extend ideas of this sort? Can we find early signs of a good milker sufficiently reliable for practical use? The search is on for such telltale signs.

Livestock which flourish in our temperate regions usually deteriorate in tropical regions, whereas the tropics have breeds able to stand the conditions to which they are exposed but with too low productivity to make much contribution to the feeding of tropical peoples. One answer to this question is to modify the two strains by crossbreeding with selection. This is slow and uncertain. What are the constitutional factors necessary to produce a good tropical animal? Temperature control is one of these; disease resistance is another. This avenue is being explored to see what modifications are needed and, eventually, how they may be produced. The answers will probably be partly physiological, partly genetic, and partly a matter of engineering to produce the best conditions for heat loss. Another approach is to compare the feeding stuffs of the two regions, to find out which ones are suitable for different conditions. Should levels of protein, carbohydrate, etc., be modified to produce the best conditions for production?

The physiologist is sometimes asked what he can find to do in the field of animal husbandry. We believe that the problems are many, as we have shown in this article, and that by teaming up with the biochemist, the anatomist, and the embryologist the animal physiologist is making his contribution, and no mean one, to the betterment of animal breeding and the material progress of the world. This applied science is a new one, and, as in all such sciences, much exploratory work has to be done; explanations have to be sought of known facts so that vital processes may be understood and brought under purposeful control.

THE PROBLEM OF PLAN AND PURPOSE IN NATURE*

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"OF SURPASSING interest to those many minds, which seek after philosophic knowledge and instruction, is the Story of the Earth, Her manifold living creatures, the human generations, and Her ancient rocks." With these words Doughty introduced the second edition of his great book *Travels in Arabia Deserta*. They might serve as a preface to all human learning and especially to that most important of the branches of learning, the study of life. "The human generations," in all their aspects, must be the constant concern of any thoughtful man, whether he aims at "philosophic knowledge" only to enrich his own life or whether he aims at "instruction" to guide him in his functions as a member of society.

In degree, man's social and intellectual complexity is something new under the sun, but man remains a part of nature and is subject still to all of nature's laws. Man is only one of earth's "manifold living creatures," and he cannot understand his own nature or seek wisely to guide his destiny without taking account of the whole pattern of life.

We feel, almost instinctively, that there is a pattern. The diversity of living creatures is neither complete nor random. All living things share many characteristics, and above this basic level we observe groups with every degree of resemblance, from near identity to great dissimilarity. There is, or seems to be, an essential order or plan among the forms of life in spite of their great multiplicity. There seems, moreover, to be purpose in this plan. The resemblances and differences between a fish, a bird, and a man are meaningful. The resemblances adapt them to those conditions and functions that all have in common and the differences to peculiarities in their ways of life not shared with the others. It is a habit of speech and thought to say that fishes have gills in order to breathe water, that birds have wings in order to fly, and that men have brains in order to think.

A telescope, a telephone, or a typewriter is a complex mechanism serving a particular function. Obviously, its manufacturer had a purpose in mind, and the machine was designed and built in order to serve that purpose. An eye, an ear, or a hand is also a complex mechanism serving a particular function. It, too, looks as if it had been made for a purpose. This appearance of purposefulness is pervading in nature, in the general structure of animals and plants, in the mechanisms of their various organs, and in the give and take of their relationships with each other. Accounting for this apparent purposefulness is a basic problem for any system of philosophy or of science.

Attempts to solve this problem are perhaps as old as man. There are few savages so dull or so primitive that they do not have some legend or belief bearing on the problem, even though they do not formulate it clearly or in these terms and even though their solutions are more implicit than explicit. Certainly the problem appears repeatedly, in more or less sophisticated form, among the Greek philosophies, in the Scriptures of all the great religions, and in other ancient attempts to grapple with the nature of the universe and of

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man. In scientific history, the need for a solution became particularly clear and conscious in the intellectual crisis of the latter part of the eighteenth and early part of the nineteenth centuries.

One possible solution, which had by then become traditional in Western thought and religion, was elaborately presented in the *Bridgewater Treatises*, published in 1833–40. These treatises, of which eight were issued, were richly endowed under the will of the eighth Earl of Bridgewater, who directed that they set forth

> . . . the Power, Wisdom, and Goodness of God as manifested in the Creation; illustrating such work by all reasonable arguments, as for instance the variety and formation of God's creatures . . . the construction of the hand of man, and an infinite variety of other arguments. . . .

They were of unequal merit, but several of them can still be read with pleasure and, indeed, profit. The best of them were, in their day, able works of science, and they strongly influenced scientific thought. (It is noteworthy that although the *Bridgewater Treatises* were, by stipulation, anti-evolutionary, a quotation from one of them stands at the beginning of Darwin's *Origin of Species*.)

The work on the hand, particularly mentioned in Bridgewater's instructions, was written by Sir Charles Bell under the title "The hand, its mechanism and vital endowments as evincing design." Bell did not confine himself to the human hand, but gave a generally excellent account of the comparative anatomy of the vertebrate forefoot and, indeed, of handlike appendages throughout the animal kingdom. He stressed the perfection with which each type of forefoot is adapted to the particular needs and habits of its owner and he pointed out that the intricate mechanism of the human hand follows a seemingly perfect and (as it appeared) obviously purposeful design.

> There is an adaptation, an established and universal relation between the instincts, organization, and instruments of animals on the one hand, and the element in which they are to live, the position which they hold, and their means of obtaining food on the other.

After considering and rejecting the theory of evolution, which was, of course, already well known at that pre-Darwinian time, Bell concluded that

> It must now be apparent that nothing less than the Power, which originally created, is equal to the effecting of those changes on animals, which are to adapt them to their conditions: that their organization is predetermined, and not consequent on the condition of the earth or the surrounding elements.

The fact of adaptation was thoroughly established by such works as Bell's. The tremendous increase in knowledge of nature since 1833 has, on this point, served only to demonstrate that adaptation is even more widespread and may be even more elaborate than Bell knew. To this extent, his arguments are just as cogent now as when he wrote them. But now that we know that evolution is a fact, we can no longer accept his simple solution of the problem of adaptation as reflecting the purpose of a Creator manifested in the separate creation of each species of animal or plant. Whether or not we can explain the evolution of adaptation has no necessary bearing on the truth of evolution. The proofs that have now accumulated, quite aside from attempted explanations of adaptation, are fully sufficient. Competent modern biologists may differ diametrically as to the meaning or mechanism of adaptations and yet all agree that these did, somehow, arise by evolution. If, however, we hope—as of course we do—to go beyond merely observing the course of evolution and to gain some insight into how and why nature has followed this course, then we must still account for the appearance of purposefulness in this history.

In Bell's day, before the proofs of evolution had accumulated sufficiently to carry full conviction in themselves, his argument could only be met by demonstrating the possibility, at least, of an evolutionary explanation of adaptation. Even now we are, quite properly, I believe, reluctant to accept the reality of a phenomenon, be it evolution or extrasensory perception, unless there is some plausible hypothesis as to its mechanism. It is, then, not surprising to find that both Lamarck and Darwin, whose ideas in more or less profoundly modified form have dominated most of the subsequent discussion of evolutionary theory, were at least as much concerned with finding an evolutionary mechanism for adaptation as they were with demonstrating the truth of evolution.

Lamarck's views have been so obscured by the often quite different theories called neo-Lamarckian, by straw men set up by the anti-Lamarckians, and by mere carelessness and forgetfulness, that it is useful to restate his theory. His best expression of it is found in a work published in the year of Darwin's birth, 1809, and grandly titled (in French) *Zoological philosophy, or an exposition of considerations bearing on the natural history of animals, on the diversity of their organization and the faculties that they derive from it, on the physical causes that maintain life in them and that give rise to the movements executed by them, and finally, on causes some of which produce feeling and others intelligence, in the case of those endowed with the latter.*[1]

Lamarck's whole theory embraced five main points, which may be expressed as follows in his own words (translated and rearranged):

1. Nature, in successively producing all the species of animals, commencing with the most imperfect or the most simple and ending her work with the most perfect, has gradually complicated their organization.
2. If the cause that tends constantly to complicate organization were the only one that influenced the forms and organs of animals, the increasing complication would be, in its sequence, very regular throughout But this is not the case. Nature is forced to submit her works to the influences of the environment, which acts on them, and on all sides the environment causes variations in the products of nature.
3. Whatever the environment may be, it does not directly bring about any modification whatsoever in the form or organization of animals. But great changes in the environment lead to great changes in the needs of animals, and such changes in their needs necessarily lead to changes in their actions. Now, if the new needs become constant or very long continued, the animals acquire new habits, which are as lasting as the needs that gave rise to them.
4. *First law:* In any animal that has not passed the limit of its evolution, the more frequent and continued use of any organ strengthens this organ gradually, develops it, enlarges it, and gives it a power proportionate to the duration of this use, whereas the constant lack of use of such an organ gradually weakens it, deteriorates it, progressively diminishes its capacities, and finally makes it disappear.
5. *Second law:* Everything that nature has caused individuals to gain or lose through the influence of the environment to which their race has long been exposed and, consequently, through the influence of the predominant use of an organ or that of constant lack of use of a part, nature keeps all this by heredity in the new individuals that are born, provided that the acquired modifications are common to the two sexes, or to the progenitors of the new individuals.

[1] The French title is: *Philosophie zoologique ou exposition des considérations relatives a l'histoire naturelle des animaux, a la diversité de leur organisation et des facultés qu'ils en obtiennent; aux causes physiques qui maintiennent en eux la vie et donnent lieu aux mouvements qu'ils exécutent; enfin, a celles qui produisent les unes le sentiment, les autres l'intelligence de ceux qui en sont doués.* The English translation by Hugh Elliot is titled: *Zoological philosophy. An exposition with regard to the natural history of animals, the diversity of their organisation and the faculties which they derive from it; the physical causes which maintain life within them and give rise to their various movements; lastly, those which produce feeling and intelligence in some among them.*

Lamarck's own labeling of the last two postulates as "first law" and "second law" has helped to obscure the fact that these did not constitute the most essential part of his theory but on the contrary were introduced to explain what he considered irregularities or perturbations in the normal course of evolution. In later terminology, Lamarck believed in an orthogenetic progression throughout the whole animal kingdom, tending in a single series from the most simple to the most complex (specified as man), and he believed that special or local adaptations were disturbances in this progression caused by environmental changes and consequent changes in use and disuse of organs, the acquired effects of which are inherited.

Most neo-Lamarckians have rejected or overlooked the broader part of Lamarck's own theory and have supported a general theory of evolution based on the two "laws" that, by Lamarck, were meant only to explain exceptions to the normal course of evolution. Many neo-Lamarckians have also postulated direct effects of the environment on the organism, which were categorically denied by Lamarck. Although I have summarized Lamarck's whole theory here, in an effort to restore historical accuracy, it is mainly his first and second laws that are now of interest to us, also.

Without using that term, Lamarck accepted the purposefulness of adaptation, but he reduced this to evolutionary and to (at least superficially) mechanistic terms by equating the purpose with the needs of individual animals and their efforts to supply these needs. The giraffe has become a stock example, so used by Lamarck himself, and later by Darwin for his different explanation. The "purpose" of a giraffe's long neck is to reach higher leaves and twigs on trees for food. According to Lamarck, the ancestral giraffes stretched their necks in efforts to reach this food—purposeful behavior by the animals. In time, by long repetition, this elongated their necks, the elongation was passed on to their offspring, and this process continued for many generations until it culminated in the long necks of the present giraffes.

This theory of adaptation is ingenious and elegant. Its very simplicity has great appeal, and there is in it something of aesthetic satisfaction, a sort of poetry of evolution. Although it had little influence when first promulgated by Lamarck, later, when evolution had become generally accepted, it was widely discussed and commonly espoused, although it probably never represented a majority view on adaptation. It had a particular attraction for the paleontologists of the latter half of the nineteenth century, and still colors paleontological thought to some extent. Paleontologists found that adaptation was a key to many of their observations and that it was as widespread in the past as in the present. They also found many examples of the gradual, progressive perfection of adaptations that seemed beautifully explicable in Lamarckian terms and they had difficulty in explaining some of these phenomena by the Darwinism of their days.

It is a pity that this thoroughly charming theory is not true, but there can now be no serious doubt as to its falsity. It still has one or two able and eminent supporters—an idea so appealing is not likely to be wholly discarded whatever logic and experience may show—but their battle is clearly lost. The theory indispensably demands that the effects of use and disuse, or other acquired characters, be inherited and that they be inherited in kind and become germinally fixed. That is, Lamarckism requires not merely that modifications acquired during the lifetime of parents should affect their offspring but also that they should affect the same parts of the offspring in the same way as in the parents

and that they should, sooner or later, become a permanent part of heredity in the line of descent, regardless of further or repeated modifying factors. It has been proved, as nearly as a negative can ever be proved, that this does not and cannot occur. Even aside from this fatal point, the evidence against Lamarckism is convincing. There are adaptations, such as those of neuter female insects and many cases of protective coloration or mimicry, for which a Lamarckian explanation is practically inconceivable. The adaptations, like the giraffe's neck, for which Lamarckism does ostensibly provide a mechanism are readily explicable by other processes which, unlike Lamarckism, have been experimentally verified.

I HAVE digressed briefly to show that we cannot now adopt the Lamarckian explanation of apparent purpose in nature, much as we might like to do so. Historically, the greatest vogue of Lamarckism was in the future when Darwin advanced an alternative theory. *The origin of species by means of natural selection; or, the preservation of favored races in the struggle for life*, to give Darwin's greatest work its full Victorian title, was published in 1859, just a half-century after Lamarck's *Philosophie zoologique.*

On different levels, *The Origin of Species*, which is surely one of the most important works ever produced by the mind of man, accomplished three different things. In the first place, it presented the case for evolution, as a general principle in the history of life, and did this so convincingly that it was the most influential single factor in the eventual general acceptance of this principle. Darwin foresaw this result and worked for it, but he knew that the theory of evolution was far from new, and, as its title indicates, this was not, in itself, the primary aim of his book.

The Origin of Species also provided a broad but incomplete explanation of the way in which evolution works. In brief paraphrase, this explanation is as follows: Organisms vary. Some (at least) of these variations are hereditary. Within a given group, some individuals have more descendants than others. The hereditary variations of these individuals are therefore more common in succeeding generations, which tend to evolve in the direction of these variations. It is not clear that Darwin himself definitely separated this theory from the subsidiary theory of natural selection, which is dependent on it. The more general theory is separable, however, and it has been widely accepted as a part of the evolutionary mechanism, even by those opposed to natural selection. Although this particular mechanism certainly is not operative in all cases of evolutionary change, it is just as certainly one of the most widespread mechanisms.

Finally, *The Origin of Species* advanced a theory to account for adaptation in nature, and particularly progressive adaptation. This was the main subject of the book, and Darwin considered it his major contribution, although he recognized that even this was not completely original. The theory is, of course, that of natural selection. The organisms likely to have more descendants are those whose variations are most advantageous as adaptations to their way of life and to their particular environment. Thus evolution is likely to move in the direction of greater or more nearly perfect adaptation, and thus the fact of adaptation, purposeful in aspect but impersonally mechanistic in origin, is explained.

Darwin did not believe that natural selection is a complete explanation of evolution, or even of adaptation. Some students have made much of the fact that he did not exclude the possibility of the inheritance of the effects of use and disuse, in Lamarckian fashion. This point should

not, however, be much emphasized. Darwin was wise enough to see that parts of the evolutionary process were still mysterious, and cautious enough not to rule out completely any supplementary factor that could not then be disproved. He ascribed only an indefinite and very subsidiary role to the effects of use and disuse and he showed that some supposed examples (like that of the giraffe's neck) could better be explained by natural selection.

As Darwin foresaw, his work was followed by the rejection of special creation as a scientific theory. That special creation remains, in some circles, a theological tenet is beside the point here. It is also beside the point, but should perhaps be mentioned in passing, that later scriptural exegesis found no difficulty in accepting evolution, rather than special creation, as God's method of creation and that there need be no conflict between rational evolutionists and rational theologians. After *The Origin of Species*, independent proofs of evolution piled up at an increasing rate, and, equally important, Darwin had demonstrated, more successfully than Lamarck, the possibility of an evolutionary answer to the theological argument of purpose in nature.

The Darwinian theory of adaptation was, however, soon under fire from other convinced evolutionists. One of their main lines of attack was that natural selection, as advanced by Darwin and his successors, is a purely negative process. It destroys but does not create. It eliminates disadvantageous variations but tells us nothing about the origin of advantageous variations and therefore does not, after all, explain how adaptation arises. This merely indicated that natural selection is not the whole story of evolution, which Darwin never claimed it was. It might not have been a very important criticism of Darwinism as Darwin understood it, but some of the neo-Darwinians, toward the turn of the century, did so overemphasize natural selection that this objection did tend to discredit the whole Darwinian school. Aside from this development, the criticism is crucial to our present inquiry because the problem of purpose does not arise from the elimination of the unfit but from the origin of the fit, and it was claimed that the Darwinian explanation of the latter point was discredited.

It was also maintained that characters destined to be adaptive are often of no real use when they first appear, consequently are not then favored by selection, and nevertheless do persist and become useful adaptations. Darwin foresaw this objection to his theory and attempted to answer it, but without complete success. A related objection is the claim that many striking and complex characters of animals have no selective value at any time and so cannot be favored by selection. This, again, would merely mean that selection is not the only influence in evolution, and it has no special bearing on adaptation, as such characters are supposed to be neither adaptive nor inadaptive but merely nonadaptive.

Neither of these latter objections proved to be very serious in the light of increased knowledge of the nature and extent of adaptive advantage, but there was another objection, particularly advanced by paleontologists, that proved to be very serious indeed. After studying numerous lineages among fossils, many paleontologists concluded that evolution tends to proceed in straight lines, that the evolutionary direction of these lines is unswerving even when it has no (evident) adaptive advantage, that it may even run counter to adaptation, and that a trend initially adaptive may continue to such a point that it becomes inadaptive and perhaps fatally disadvantageous.

It was objections of this sort that led many students to believe that attempts

at mechanistic interpretations of evolution, and particularly of adaptation, had failed and were foredoomed to such failure. If characters arise regardless of adaptive significance but nevertheless do, for the most part, come to fill a need, if they do not arise at random but in regular progression, or if they progress steadily in straight lines as if toward a goal—then, it was felt, there must be some guiding force controlling evolution, quite possibly a purposeful force, but this force cannot be found in the environment or in the physical laws of nature. It must either be a force peculiar to life and to this extent non-physical or it must reside on some super-, or at least extranatural, plane. Such a force was repeatedly postulated by students disillusioned with both Darwinism and Lamarckism. Driesch called it "entelechy;" Bergson called it the *élan vital;* Osborn called it "aristogenesis" (among other things); Teilhard calls it "noogenesis." Others call it other names, and some are content to postulate such a force without naming it.

It would certainly be a mistake merely to dismiss these views with a smile or to ridicule them. Their proponents were (and are) profound and able students. Yet in essence what they are all saying is little more than "The cause of evolution is the force that causes evolution." Attempts to define this force usually amount to no more than a description of the way these students suppose evolution to occur, which really leads to no comprehension of how or why it occurs in this way—if, indeed, it does, which is also a decidedly debatable point. As Julian Huxley has remarked, ascribing evolution to an *élan vital* is like "explaining" the movement of a train by an *élan locomotif*. It may be added that "definitions" of these various names for the supposed nonmechanistic causes of evolution generally sound like defining the *élan locomotif* as "that force which causes a train to travel undeviatingly from one station to its goal in another station."

A word must be said, also, on those theories of this general class that go to the extreme of assigning a supernatural cause to evolution, not in the sense of divine establishment of the natural laws of evolution, a belief held by many evolutionists who nevertheless seek to discover these mechanistic laws, but in the sense that the laws themselves are supernatural. Among these views, that of Broom is extreme and yet representative of a trend of thought usually less elaborated and less clearly or frankly expressed. Broom maintains that there is "some spiritual power which has planned and directed evolution" and that below this there are other spiritual agencies, some good and some evil, which in turn direct "partly intelligent" inferior spiritual agencies directly associated with the various animals and plants.

If any of these views are true, we cannot hope to explain "the eternal problem of adaptation," as Osborn called it. They postulate that the purposive element in evolution involves some force that is quite outside the sphere of scientific investigation. We can see what it does but we cannot and need not determine how or why it does this. In fact, these ideas are metaphysical and not scientific, even though this is often denied by their supporters. One cannot but suspect conscious humor when Teilhard (a trained theologian, as it happens, in addition to being one of the most able of paleontologists) states in the midst of one of the most subtly metaphysical discussions of evolution known to me, "In this, note well, nothing metaphysical."

Of course an explanation might be metaphysical and nevertheless be true. It is, however, an obvious lesson from the history of scientific progress that in science one should never accept a metaphysical explanation if a physical explanation is

possible or, indeed, conceivable. In some cases these theories were clearly born of despair and faintness in the search, an emotional state with which we must sympathize but which we should surely seek to avoid in ourselves. Wallace, who independently from Darwin arrived at the principle of natural selection, finally became disillusioned, along with most of his contemporaries, as to the ability of this principle to provide an adequate explanation of evolution. He concluded that "a superior intelligence" must also be involved. Commenting on this after Wallace's death, Osborn wrote of "transformations which become more and more mysterious the more we study them, although we may not join with this master in his appeal to an organizing and directing supernatural principle." Yet Osborn himself, in the course of the twenty-two years that then remained of his long and fruitful life, came to expound evolution as the result of an organizing and directing principle which he refused to call supernatural but for which he found no naturalistic basis or explanation.

Rise of modern knowledge of heredity in the early years of this century brought with it an attempt to dispose of this troublesome, eternal problem by abolishing it, an astonishing solution as seen in retrospect and yet one that held sway among the most advanced biological theorists until quite recently. It was found that new characters and new types of plants and animals can arise quite suddenly by some change within the hereditary substance in the germ cells. As far as is known, these changes, mutations of various sorts, arise quite at random. The geneticists, fervently exploring on the frontier of this newest offspring among the biological sciences, proposed on this basis to do away with all the musty problems of purpose or adaptation by simply denying their existence. New types of plants and animals arise at random, and that is all there is to it. No adaptation and, of course, no purpose.

The geneticists quickly took a great step forward in the attack on one very essential phase of the problem of adaptation, the question of the nature and origin of hereditary variations. Lamarck faced this question more squarely than almost any later student of the nineteenth century, and he proposed an answer, but his answer was wrong, as the geneticists, among others, have clearly shown. On the whole, Darwin avoided the question. It is doubtful whether even a partial answer was practicable in his day, and to this extent Darwin's study of adaptation was premature, but his evasion or glossing over of the question was a serious defect in his theory. It led to some confusion among his followers and to a reaction against this concept during the next half-century. The vitalists met the question in a still less satisfactory way, by relegating it to the realm of the unknowable or by self-deceiving naming fallacies.

The geneticists have observed and isolated nascent variations (mutations of various sorts), and they have found out the more essential facts about how these are passed on, combined or separated, and expressed in later generations. Beyond these processes there are other, deeper, mysteries into which the geneticists have barely penetrated, if at all—all steps toward greater knowledge bring us newly to the threshold of more profound mystery. But modern geneticists have supplied what seems to have been the last lacking basic information necessary for an explanation of evolution essentially complete on, at least, the descriptive level. Their own attempts to apply this information to evolutionary theory were not, at first, fruitful, although we shall see that they later became so. In earlier years of the present century most geneticists worked in (mainly self-imposed) isolation from the paleontologists, systematists, comparative anatomists, and others

who had accumulated a vast body of data on the course and on the results of evolution. As is so likely to happen when great new discoveries are made, some of these geneticists were overenthusiastically sure that their discovery quite superseded all that had gone before and was the sole and whole key to evolution. In no other way can one understand their proposed solution of the problem of adaptation by saying that there is no such thing as adaptation.

Adaptation does exist and so does purpose in nature, if we define "purpose" as the opposite of randomness, as a causal and not a merely accidental relationship between structure and function, without necessarily invoking a conscious purposeful agency. Denial of this does violence to the most elementary principles of rational thought. Look again, with Sir Charles Bell, at your own hand, manipulate the fingers, think of the intricacy of the combination of bones, muscles, tendons, blood vessels, and nerves in this mechanism and of the complexity and delicacy of its conscious control. How can one estimate the improbability that such a structure arose by sheer accident, or by any continued series of accidents short of infinity? The example is simple but it is convincing in its very simplicity. Consider, then, all the other structures and organs of man and of all the other animals and plants and the way in which these function and serve the many special needs of each type of organism. Consider, too, the interrelationship of these organisms, the dependence of animals on plant photosynthesis, the social groups of ants, the fertilization of flowers by insects, and the myriad other adaptive ecological relationships. Whatever improbability you assigned to the random origin of your hand, this might be multiplied by a billion billion to express the improbability that nature as a whole is the result of any sequence of accidental and random events. It is necessary to agree with Julian Huxley that "to produce such adapted types by chance recombination . . . would require a total assemblage of organisms that would more than fill the universe, and overrun astronomical time."

Although some geneticists really were, for a time, so naive as simply to deny the reality of the problem of adaptation, it would be unfair to imply that this opinion became universal or endured long among them. Acceptance of random mutation as the mechanism of evolutionary change and of the fact that adaptation—or something that looks singularly like it—is well-nigh universal in nature led to emphasis of another evolutionary principle, that of preadaptation. The essence of this principle is that structural or physiological peculiarities, although arising at random, may prove to be suitable or useful in the environment of the organism or in some other environment available to it. For instance, animals living in fresh water may by mutation become tolerant of salty water and then spread into brackish streams or into the sea because they now can; or animals living on a forest floor may undergo mutations enabling them to climb and may then take to the trees for refuge. This is a sort of reversal of the old idea of apparently or really purposeful adaptation in, for instance, the Lamarckian sense. The "adaptation" (it should probably be in quotes when used in this way) comes first and its use follows. According to an extreme version of this theory, all strongly distinctive types of animals originated as sports, or "hopeful monsters," as Gold schmidt calls them, that happened to find a practicable way of life adapted to their peculiarities, rather than originating by any process that adapted them to peculiarities of the environment.

This is an interesting and important theory. There is little doubt that preadaptation does really occur, although rarely if ever in the extreme form supported by Goldschmidt. We shall see that preadaptation, with some expansion and modification of its

significance, must be accepted as one of the general factors in the new synthetic theory of evolution. Earlier opinions that random preadaptation is an adequate explanation of adaptation were, however, quite unjustified. Objections already expressed to accepting any explanation of adaptation as a result solely of random, accidental, or unsystematic processes apply with equal force to preadaptation in this relatively crude form and without any additional postulate involving a systematic guiding influence and providing for pseudo- (if not really) purposeful progressive increase in adaptation.

At this stage in the inquiry, the situation seems almost hopelessly confused. This was the position only a few years ago, with many special theories ardently supported, although to a dispassionate observer none would have seemed adequate and some would have seemed absurd. Some students, a very small minority, denied the existence of adaptation. A host of other students had, however, demonstrated really beyond any doubt that adaptation does exist and that a great majority of the characters of animals and plants, although not all their characters, must be considered as definitely related to, and requisite for, their particular ways of life. For some this was merely an inexplicable fact; these students were few, because scientists rarely are psychologically capable of accepting a phenomenon as a fact and also accepting it as inexplicable. It is well for the progress of science that this is so, but it has led to many premature and to some bizarre theories.

Die-hard neo-Lamarckians still maintained that adaptation is explicable as the result of the effects of the environment on the organism and of the organism's own efforts to cope with the environment. Die-hard neo-Darwinians still maintained that the removal of misfits by natural selection is the whole story. The latest comers, the geneticists, were in many if not in most cases convinced that the only adaptation is preadaptation. Besides these three main mechanistic schools, there were many who supported a wide variety of covertly or overtly nonmechanistic views. In part they were reacting in a natural way against the excesses and deficiencies of each of the mechanistic schools. Rationally unable to accept one or the other of these, they were unwilling to leave adaptation unexplained and sought refuge in nonscientific or metascientific theories. In other cases they were influenced by, or returning to, the pre-evolutionary and long antievolutionary concept of adaptation as literally purposeful but, as a rule, they dodged the question of who or what formulated the purpose.

The story of the elephant and the blind men, who argued bitterly over the nature of the beast only part of which was known to each of them, is old and trite. It has become trite because it is so often apt, and it is very apt here. Each of the diverse schools of thought about adaptation knew or emphasized only part of the pattern. Each tried to complete this pattern as a reconstruction from a part only and each bitterly rejected the reconstructions of the other schools, based on other parts. The neo-Lamarckians knew and overemphasized the fact that adaptation is pervasive in nature and essentially purposeful in aspect, as if the environment had forced and the organism had sought adaptation. The neo-Darwinians knew and overemphasized the fact that the more or less adaptive status of variations is influential in determining the parentage of a following generation. The geneticists knew and overemphasized the fact that new hereditary variants arise abruptly and, as far as we know and as far as adaptive status is concerned, at random. The various non- and antimechanists knew and overemphasized the fact that adaptation is usually an essentially directional, progressive, sustained, and nonrandom process.

Once the search has been summarized in this way and its outcome expressed in these words, it becomes obvious how to seek further for an answer to the problem of plan and purpose in nature, or of adaptation, to which the problem is reduced by elimination of some of its metaphysical overtones. What was necessary was synthesis, bringing together the facts and theories of all the schools, accepting those mutually consistent and reciprocally reinforcing, rejecting the inconsistent, inadequately supported, and unnecessary, and building anew on this basis. Such a development, obvious as it now seems, would have been premature until rather recently because essential elements, especially some of those lately supplied by the geneticists, were still lacking, and the synthesis would still have been so incomplete that it would have satisfied none of the participants. When time was ripe for it, it was still impeded by the extreme specialization that had grown up in biology as in other sciences. Students in the various narrow specialties tended to be ignorant of any field but their own and were sometimes downright hostile toward any other.

The breaking down of these barriers, a necessary preliminary not only for further progress in the study of evolutionary adaptation but also for true comprehension and integration of all fields of biological research, is a recent development. A precise beginning can hardly be designated, but this movement was presaged some ten or fifteen years ago by individual efforts, mostly English and American, to renew, broaden, and modernize the attack on evolutionary problems. More recently it has been put on a wider basis and has become a conscious collaboration between many specialists in different branches of biology. In this country, this has been promoted by the National Research Council Committee on Common Problems of Genetics, Paleontology, and Systematics. The work of the committee is broadened and put on a permanent basis by the recently (1946) established Society for the Study of Evolution.[2] A high point in this movement toward cooperation and synthesis and a powerful stimulus for future work were achieved by the recent Conference on Genetics, Paleontology, and Evolution held at Princeton University.

Those of us engaged in this movement find the results so exciting that we may unintentionally exaggerate their extent and importance. It is quite clear, however, that the study of evolution, viewed with apathy and even considered wholly outmoded by many biologists a few years ago, now attracts more attention than at any other time in the history of science. The future alone can judge, but it also seems probable that more sound progress has recently been made toward understanding the processes of evolution than in all the previous centuries of study put together.

The result, to this point, is essentially a new theory of evolution, or a new body of theory, although its basis is a synthesis of previous theories. It has been called neo-Darwinian because it includes the Darwinian factor of natural selection and excludes the Lamarckian factor of inheritance of acquired characters, but this is a misnomer and is likely to mislead seriously. It involves, and to a considerable extent it grew out of, rehabilitation and restatement of the principle of natural selection in genetical and statistical terms, but its understanding of natural selection is quite different from that of Darwin and still more different from that of the neo-Darwinians. It also embraces a great deal more than natural selection. It is more complex than any one of the previous

[2] With the aid of a grant from the American Philosophical Society, the Society for the Study of Evolution has also established an international journal, *Evolution*, Volume One of which is being published in 1947.

theories; their factitious simplicity was a weakness and a cause of their failure. It can hardly be labeled with the name of any one man, and an outstanding characteristic is precisely that it is the product of many minds and has drawn data and principles from many fields. It may perhaps most appropriately be called simply the synthetic theory.

It would manifestly be impossible to present an adequate statement of the synthetic theory in a simple or brief way, and this would not be appropriate in this broad review of a single, even though of a very basic, problem of evolution. Various aspects of the theory have recently been treated at book length by several students from different points of view, among them Dobzhansky, from the point of view of a geneticist, Mayr, from the point of view of a systematist, Julian Huxley, from the point of view of a neobiologist or naturalist, Stebbins, from the point of view of a botanist, and Simpson, from the point of view of a paleontologist. Equally or more important studies have also been made by Muller, Sewall Wright, Haldane, and R. A. Fisher, and fairness would demand, but unfortunately space prohibits, the mention of dozens of other names.

To elucidate, as far as can be done in a brief summary, the synthetic theory's solution of the problem of adaptation, it is well first to restate the problem as it appeared to the opposing teams which, by combining their efforts, produced the synthetic theory. The geneticists had, as they thought and as we paleontologists, systematists, and other nongenetical naturalists have come to accept, identified the materials for evolution. These materials are discrete changes in the hereditary mechanism which is, in the main, an elaborate organization of chemical substances into units called genes, which are assembled into united groups called chromosomes, which in turn occur in sets one or more of which are present in every cell that is to grow into an organism. These chromosome sets as a whole, with all their constituent parts, determine what the organism will become as it grows. The geneticists found, moreover, that the changes in this mechanism, those changes that are the materials for any evolutionary change, are not predominantly adaptive. On the contrary, they have no particular orientation toward adaptation but are random in this respect. Still worse, this means that the great majority of them are definitely opposed to adaptation, because in any organism the number of possible changes toward increased adaptation or toward new adaptations is very much less than the number of possible changes away from adaptation. Moreover, what an organism is like is determined by its whole set of chromosomes. Adaptation rarely requires a single new mutation, but an entire set coordinated in a new way. That genetic mutations should produce a new sort of organism adapted to an available environment is improbable in so high a degree as to seem almost impossible. Even granting that natural selection would, in Darwinian fashion, weed out the grossly unfit genetic combinations, the fit simply would not arise, or would arise so extremely infrequently as to be altogether exceptional in nature.

On the other hand, the set of facts known to the rest of us, the nongeneticists, demonstrates beyond any possible doubt that these almost impossibly improbable genetic combinations are in fact so common in nature as to be nearly universal. The only possible way to reconcile the facts of genetics with the facts of adaptation is to find some force or process in nature that is capable of generating a high degree of improbability, as R. A. Fisher has put it or, in other words, of assuring that an outcome that is genetically extremely improbable will nevertheless become usual.

This force has been identified beyond reasonable doubt, and it turns out to be an old friend (or enemy), natural selection, but

natural selection on a new basis and in a new role, a process subtly but fundamentally different from the natural selection of Darwin or of the neo-Darwinians. It is not merely the negative process of elimination of the unfit, by assuring that they will have fewer offspring than the fit; it is the positive and creative process that was left out of the picture by the Darwinians and that was sought in vain by the Lamarckians, the vitalists, and others.

How natural selection works as a creative process can perhaps best be explained by a very much oversimplified analogy. Suppose that from a pool of all the letters of the alphabet in large, equal abundance you tried to draw simultaneously the letters *c*, *a*, and *t*, in order to achieve a purposeful combination of these into the word "cat." Drawing out three letters at a time and then discarding them if they did not form this useful combination, you obviously would have very little chance of achieving your purpose. You might spend days, weeks, or even years at your task before you finally succeeded. The possible number of combinations of three letters is very large and only one of these is suitable for your purpose. Indeed, you might well never succeed, because you might have drawn all the *c*'s, *a*'s, or *t*'s in wrong combinations and have discarded them before you succeeded in drawing all three together. But now suppose that every time you draw a *c*, an *a*, or a *t* in a wrong combination, you are allowed to put these desirable letters back in the pool and to discard the undesirable letters. Now you are sure of obtaining your result, and your chances of obtaining it quickly are much improved. In time there will be only *c*'s, *a*'s, and *t*'s in the pool, but you probably will have succeeded long before that. Now suppose that in addition to returning *c*'s, *a*'s, and *t*'s to the pool and discarding all other letters, you are allowed to clip together any two of the desirable letters when you happen to draw them at the same time. You will shortly have in the pool a large number of clipped *ca*, *ct*, and *at* combinations plus an also large number of the *t*'s, *a*'s, and *c*'s needed to complete one of these if it is drawn again. Your chances of quickly obtaining the desired result are improved still more, and by these processes you have "generated a high degree of improbability"—you have made it probable that you will quickly achieve the combination *cat*, which was so improbable at the outset. Moreover, you have created something. You did not create the letters *c*, *a*, and *t*, but you have created the word "cat," which did not exist when you started.

Creative natural selection works in a similar but vastly more complicated way. The number of possible combinations and arrangements of genes and chromosomes in a group of organisms, even of relatively simple organisms, is enormous, so enormous that most of them will never occur because the number of organisms in the group is much less than the number of possible genetic systems. Clearly the critics of natural selection, in the old sense, were quite right in concluding that merely eliminating inadaptive combinations would rarely, if ever, insure the appearance of adaptive combinations. A mutation favorable in itself or in some particular combination would quickly be swamped among the inadaptive mutations and would usually be eliminated because it occurred in unfavorable associations. But it has been demonstrated both theoretically and experimentally that selection acts in a positive way tending to increase the percentage of favorable genes in a population, a process analogous to your increasing the proportion of *c*'s, *a*'s, and *t*'s in the alphabet pool. It thus greatly increases the chances not only of favorable single genes but also favorable hereditary combinations. Moreover, selection also acts on combinations and arrangements of genes. Just as you clipped together the "adaptive" sets of letters *ca*, *ct*, and *at*, selection prevents adaptive combinations of

genes from being dispersed again and increases their frequency in the population, thus promoting the probable development of still more complex, adaptively still more favorable, combinations. It has also been demonstrated, to meet another old criticism of natural selection, that under certain conditions (conditions that do obtain in natural populations that are well adapted) an extremely small selective advantage suffices to insure that favorable genes and genetic systems will be preserved and will increase in frequency.

Another major criticism of natural selection, in the old, Darwinian, sense, is also being well answered by the studies of, particularly, paleontologists and geneticists. This criticism, you will recall, was that evolution exhibits trends that proceed without deviation ("orthogenetically," in paleontological cant) and that may be inadaptive or may proceed well beyond the point of greatest adaptive advantage. In some cases these trends simply did not really exist or were not really the single, straight-line affairs that they had been thought. W. K. Gregory and others have well shown that many of these straight lines are more a product of scientists' minds than of nature. In other cases, the interpretation of some features of these trends as inadaptive seems to have been due to an inadequate, sometimes a naive, conception of adaptation and of heredity. One factor here is that the hereditary mechanism makes it quite possible for an unfavorable trend to become associated with a favorable trend and to be carried on with it as long as the balance continues favorable. There are some remarkable complexities involved in this subject, but it seems reasonably clear that long-continued trends in general are kept going by natural selection.

The action of selection, understood in this newer way, results in the appearance and spread of genetic systems and therefore of sorts of organisms that would never have existed under the uncontrolled influence of mutation and random recombination of the elements of heredity. In this sense, although it does not create the raw materials, the mutations, natural selection is definitely creative. It creates the most important product of all, the integrated organism. Builders do not make bricks, but they create houses, and the bricks are not adapted into a use until they are assembled into houses. Natural selection is a builder that uses mutations as bricks, and its constructions are adaptive. The figure of speech would be still more apt if builders had to find their bricks in piles of rubble in which most of the pieces were inappropriate for their purpose.

The whole process and its results are much more intricate than is apparent from this simplified explanation, and natural selection is by no means the only process involved in evolution or even in adaptation. Creative natural selection is the directive, pseudopurposive factor back of adaptation, but it is not always the decisive factor in evolution and it never acts alone. Other factors explain, for instance, why not all characters of organisms are adaptive, why not all newly evolved sorts of organisms are adaptively superior to their ancestors, and why organisms adapted to essentially the same activities and environmental conditions may nevertheless be strikingly different in many respects.

Among the other processes of evolution, one particularly pertinent to the present theme requires further, brief, comment, and that is preadaptation. Preadaptation commonly occurs in evolution and is sometimes of crucial importance, but it is not as universal as was believed by its earlier proponents, and its role and the factors underlying it are rather differently understood in the synthetic explanation of evolution. It now appears that preadaptation may arise in two or three quite distinct ways, none of which is wholly random or

spontaneous, as all preadaptation was once thought to be. Occasionally one mutation, using the word in a broad sense, may produce a relatively great difference from the parents in the organism affected by it, and this difference may be adaptively favorable for available new habits or environments. This is, in a sense, preadaptation, but in the same sense any useful mutation is preadaptive, and this application of the term is not enlightening. Once such a mutation has arisen, it is simply another bit of material for natural selection and its status is the same as any other mutation except for its larger size, in terms of structural change. Large or small single-step adaptations, as Julian Huxley calls them, seem, however, to be of relatively little importance in evolution as a whole. They explain occasional evolutionary events, but the usual course of adaptation is slower and more cumulative.

One type of preadaptation, speaking more strictly, may occur when selection becomes relatively ineffective and the frequencies of the various genes in a population tend to drift, aimlessly as it were, without definite control by selection. This has been called the Sewall Wright effect, after one of the eminent cofounders of the synthetic theory, who has demonstrated that this effect is most likely in small, isolated groups of organisms. In most cases the changes brought about in this way are inadaptive, and they are usually a prelude to extinction, but occasionally they are preadaptive. Then they may facilitate a relatively abrupt and radical change in habits and environment, a change of the sort that I have called quantum evolution. It may be that many of the major and broader new types of organic structure arose in this way. This is probably the nearest thing to purely random preadaptation in the old sense, but even this cannot be considered wholly random. The structure from which such developments depart owed its integration to (nonrandom) selection, and the new grade of structure must, if it survives, in turn be integrated by selection which will act with particular force on such groups.

Another, and probably a more common if not more basically important, sort of preadaptation arises when, for instance, a selectively controlled adaptive structure develops to the point where its use in a new way becomes possible. Then it is preadaptive with respect to its new use, and selection will then direct evolution in this new direction if the new use is advantageous. This sort of preadaptation is not random at any point. It is always directional and directed, but the direction changes. Such occurrences demonstrate how the direction of evolution can change under the influence of selection even though the environment remains essentially constant.

Adaptation by natural selection as a creative process and preadaptation in the special senses just explained are the answers of the synthetic theory of evolution to the problem of plan and purpose in nature. Of course much work remains to be done, many details to be filled in, and many parts of the process to be more clearly understood, but it seems to me and to many others that here, at last, is the basis for a complete and sound solution of this old and troublesome problem.

Adaptation is real, and it is achieved by a progressive and directed process. This process is natural, and it is wholly mechanistic in its operation. This natural process achieves the aspect of purpose, without the intervention of a purposer, and it has produced a vast plan, without the concurrent action of a planner. It may be that the initiation of the process and the physical laws under which it functions had a Purposer and that this mechanistic way of achieving a plan is the instrument of a Planner—of this still deeper problem the scientist, as scientist, cannot speak.

THE SCOPE OF SCIENCE

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SCIENCE, like a flooding sea, has washed over many successive shore lines toward man's intellectual horizon. Today, more than three centuries after Newton, we can still say, with him, that an ocean of truth lies before us—but not "all undiscovered." How extensive the unknown remains, how quickly or slowly man will chart it, however, is not our present concern. This is rather with the unknowable than the unknown or, more precisely, with the necessary limits of natural science and its methodology. For the exuberant optimism of the last century, which saw progress to Utopia as man's destiny and science as the means to it, has, in disappointment and impatience, given way to a mounting distrust of the rational and natural and to a resurgence of faith in the supernatural and divine. As when any political party retains power long enough for its imperfect performance to become apparent, or even for fortuitous distress to fall upon the people, the citizens "turn the rascals out," so mankind is now inclined to turn upon man's reason and its representative, Science.

Some thinkers of today insist on subordinating scientific knowledge to a higher "wisdom," as does Maritain, or even denounce science and its attitudes as the "central corruption" of modern times, as does Adler. Others set sharp limits to the methods of natural science: they are not applicable to the study of mind, says Blanshard; or society, say Knight and Novikoff; or history, say Becker and Barzun; or values—moral, aesthetic, or rational—say most humanists. With these positions I propose to take issue, and as sharply as possible, to aid clarification. I am not concerned to exalt my own field of endeavor for parochial pride or privilege; the most restricted acreage one could allot to science would still keep scientists busy cultivating it for many generations. Perhaps this essay exemplifies only "that fierce passion for monism which burns in many scientific breasts" (Woodger). Or perhaps it is an honest effort to avoid "the error of misplaced concreteness" which results from the greatest sin of the intellect, "the intolerant use of abstraction" (Whitehead).

THE NATURE OF SCIENCE

First, what is science? Not merely the work of men in the ranked laboratories on our campuses, nor of men everywhere in laboratories or on campuses. It is wider and deeper than that. It is a state of mind or philosophy, and a procedure, and the results of these. It aims "to translate experience into general laws of predictive value." "Value," in this phrase of Malinowski, implies the more abstract values of science: predominantly truth; with utility (a form of good, if not the whole of it) in increasing focus; and beauty, not absent, but background. It attacks, and solves, problems by imagination applied to sensory evidence and "curbed and ruddered" by reason. Science is the outcome of the Hellenic break with the more primitive "mythopoeic" mind; of seeing nature as impersonal and implacable and so regular, rather than personified as "I and thou," as Frankfort puts it, and so capricious.

Science is a creation of man and is a work of art. It requires the same talents of mind and hand and results in the same generation of new form or structure as do other artistic

productions. To be sure, the particular ingredients needed for achievement differ in proportion and detail, and an individual may be more fit for one type of endeavor than for another. Also, because of its highly cumulative and cooperative character, artisans or technicians play a greater part in science than in some forms of art. Perhaps science is closest to architecture, which combines use with beauty and requires collective effort at many levels of creativity. But it would be an error to consider technology as unimportant even in the most individual arts; where would they be without chisels or strings or paper, without printing and radio, without, for that matter, the tool of language itself? As Shotwell has well put it, we "need a *Sartor Resartus* in the history of literature to show how naked and helplessly limited is thought except when provided with mechanism." (This, to anticipate, is equally true for supplying fresh ingredients to think about as for expressing the resultant thoughts. Technology enhances the sense organs of man and enriches his experience, no less than it gives power to his expression.) Shotwell further distinguishes two kinds of creations of value, one possessing "monumental immortality," the other "useful immortality." The former are enduring objects, statues or buildings or dams; the latter, enduring ideas. The ideas are more important in science and some arts; the objects, in technology and other arts. Perhaps one reason the line between the sciences and the humanities is so blurred is that humanistic thought has itself been strongly influenced by the attitudes of science; or, if you prefer, the Hellenic mentality has created both in its image.

For science and scientists, like art and its devotees, are products of man's culture, as they are also its molders. They are differentiated mechanisms and units of the human epiorganism. They have come into existence by action of the same forces of evolution that earlier produced man and his thinking brain. In detail, the science of an epoch can be traced back to the social pressures of its time, as Crowther and Hogben have emphasized, and forward into the social pressures of the following period. The scientist may be looked upon as the sense organ, or receptor, of the social organism which, like the receptor of the biological organism, develops as an adaptive response to environmental influences and, developed, accelerates further adaptation in the whole organism—an autocatalyst of evolution. And it were well to note, at this point, that science transcends the individual man or mind as every organized system does its component units.

I certainly cannot prove that the scientific view of the universe is the best or the final metaphysic which man will reach, or that truth is a crowning value. But I do maintain that this approach, of those man has tried, has led to great and progressive change in human affairs—mental as well as physical—and that, until an approach more satisfactory by empiric standards arises or the limitations of the present one are clearly demonstrated, man had better "hold fast that which is good."

DIVISION OF SCIENCE

Only such broad descriptive sweeps deal with science as a collective entity; for the particular sciences differ from one another enormously in detail. The units of concern, the concepts which prove useful, the methods for study are, in practice at least, fairly unique to each area. A good yardstick for yards is likely to be a poor one for health. The method of breeding in biology is as different from that of timing in physics as is the mental test or sociological questionnaire from breeding. Even if all could be accurately measured, the "pointer readings" would still be vastly different for volts,

vitality, and values. Further, at present, the degree of quantification, of logical compulsion of available evidence, and of predictive assurance varies greatly from segment to segment. Yet within the proper scope of science these particularities are secondary and might, with sufficient knowledge, reduce to continuities. Which brings us sharply to our primary problem.

SCOPE OF SCIENCE

Historical. It is all too obvious that the Newton of mind or the Darwin of society or the Lavoisier of values has not arrived. The question is: Is he possible? I maintain that he is; and that those who deny such a possibility are no more than modern Bishop Wilberforces. Look at the past; what pessimists have men been! When regularity and predictability were established for the simpler material phenomena on earth, when geo-metry was developed and the action of levers understood, celestial events were still considered beyond man's understanding and daring. For a millennium, the angels still moved the heavenly bodies on their ways. Not until Newton demonstrated the universality of application of his equation for gravity, indeed not until Bunsen, a scant century ago, showed by spectroscopic analysis of sunlight that the same elements compose the sun as the earth was this tidemark finally flooded over. (Kirchner, considering that water might occur on other planets, earnestly raised the question whether a baptism with water from Venus would be effective.) Today, no vestige of thought leaves our planet unique in the universe—even as a habitation for life, none question the possibility of other similar satellites scattered about the galaxies; yet the best minds of the past, as good as those of today, were emphatically convinced that such matters were forever beyond the grasp of man's understanding and must be settled by faith.

Or, consider the problem of the living. Almost to our day, if not actually into it, the animate was shrouded with the mysterious and the impenetrable. Vital spirits, presiding entelechies, a whole corps of benevolent gremlins made the parts of the body go through their chores and the whole execute the beautiful acts of living. Why, even in my own student days, secretion was an expression of the vital activity of glands; now it is analyzed in terms of oxidative energy, ionic interchange, differential permeability to polar molecules, and the like. A little over a century ago, the substances of the living were forever beyond the province of the chemist, a vital force alone could make "organic" compounds. Then Wöhler made one in the test tube, and the chemical hordes, given hope and courage, poured after with hundreds more. Yet organic compounds *are* different in many important respects from inorganic ones, and it is only in the immediate present that the underlying laws of molecular organization are reducing these differences from the qualitative to the quantitative. (This is an especially convincing instance to support the view that, at all levels, the qualitative jump will in time become a quantitative difference and that "prediction upwards" is not impossible but only enormously difficult.) Again, the gross structure of organisms was well known to Galen and was described in modern form by Vesalius four centuries ago; and even its microscopic structure has been limned out for three hundred years. Yet our knowledge of function was largely nonexistent before Harvey and is still expanding at an accelerating rate—along with the unraveling of submicroscopic organization.

When it became clear that vital phenomena were not outside the grasp of science, new boundaries were set by the pessimists. The properties of the individual brute organism, yes; but its origin, development, inheritance, never! Plants and ani-

mals, yes; but man and mind, never! In 1909 (or actually 1930, in the English translation) Radl could say in his critical *History of Biological Theories:*

> No observation, experiment, or intellectual speculation can alter the fact that each organism is an autonomous individual; it contains within itself the laws of its existence; and, in spite of the influence of its surroundings, it develops along its own lines. As to the nature of the directive force we know nothing.

Also he says, "In recent years the fantastic idea that we come nearer and nearer to reality, the more we magnify the structures we are observing, has lost ground." With electron microscopy, X-ray diffraction, and still newer methods just now beginning to reveal the molecular machinery of protoplasm! Yet he makes this vivid comment on the reception given Darwin's views even by scientists best qualified to follow them:

> But few dared ask how the creation of these various forms of life, or of the whole living Universe, came about. They felt that human understanding was powerless to attempt an explanation—too much was known of Nature's impenetrable mystery and of her infinite power. Then came Darwin . . . he taught that life has continued since the beginning in one unbroken stream, and that the origin of new forms and extinction of old ones can be explained without invoking any factors which surpass human understanding. Many of the paleontologists of the time regarded the theory almost as a personal insult!

That battle to unbind inquiry is won, except for some mop-up action in the intellectual back country. But the barricades, or, returning to the earlier figure, the dikes, are still actively manned to defend the territory of the human and the mental from the encroachments of science. These we shall examine later; now the more general problems of scope require attention.

GENERAL LIMITATIONS

Science has limited aims, it is directed toward understanding rather than feeling. Even if in time it becomes possible to explain fully the factors that enable a particular masterpiece to stir the awareness of beauty in some individuals and not in others, this will still be separate from creating or appreciating masterpieces. True, such understanding should make easier and more certain the construction of new forms which will arouse desired feelings; but the creating and the feeling remain separate from it. So also for objects of utility rather than beauty; understanding makes possible their initial or efficient production, it is not necessarily concerned with their manufacture or use. Science has learned much of how caffeine acts on the brain, with little influence on coffee-drinkers; it has learned much of how pneumococci act in the body, with tremendous influence on the expert treatment of pneumonia.

Reason and emotion are more than traditional or vulgar terms of crude description. They can be characterized by more rigorous psychological and physiological criteria and are fairly sharply separated in brain structure and function. Emotional behavior, and probably consciousness, depend on the activity of phylogenetically ancient brain parts, which are similar through much of the vertebrate subphylum. The structures, the behavior, and presumably the basic awareness involved in fear, rage, pain, and the like are comparable for most individuals of many species. The emotions are aroused and discharge themselves with the same sort of predictability and inevitableness that characterize simple reflexes. They are mainly of significance for self-survival and are private. The feelings of an individual cast a small glow in his immediate vicinity, since others recognize that he is angry or elated; but even they cannot enter into his current experience, though they may have had like ones (how pale is one's personal recollection of his own past seasickness or one's present empathy with a friend's), and any removal in time or space washes away all trace of

influence. The emotions experienced by victims of Buchenwald have absolutely no influence on us; the reports of what happened to them do. For these pass through reason and the newer evolved parts of the cerebrum, which are concerned more with group and species preservation. Reason is public and permits co-understanding. A person's private feelings—his appreciation, belief, ecstasy—are of little and rare concern to others, however overwhelming to himself, except as they guide his actions; a person's insights, ideas, thoughts, can be public and can generate co-understanding across centuries and hemispheres. Art is concerned mainly with private feeling, science with public thinking.

Which statement raises sharply the question of whether science can ever penetrate the private or is limited to public "pointer readings." Bridgman, especially, has emphasized that science cannot penetrate the secrets of the individual but must be content with unraveling the class. In the case of sentient individuals, there is the similar problem of subjective and objective, but it will be convenient to consider this separately. There is much validity to this position, but I am not so much impressed with it today as earlier.

For one thing, while it is true that science (as contrasted with art) is rarely interested in the individual as such, it can disentangle the particular instance, with effort, to a degree set by the general level of understanding. Science has revealed enormous detail about our single, individual, private solar system. The physician diagnoses and treats each particular case, as an individual, on the basis of general scientific knowledge. For each person is not merely a member of one class but of innumerable ones—classes of age, sex, weight, race, build, metabolism, blood pressure, artery hardness, and so on—and the criteria of membership in each of several classes soon suffice to define uniquely the individual. This is entirely familiar at the simpler and more abstract level; for three planes (classes of points) determine a point (an individual in each of the three classes). And, while it is true that the internist still is far from understanding the whole individual who is his patient, the analysis can proceed as far as interest and time and abstract knowledge permit. The psychiatrist, devoting far more time to the single patient, knows far more of him than does the internist. So individual and private a matter as one's dreams is now subsumed under general rules, and dream content can be interpreted, predicted, and even manipulated. And Lowes, dedicating years to the scientific study of Coleridge's unique imaginative processes and products, has been able to trace these in superlative detail. If the philosophic position is finally taken, with Bergson or Kant, that, however completely an entity be analyzed from outside, there remains an unknowable essence of being itself, I would only point out that this would have the same degree of validity for a stone, a crystal, a machine, or a tree as for a mouse, a man, or a symphony.

A second weakness in the public-private dichotomy is revealed by consideration of the system to which something is public or private. The sharp margin then melts into a series of gradations. I must refer to other essays for consideration of the nature of orgs and organisms, single systems composed of subordinated interrelated units, of the relationships between units at different hierarchical levels, and of other undeveloped statements in this article. (See *Science*, **101,** 582, 1945, for references.) What is external to the atom, narrowly considered, may be still internal to the molecule of which the atom is part. Likewise, what is public in relation to a particular cell may be private to the multicellular organism in which it finds its being. Yet, because there is interaction and some sharing of entity between

such parts and wholes, the public and private at each level also partly interpenetrate. What is private to the cell is still in part public to the organism, as what is private to the organism is largely public to the cell. But there are solid biological grounds for recognizing a society, or epiorganism, as an org composed of individuals as its penultimate units, and the same considerations apply as to public and private between the individual man and his society. All members of a culture are to an important degree privy to each other's private existence, as they are to the collective privacy of that particular culture. And, in rapidly diminishing degrees, this is also true for all entities as part of a single universe.

This same consideration applies to the problem of objective-subjective, or body-mind, as does also the discussion of class and individual. The private emotions of a particular person are certainly difficult of study, but they are not inaccessible. Direct evidence for awareness is available to each of us only from his own subjective experience. Nonetheless we are completely satisfied, from the similar behavior of others, that all normal men have a comparable subjective experience. What the range is of detailed differences is less easily decided, but the sciences of mind can make highly probable statements on such matters. We are likewise satisfied that apes, dogs, and other mammals also possess a conscious awareness and guess, from the degree of likeness of their bodies and behaviors to man's, the extent to which theirs is comparable to ours. As attention passes on to less similar entities—the simpler animals, plants, unicellulars, viruses, definitely inanimate objects—we feel less confident in extrapolating as to the amount, kind, or even presence of an attendant consciousness. A similar uncertainty faces us as to psychic aspects of our own constituent cells and organs, of our brains in sleep, coma, or under depressant drugs, and of our collective selves as units in an epiorganism.

My own view is: that every org or system offers both physical and mental aspects; that there is the same sort of continuum (in kind if not degree) of awareness as there is of body from the simplest inanimate system to the most complex living organism; that what is private to the org, viewed from the internal aspect, is recognized as subjective and mental, and, viewed externally, the org is seen as objective and material; that each org is a unit in a more inclusive org or is composed of subordinate orgs or, mostly, is both composed of and helps compose other org units so that subjective and objective interplay from level to level; that, to the extent that a given org is poorly integrated and but little set apart from other systems or the rest of the universe, the distinction between internal and external approaches meaninglessness and subjective awareness tends to vanish; and that material and mental are relativistic terms which involve the relation between the observer and the observed.

This condensed statement demands far more elaboration than is here possible, but a few points must be followed up. If mind and matter are but different aspects of the same entity, it is useless to ask if material events cause mental ones or if the psychic can act on the physical. Man, seeking knowledge of the universe, may find the clues, now from his inner experience, now those from the outer world, more pregnant with suggestions for study. It may be convenient and useful to think of an anesthetic producing unconsciousness or of a neurotic state producing gastric ulcer; but ultimately we will recognize the physical situation in the brain, seen most simply as induced by a chemical agent, of which unconsciousness is the concomitant of greatest interest; we will recognize the physical part of the situation, most interesting in its psychologi-

cal aspect of neurosis, which generates another situation, of most concern in its physical aspect as ulcer. In comparable manner, the relation between successive states of a system is sometimes most easily grasped in terms of the first as the *cause* of the second, sometimes, of the second as the *purpose* of the first. Either may suggest the direction in which to seek for the mechanism of the relationship.

This leads to a second critical issue, that of fate or freedom, of inevitability or contingency. On the physical plane, indeterminacy at the electron level seems now established. Whether this is a manifestation of ultimate contingency in nature or is a consequence of our present limits of analysis of the subelectronic realm is perhaps not known. Let us assume that contingency is inherent in systems. Then a system may pass from state A, fully delineated, to state B, slightly fuzzy—only slightly, since certainly what indeterminacy may exist is only marginal. This allows complete freedom of locus for cosmic unrolling over its eternities—as the most minute deviation from geometric straightness will permit a line to curve on itself in a sufficient distance. But it allows, on the usual temporal scale of human interest, a narrow cone of indeterminism, indeed, along "time's arrow." The degree of uncertainty might be more like that in the acting of a play as compared to the certainty of unreeling of a motion picture. Further, this contingency, being uncaused, cannot be a matter of active choice; and certainly cannot be interpreted as the control of the physical by the mental. Our experience of freedom and volition, to come sharply to the human level, is but a part of our experience of awareness: it may be, to be sure, the subjective aspect of whatever amount of contingency is possible to so elaborate a system; it cannot be an uncaused cause of physical action.

Much direct evidence supports this conclusion. Most of man's problem-solving activity is actually unconscious and so certainly not directed by a conscious will. Under posthypnotic suggestion, a person will perform some unusual act—known beforehand to all present but himself—and will be completely persuaded that he acted of his "own free will." Conversely, the compulsive, despite every effort of his conscious will, performs acts of which he disapproves —such as unending repetition of washing his hands. Recently it has been discovered that, during recovery from a chemically-induced coma, a person may "will" to clench a hand, in response to a request, but nothing happens. A minute or more later, while the subject may be busy "willing" to move a foot, the hand suddenly clenches—to the complete surprise of the subject! The observant friend, only less than the trained psychiatrist, can predict with impressive accuracy how a person will decide and what he will do in a great variety of situations. And so on and on through the familiar phenomena and arguments.

Perhaps man should have been able to deduce electron indeterminacy from his own private feeling of freedom, just as he has had to follow from the overwhelming determinacy of the electron and more molar systems to the overwhelming determinacy of animal and human behavior. Certainly we are closer to the internal view of man's mind than of the electron's "mind" and must exploit this vantage point fully in getting clues as to the direction of analysis in the external dimension. Artists, especially poets and other writers who deal with man's inner life, have contributed greatly in noting and clarifying the phenomena ultimately to be analyzed and synthesized—as the naturalist-observer has supplied the raw factual nuggets which were beaten by the theorists and experimenters into such syntheses as evolution. Subjectively, man recognizes purpose as well as volition, values and ends

as well as strivings and means. These clues to nature have been used widely, if not always wisely, in scientific, especially biological, thinking. When the mischievous confusion of personified willing or purposing as a cause of material happenings is avoided this has been very profitable.

The physiologist von Brücke once said, "Teleology is a lady without whom no biologist can live. Yet he is ashamed to show himself with her in public." To discover the purpose or significance or utility of a biological process or structure contributes to an understanding of it just as does the discovery of the mechanism which mediates it. The former suggests directions in which to look for mechanisms and for new phenomena, but it does not lead to that detailed mastery of a problem which enables man to manipulate its course. This comes only from the knowledge of mechanism. Teleology is suspect because, too often, a solution in terms of purpose has been the end rather than the start of an inquiry. The development of egg to embryo to adult—the multiplicity of events: cell growth, division, migration, differentiation; the structural formation of folds, bubbles, buds, and the functional eruption of contraction, secretion, digestion; all in amazingly timed and interacting sequence—all this has meaning in terms of the end result. The outer cell layer folds into a tube *in order to* form a nervous system; the middle layer, to form a heart and blood vessels. The skin over the growing retina forms a transparent lens in order to create a useful eye. Or, a related basis for giving meaning, in terms of history rather than purpose, the gill slits appear briefly in the human embryo and then vanish because the young individual is recapitulating the evolutionary stages of its kind.

Without such orientations the welter of phenomena would be overwhelming; with them, phenomena are put into a pattern, take on form and relative importance, and can be grappled. The major zigs in the zigzag course of science result from shifts in such orientations. But, it bears repeating, these only furnish the basis for the detailed analysis which eventually reveals mechanisms. A mechanical pressure, due to osmotic swelling, due to metabolic products, due to a rich supply of oxygen, due to its surface position, leads to the folding of the outer embryonic layer which forms the neural tube; the forming retina liberates a specified chemical which diffuses to the surface cells nearby, modifies the protein they form so that the molecules are parallel threads rather than scattered balls, and leads to the production of a transparent lens: such are the answers one seeks to demonstrate by experiments directed toward mechanisms. And only such answers enable one, in time, to tinker intelligently with the works—by increasing osmotic pressure in other known ways, by adding other chemicals to influence protein formation, by the myriad tricks that man has learned from nature. Mechanism adds utility to truth.

Conversely, the finding of well-elaborated mechanisms which supply concrete means to a guessed end gives important support to the validity of the guess. It even may supply some quantitative criteria for a scale of values. When man, proud possessor of an overstuffed brain, calls the brain the most "valuable" organ and himself the highest form of life, he does well to suspect himself of anthropomorphizing. Valuable by what criteria? Animals can at least live without brains, they die without many other organs. But when he discovers, in many animals, that during starvation all organs waste away, are used for food, except the brain and heart, which are fed by the others; that a special reflex device is built into the arteries to the brain so that, within the limits of possible adjustment, the brain is insured its

full blood supply whatever happens elsewhere; that the brain is more elaborately protected from injury than any other organ, even than the growing embryo, for it, like the embryo, is floated in a shock-absorbing fluid and, unlike the heart, is completely encased in bony armour; that success in winning habitats and capturing food parallels the development of the brain more than any other attribute: when man discovers these facts, he can feel distinctly more secure in his value judgment about the brain and so about his position. Not only man's mind, but his body and other animal bodies "value" the brain.

We recognize the staggeringly important purpose of mitosis and gene duplication; we are consequently willing to devote tireless effort to discovering how this duplication is managed. We follow forward in time the consequences (purpose) of gene action, we follow backward the antecedents (cause). In the case of an idea too, a sort of social gene, we are concerned with purpose and also with cause; we may deal with it from the mental aspect if, at present, this is more convenient, because immanent, than from the physical aspect. The scientist, especially, but not uniquely the biologist, thus finds in the outer world the counterparts of his subjective experience of purpose, of value, of volition. If he does not confuse his time dimension to mix mechanism and purpose, or become lost in the levels of orgs and so tangled in the internal-external with its attendant mental-physical; if he uses his subjective awareness, as he does his sensory information, as a clue to the universe and of useful ways to study it; then such "projection" is not only warranted but indispensable.

This leads to our last general problem, the epistemological one of reality and knowledge of it. Man may indeed be, with Henry Adams, "on a sensuous raft adrift in a supersensuous sea." But, if so, he will never know it except that the raft expands indefinitely as science extends his perceptions. As argued elsewhere, scientists and their instruments are sense organs of society; and collective man is certainly sensible to vast territories of the universe beyond those accessible to the individual. Not only the microscope and telescope, which extend spatial vision to the minute and the colossal, and the spectroscope, which extends "color" far beyond the visible spectrum; but also electronic instruments which create or detect radio waves, X-rays, radioactive rays and particles, cosmic rays—or for that matter, the simple compass needle which detects a magnetic field—all inform us of phenomena completely foreign to man's biological senses; they stretch the raft. But no extrasensory avenue to the mind exists, there is no royal or divine road to knowledge. This can be certainly demonstrated, I am satisfied, from the facts of biology and especially of neurophysiology, if one accepts the view suggested earlier as to the relation of the material and mental, or even any view which places brain and mind in some interdependent relationship; and such interdependence can also be demonstrated by unexceptionable facts.

Of course, the resultant of any action upon any system includes the nature of the system acted upon; and the neural and mental events provoked by an experience depend also on the nature of the brain and mind in which they transpire. Man's mind can act upon sensory data only in terms of its own organization; all experience is sieved through the "a priori net of the mind," in Eddington's telling phrase, and Koffka has emphasized that thought itself carries a "tone" of true or false. In material terms, this is to say that the structure and function of the nervous system determine the future adventures of nerve impulses which reach this organ; or, more generally, the consequences of any given action upon

the nervous system depend on its general organization and particular state. The "universal" truths are those which are built into the brain machine and are difficult even to discover and recognize, let alone to analyze in terms of mechanism.

Yet real progress is steadily made. "Generalization," "extrapolation," "closure" as recognition of entity, "insight," "relationship" in "more than" or "nearer than" or even "better than;" all these can be demonstrated in animal as well as human behavior, studied quantitatively by such laboratory techniques as the conditioned reflex, and related, still gropingly, to the physiological anatomy of the brain. Revelation is no different. It can, often enough, be understood objectively, especially in the insane; and, however impelling to the individual, can have no collective weight. Unless cumulative reason is held superior to individual inspiration all of man's knowledge and achievement is imperiled. (This is a far cry from Luther's estimate of reason, "that silly little fool, that Devil's bride, Dame Reason, God's worst enemy." Or is it?)

This is not to say that all questions can be answered. Many can only be approached ever more closely, like comprehension of the individual entity as earlier discussed; and, for many of these, the approach has not, or has barely, begun. Science continues to widen and deepen its generalizations, even to ethical problems; but any answer which is inherently beyond the potentiality of science is also beyond the possibility of reliable collective knowledge. Man, individually or collectively, must often act, must answer questions, with no assurance that his conclusion is correct. Well and good, even incorrect action generates new experience and permits improved understanding. But it is a sign of childhood, in the individual or the culture, to believe one's fancies. The adult attitude is to face our present limitations with courage and our final limitations, as these are established, with renunciation.

Aside from the more philosophical problems of the outer limits of science, there are a number of particular areas which have been placed "out of bounds" by some scholars. Such areas as "mind," "society," "history," and "values" have been excluded on the basis of arguments now to be examined. (See Anshen's *Science and Man*, and the recent *Scientific Man vs. Power Politics*, by Morgenthau. A position like my own is taken by Lundberg in *Can Science Save Us?*) One other problem, of whether such areas, though all within the realm of science, are yet themselves incommensurable and doomed to qualitative separation, will not be further pursued here. What has been said earlier indicates my conviction that continuity underlies discontinuity and that the lines separating biological from physical science or sociological from biological are only dotted lines.

MENTAL SCIENCE

Psychology cannot ever become a "science," it is argued, for these reasons: (1) the psychologist, in contrast to the physicist, for example, is internal to his subject; (2) values enter into his subject matter; (3) the phenomena he deals with are subjective; (4) they cannot be measured or quantified; and (5) he cannot avoid a personal bias in handling his material. These same points, especially 1, 2, and 5, are also urged in connection with social problems.

The question of internal and external has already been considered in general, as has that of the private and public avenues to knowledge. Man is indeed relatively more internal to his mind, directly apprehended, than to his body or to his culture, and far more so than to his inanimate surroundings. But it remains solely a matter of degree, in practice as well as principle. For, as often

emphasized, the observer enters into the physical observation as definitely as into the psychological; the doctor is part of the case he handles. Nor is the inwardness of mental phenomena, as such, a barrier to study; but rather the reverse. We understand far more of the properties of mind than we do of the brain mechanisms associated with them just because we are more internal to the mental than the physical aspect; because direct observation of our consciousness yields more fruitful clues than the indirect observation of our brains has yet done.

Another consequence of inwardness, urged especially in the sociological field, is that man necessarily alters the phenomena when he studies them. This, of course, is universally true—Heisenberg's indeterminacy is derived from the fact that the photon man uses to measure an electron's position has of necessity altered the position. It is also true that this influence is greater as the systems studied become more complex and closer to man. The physiologist must continuously guard against his experimental procedures making "abnormal" the performance of a body or organ; the physician is well aware of the factor of suggestibility which alters the action of drugs on a patient; and the psychiatrist may depend entirely on "psychotherapy" to relieve a patient's neurosis. But the psychiatrist nonetheless operates, within closer restrictions yet in a wider area, as does the psychologist producing or manipulating "neuroses" in animals, by turning to profit the ability of the experimenter to disturb the system under study. So, also, the fact that persons given bread pills improve in health or performance can be used to measure very subjective phenomena indeed. In an extensive study of the influence of vitamins on the performance of average factory workers, one group received no pills; a second group, a daily vitamin concentrate; and a third, a similar appearing but inert pill. Groups Two and Three believed they were getting vitamins, and the performance of both rose equally over some months, until almost a third better than Group One! Now, had these people not been bombarded by advertising about vitamins, Group Three (and actually Two, as well) would have shown but little change. I am certain one could use some such objective technique to measure quantitatively, if roughly, the effectiveness of advertising or other forms of promotion in arousing belief.

Conversely, even in the sociological field, the process or results of study do not necessarily alter events significantly. Surely the anthropologist, living a year in a primitive community, does not by his presence distort life there so as to invalidate his observations. Surely the actions of sociologists and economists affect their own society so slowly and feebly that their current observations are immune and even their future projections reasonably safe from such an error. The public opinion polls offer a clear case in point. By learning and revealing the state of public opinion ahead of time, it might certainly have led to a change in opinion when put to the realistic test of a vote. This has been much discussed, yet fairly convincing evidence now shows that no such "bandwagon" effect has, in fact, occurred.

The second issue, of values entering into the studies of man, requires little further comment. Values are shot through the biological sciences, and even through the physical sciences, only less than in those of man. Good and bad, perfect and imperfect, higher and lower, better and worse, useful and useless, successful and unsuccessful, beautiful and ugly, true and false, are common in the vocabulary of the natural sciences and are used in senses akin to, if not identical with, their use in man's affairs. Indeed, in one way at least, values may be more of a problem in the natural sciences because they have sometimes been smuggled in unwit-

tingly, in a form of anthropomorphizing, and are not recognized and handled in a clear fashion. In the human area the value problem at least cannot be suppressed or confused, whether or not it can be answered satisfactorily. But this will be considered later.

Third is the argument of subjectivity: mind, being subjective, cannot be studied by objective science; and, in social relations also, the subjective beliefs and motives, often more critical to any understanding than are the overt actions, remain inaccessible. This again turns out to be a matter of degree as to the facts and of some confusion as to the conception. For, as argued, there is a subjective element in every system, and we are only peculiarly happily situated in the case of man to exploit the clues presented from our own consciousness. Introspection, like any observation or exploration, yields grist for the mill of analysis. But the mental sciences, no more than the others, halt at the stage of the naturalist; and the further analysis is, for them also, at the objective level. It is commonly overlooked, despite the impact of behaviorism, that men judge one another—under study or not—entirely by objective evidence; by what they do, how they look, what they say. It is urged that behavior is no index to the "real" thoughts or feelings of a person; but this is, even now, an overstatement, for successful judgments of this sort are commonly made by sensitive laymen and regularly by expert psychiatrists. And as knowledge grows and techniques develop, the demonstrable correlation becomes ever finer.

The humorous result of testing a lunatic, who regularly claimed he was Napoleon, on the "lie detector" yielded powerful support for this position. Being asked during the test, "Are you Napoleon?" he craftily replied, "No." But the pulse and respiration changes registered a lie. Clearly, in his belief, he *was* Napoleon; and this belief could be objectively established. By study of the bodily concomitants of mental states, by noting behavior under selected conditions and with few alternate possibilities, by pitting one possible satisfaction goal against another, by the rapidly developing technique of attitude scales, personality trait tests, and the like; by all these means and many more certain to be devised, subjective states can be identified and quantified (still only roughly) from the objective aspect. Such evidence likewise refutes the fourth argument; that mental entities—beliefs, motives, desires, ideas, even dreams—are inherently nonquantifiable. They can be and are being measured.

Last is the problem of bias. This is surely a general human failing which manifests itself in all science as well as in all of life. Each man approaches a problem with his particular limited orientation and, having devised a solution which satisfies him, develops some parental affection for his hypothesis. But it is just the strength of science, as a cumulative human effort, that all possible orientations are finally brought into consideration and that individual emotion is countered by other individual emotion until all affect is rubbed from a rational core of common agreement. In proportion as the facts and relations are clearly and unequivocally demonstrable does this core of conviction as to the "truth" emerge easily and rapidly. Surely, then, "proof" is more simply attained in the physical sciences than in the mental or social ones. But this reflects the present state of development and not the impossibility of more. Even now, bias and emotionalism are turned to positive advantage in the psychological realm. The psychoanalyst, noting the regular appearance of an emotional "transference" by the patient to him and a lesser "countertransference" from him to the patient, has deliberately utilized this "bias"

situation to gain insight into, and to direct the cure of, the disturbed psyche of his patient.

SOCIAL SCIENCE

The arguments offered by those who would place social phenomena beyond reach of the scientific grasp include, besides those already considered, such ones as the following: (1) Causative factors are too numerous and complex to be handled. (2) Phenomena are so individualized and variable that it is impossible to classify them meaningfully, which is essential for the attack of science. (3) The objects of study, men, must give their active cooperation; which may not be secured or, if secured, may be colored. (4) Experimentation is impossible, because of moral or social restrictions, the nonrepetitive sweep of history, and the sheer mass and magnitude of the forces and events. These are mainly methodological problems and not so serious, even if valid, as the next group, which is ontological. Thus: (5) Man alone has a history known to the subject of that history. Man's past, therefore, can influence his future not only in the usual sense of a causal sequence, but also by guiding his conscious choices. (6) Man, as the historical argument also implies, is free and his behavior, therefore, indeterminate. (7) Man purposes, creates, and shifts his values and accordingly brings about unpredictable social change; for (8) the individual is prior to society and not the reverse.

So far as the first group of objections is concerned, it is surely obvious that they carry weight only in degree, not kind. Every one of them has been urged in the same vein in the past to prove that there could never be a science of biology. If the position were: social phenomena, being especially complex, individualized, and hard to manipulate, must remain for a longer period, or even indefinitely, less fully analyzed, and so less predictable, than those of the physical world; and that a body of knowledge cannot be called a science until it has achieved some minimum of these desiderata; then the only argument would be about the position of the dividing line and when each aspect of social study is judged to have crossed it. But to argue on these grounds that sociological problems are extrascientific is nonsense.

We have come to accept, by reiteration, that the social is more complex than the biological; but I sometimes wonder if the reverse is not the case. The cell population of a single human brain is four or five times the total human population of the world. These cells are in direct or indirect, and reciprocal, "communication" with one another to a far greater degree than are men—it has been estimated that the permutations of their connections are greater than the total of particles in the known universe. Each cell has its individual role, partly irreplaceable, in the whole, its own quantitative, and often qualitative, irritability pattern, its bizarre structure as a variable unit in one of hundreds of distinctive groups. In number and nexus, if not nuance, relationships in the brain are far more complex than in a social group. But who would try to tell the neuroanatomist or physiologist that his study of the brain is extrascientific or his progress in understanding is illusory? The vast range between a micrococcus and the mammalian brain (let alone the whole mammal), a range perhaps greater than from a salt crystal to the microbe,* is yet effectively bridged by the same basic concepts and by like methods. Is the chasm from solitary to social man as great?

Chemists have found or made over a million separate kinds of molecules of pure substances; and entomologists have identified a similar number of species of insects.

* In sheer mass, a sulphur-bottom whale is as large compared to an influenza virus body as is a large drop of water compared to a single water molecule.

These various individual classes have nonetheless been grouped, analyzed, accounted for, understood to moderate or high degree by scientific procedures. Is the variability greater of a few hundred men in a tribe or village or a few million in a nation? Of course, the men are units in a single class; but so also are the insects and molecules, and, conversely, each man is also a class of organs and cells. And these human units do not vary over a wider range in mental or social attributes than they do in physical—if the studies of psychologists, psychiatrists, and social anthropologists mean anything, these are perfectly measurable and classifiable. Would "Middletown" be of significance to anyone, except residents and their friends, if it were not representative of a class? Would even "Main Street" interest us if we didn't recognize the class of Babbitts?

Nor is the need of obtaining cooperation of subjects either unique or entirely disadvantageous in the social field. Just as the observer enters into all observations, so does the "submission" of the observed. If a substance "refuses" to melt sharply but chars instead, its melting point cannot be determined. Or, more clearly comparable, a dog must cooperate even when being given an injection, at least to the extent of not biting or struggling. He can, of course, be compelled, by force or fear, which might suffice for injecting a narcotic; but if studies on the sentient animal are at issue, the cooperation must be positive. Only an animal gently and carefully trained to lie quietly can be used for basal metabolism studies, for full relaxation is essential. A similarly trained dog, willing moreover to swallow a balloon and tube, is needed to observe stomach movements, for any emotional arousal stops them dead. And animals used in conditioned-reflex studies are "cases" almost in the sense that are humans: their full history is desirable, their daily life is noted and controlled, they are elaborately educated, they often are incorporated into the family of the investigator, they may react differently in the presence of different experimenters, and so on. They cooperate at somewhat the same level as does a child in school or taking a mental test; or as an adult after once volunteering for some experiment. Actually, since a human more readily understands and *can* cooperate, some conditioned-reflex studies are made with greater ease and certainty on man.

The same general incentives to cooperation, those of reward or punishment, are active with man and animals and have been shown to operate in the same manner. At the simple level of food versus shock, conditioning proceeds alike with man or dog, and at the more abstract level of giving praise or blame (by word and intonation)—indicating, that is, value judgments—training again follows parallel courses. Conversely, humans may be exposed to desired conditions, or observed under them, with no active cooperation on their part. It is a similar objective problem in each case to note which individuals sicken during an influenza epidemic, which blush at a taboo word, which choose green ink for their correspondence, which vote at local elections, or go to church or belong to the CIO, which falsified their income tax returns, even which say they believe in God or internationalism or free trade. It is little different when a children's play group is observed through one-way mirrors and with microphones. And just because man can react, by act and word, in finer detail than can animals, his cooperation as a subject is mostly an advantage, not a handicap.

Experiments in the sociological field can be, and are being, performed, though the techniques are only now being developed. It is a sociobiological experiment when a trace of fluorine is added to the drinking water of one small town over a period of years, while

a neighboring one is kept as control, and the amount of tooth decay in both communities noted. It is pure sociological experimentation when diet education is given in the grade schools of some sectors of a backward community and not in others, and the change or absence of change in food habits noted. So also for studies on racial cooperation in collective farming groups in the "white-trash" zone which, like the above cases, can be carried out with sufficient numbers of experimental and control units to be reasonably decisive for the factor under study. Even a single experiment like TVA, while not entirely convincing until like findings are obtained in additional instances, may carry a high probability of significance. Experiments have amply shown that youth groups flourish, in all ways measured, under a democratic leader as compared with an autocratic one. Evaluation studies underway should objectively demonstrate strengths and weaknesses of alternate education procedures. Market analyses, radio listener ratings, promotion studies, opinion polls, are all techniques for measurement which, combined with experimental manipulation of the product or program or advertising or publicity, are yielding reliable and predictive generalizations, which will in time transcend their applied focus, in the area of human affairs.

The ontological arguments have been met in the general considerations presented earlier, but a few further comments are desirable here. Assuming, as above, some ultimate contingency in nature and also the inner-outer view of mental and material, there still remains neither a basis for regarding man's will as an independent active agent nor any fulcrum on which a mental state can lever a physical one. And the degree of indeterminacy of any system over a time span commensurate with its duration is minute at most. These points have much bearing on the problems of human history; of whether history, being known, diverts the future unfolding of human actions and their social residues, and whether historical study can itself be included in science as "scientific history."

The argument as to whether history can be a science has raged for a century. Frank, in his recent *Fate and Freedom*, emphatically decides not; on the basis of the fugitive character of what "really" happened, the selective bias of the historian, the accidents of men and events which laugh at predictive rules, the inherent evitableness rather than inevitableness of human affairs, and all the other general objections presented in the sociological area. These are considered more fully in last year's *Ethics*, but it deserves note that the points here mentioned all apply with equal or greater force to archeology and paleontology and to much of ethnology and social anthropology—all universally accepted as in the scientific fold. The whole evolutionary past of the world of life is, of course, a historical study, a description of what was, with all the dangers of vanished evidence, theoretical blinders, the riot of chance, and fuzz of contingency; but, also, following description came analysis of guiding factors and forces which, today, are being resolved into quantitative formulae (Wright) and are yielding verifiable predictions. The role of new genes in biological evolution and of new ideas—mutations both—in social evolution is comparable even though the mechanisms are different enough to operate respectively over aeons or decades.

Conversely, the fact that man knows or can know his history, like the comparable facts that he knows his present mind, individual or collective, and his future aims, does not vitiate the degree of determinacy that exists in the cosmos. Man's mind (and brain) evolved without a "purposing" by man; it made possible a complex society, a knowable past, and an envisioned future; it is the internal aspect of reality and does

not independently operate upon the external aspect of the same reality. The significant problem is that of interaction of part and whole, of organism and epiorganism (as of cell and organism in biology); and of those regular temporal sequences in state of all orgs (systems) in which we describe mechanism and see cause or purpose.

Man's conscious purposing, on this view, is part of the scheme of cosmic change. It is our internal view of the succession of events, looking downstream in time rather than up. Purpose, even retaining its anthropomorphized connotation of the purposer as a free and independent mover, is constantly referred to other men and to most animals. But the same objective picture remains when any conscious aspect is ignored and systems are viewed externally; we see purpose or teleology when the behavior of an org is more intelligible to us in the light of its future states than of its past ones. This is sometimes the case for inanimate systems, as when "water *seeks* the lowest level;" it is commonly true for animate ones. The organism, as unit or as species, continuously solves problems; and it changes its "values" of a good solution. All evolution is a succession of solved problems—of successful reproduction, of capturing food, of rapid movement, of survival in dry or cold surroundings, etc.; and of the secondary and tertiary problems which arise in meeting the primary one—and the relative "goodness" of solutions changes with environmental conditions. A species that has been beautifully adapted to, and successful in, one environment is quickly branded unfit when this changes; as the exiled St. Petersburg aristocracy was in Paris.

All structural development and physiological activity of individuals is a similar succession of solved problems, with shifting values. Blood is shunted to the muscles when one runs, to the gut during digestion; to the skin "to nourish" it, yet from the skin, which may be sacrificed, "to conserve" body heat. A hand is jerked reflexly from a hot bar "to avoid" injury, but it will grasp the same object reflexly "to maintain" balance. Almost always the physiologist first sees meaning in phenomena in terms of such purposes; as analysis proceeds he discovers new phenomena which enable him to think upstream in time, in terms of mechanisms. There is no reason to view man's purposing differently, it is only more difficult to emerge from the immediate internal view of it.

And so for the argument that society is merely the outcome of many individual ends. Part and whole act on each other reciprocally—such coeval, rather than sequential, interaction is part of the very character of orgs—and neither is truly prior to the other. The several aims of organs as parts of a man, of a single person as an entity, of men as units in a society, of a single social epiorganism, are in continued interplay. There is at each level and between levels conflicting and compatible purposes, competition and cooperation. Only while the centripetal forces exceed the centrifugal ones does the org continue as such; otherwise the liquid evaporates, the body dies, the group disintegrates. Infants form no group, several together go their separate ways which may cross in conflict but not in joint play. With development, cooperation increases, absolutely and relative to conflict. In evolution, the early stages of many types are solitary, the more developed ones social. And man's history likewise is a record of org evolution toward cooperation of the parts and integration of the whole.

OF COURSE, what has been said of purpose applies to values. I see no reason why value problems are extrascientific. Psychologists study animal learning and actions in terms of "goal-directed" behavior, biologists recognize values in body organization, function, and evolution, psychiatrists expose

unconscious drives which underlie conscious purposes. All see ends and seek mechanisms. All find, with increasing success, factors in the organization, history, and environment of individuals or types which account for their particular values; all can trace values, more or less precisely, even quantitatively, back to biologic "needs" and forward to predicted actions. As Perry has argued, values are facts. Objects of value are objects of interest, and interests of value are interests in objects (whether of present or future or imagined existence), so that no real separation exists here between objective and subjective values. And knowledge concerning the relations of nonexistential goals, ideals, and the like is in the same category of knowledge as mathematics, which also deals with the relations of nonexistent entities.

One stock objection to a science of values is that "ought" is beyond science. Yet this word, with all except the volitional connotation, is continuously used outside the sector of human actions. And, more weighty, the very notion of "ought," like that of "must" or "true" or "cause," is implicit in the functioning of brain and mind. The whole process of conditioning shows, in animals as clearly as in man, the unverbalized and unthought axiom: past regularity *ought* to extend into the future.

Another objection is that reason cannot be the ultimate standard, for it can ask the question whether it is ultimate. To the extent that this has any real meaning, two answers are relevant. First, that if science and reason must forswear absolute certainty, even more so must belief, faith, and revelation. Second, and regularly overlooked, the collective mind of man (or the mind of collective man, of the epiorganism) transcends the mind of individual man. The cumulative knowledge, imagination, and critical reason of mankind is of a different order than that of the ablest person. Mankind's wisdom is surely finite when projected against the teeming universe, but no less is it infinite as measured against a single man. And, in the same vein: as man's science, his accumulated and organized and tested knowledge, leads him to state evermore-sweeping generalizations, with ever more confidence in their enduring validity, about what the world is like and in which way it is moving, he approaches closer to an empirical answer to the grand problems of philosophy. Certainty of absolute values we shall never have, but certainly science will supply values founded on an immense knowledge of the universe, wide in space and deep in time, values more weighty to all men than those imposed by a particular period and locale on some of them.

ACID CLAY—A WEATHERING AGENT

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THE minerals which serve the most critical needs of man and which merit a rank of most fundamental importance to the human race even in this atomic age are not the ores of iron and gold, nor coal, platinum, uranium, or radium, but the clays. Clay minerals, those colloidal, molecular, microscopically elusive, versatile, and adaptable, but yet crystalline, plates and needles which act as "jobbers" between the inorganic, lifeless rocks and the life-possessing and life-sustaining food plants, become the foundation stones that support the entire vital structure culminating (that is, "culminating" from man's viewpoint) in man. If driven to the extremity of Adam or Pithecanthropus, we could do without other minerals but we would need clays to grow the food and fuel to sustain the race, hydroponics notwithstanding.

Because of our dependence on clays and because they have been with us since before "Adam was formed of the dust of the ground," it is reasonable to expect that researches on them would have long been so thorough and exhaustive that little of major importance would remain to be discovered about their nature and origin. Quite the contrary is the case, however. Only a little while before World War II, a new agent in the chemical weathering of rocks was described by Dr. E. R. Graham, of the Soils Department of the University of Missouri, who recognized that clay minerals themselves might play an important role in decomposing the primary silicates into more clay minerals, and so on and on. Prior to this time the action of ground water with carbon dioxide, oxygen, and various indefinite humic acids was held chiefly responsible for the alteration of silicates like the feldspars, amphiboles, and pyroxenes into clay. It is interesting that Dr. Graham's contribution stemmed from activities in two scientific fields. By means of an alert imagination, he coupled a rich background of soil science with a geological perspective to erect the concept and then proved experimentally that acid clay functions as an active aggressive agent in the weathering of rock minerals.

Fundamentally, the surficial decomposition of most silicates like the feldspars and mafic silicates is a result of hydrolysis of the aluminosilicates. The primary minerals, exemplified by orthoclase, a potassium-aluminosilicate, are salts of weak aluminosilicic acid combined with strong base-forming elements such as potassium, sodium, calcium, or magnesium. When the aluminosilicates are wetted, hydrolytic breakdown occurs, the pH of the solution rises, mobile alkali dissolves, and a hydrous aluminosilicate, relatively insoluble, remains. Obviously, a removal of the basic alkali or an introduction of acid into the system will upset equilibrium and promote more hydrolytic decomposition of the primary silicate and the formation of more new clay. Therefore, the carbonic acid arising from the solution of carbon dioxide derived from the air or the soil atmosphere, and the organic acids from plants and plant residues, act to increase hydrolysis.

Likewise, any clay having acid properties, like that from an acid soil, reacts with the products of hydrolysis to drive the reaction toward the decomposition side. Consider the magnitude of acid soil clay available right within the critical locale of rock weathering, and one can appreciate the significance of this agent. Immeasurably large areas of primary minerals and rocks, exposed as surfaces of innumerable silt- and

sand-sized particles occurring in the soil, subsoil, and mantle rock to the wide surface of underlying solid rock, are susceptible to the gentle but ever-pressing corrosive attack of acid clay, which may coat the primary material most intimately.

A single neutralization of the once acid clay is not the end of its action. The base-forming cation (potassium, sodium, calcium, or magnesium) is held only loosely to moderately tenaciously by the clay's space lattice and may be removed by repeated ground-water leaching with simultaneous replacement by a hydrogen ion. The further this replacement proceeds, the more acid the clay again becomes and the more aggressive it is toward further weathering.

Of still greater significance is the removal of the metallic cation by plants utilizing the mineral nutrient in their growth. Plant nutrition (of mineral nutrients), as we know it today, involves predominantly the extraction of mineral nutrients loosely held by clay minerals and organic matter. The more vigorous the growing crop is, the more quickly calcium, potassium, etc., will be taken from the clay and hydrogen substituted for it, and the more quickly it will become acid. In consequence, the recurring acidity of the clay rearms it with power to steal away calcium, potassium, etc., from more primary silt minerals by decomposing them and releasing more new clay. That is one of nature's ways of attempting to restore the mineral nutrient balance in the inorganic-organic cycle. Indeed, the clay minerals are "go-betweens" for the inorganic and organic worlds.

Graham found (1939) that within 107 days an acid clay colloid having originally a pH of 3.3 reacted with silt-sized anorthite (a calcium aluminosilicate feldspar containing probably a little sodium) particles to raise the pH of the solution to 5.7. Moreover, within those 107 days, 3.4 percent of the total calcium of the anorthite had been taken into solution. By extrapolating those data, a somewhat speculative extension, we might expect silt particles of anorthite to be competely (100 percent) weathered in a highly acid soil within 10 years. Obviously, other modifying factors, such as temperature, leaching by rainfall, plant growth, and activity of carbonic and humic acids, would affect the rate of decomposition, so the figure of 10 years, with the implication of anorthite impoverishment of soil in 10 years, must not be taken too rigorously. It does indicate, however, an approach to one line of attack on the formation and replenishment of soil and on rock weathering.

The element iron, which is present in the amphiboles and pyroxenes, was left out of the above discussion because it usually oxidizes and hydrates, probably more or less independently of silicate hydrolysis, in the weathering environment. Where the primary mineral is essentially an iron or iron-magnesium silicate poor in aluminum, the acid clay will probably be as corrosive as with an aluminosilicate.

As a weathering agent the unobtrusive acid clay may be likened to a bloodsucking, sapping, parasitic guest which eventually fully coats and encloses its rock or mineral host. Slowly, but with almost irresistible sureness, this parasitic agent pulls out from the host its metallic atoms which link its silicate sheets and hands over those elements to plant roots, leaching water, or other chemical solvents. The predatory guest effects insidious alteration, with release of energy, of the host mineral to clay of its own family and continues to bore in. Acid clay is indeed a powerful weathering agent that has worked in a hidden fashion for a long time. We have reason to be thankful that acid clay has been restocking the soil and mantle rock with its own kind of mineral through long geologic time, and that it has been a medium of transfer of mineral nutrients from stones to wheat and corn and bread and meat.

SCIENTISTS AND THE CIVIL SERVICE*

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THE recruitment and retention in the federal service of the nation's best-qualified scientists is one of the government's most serious problems. Complex and tremendously significant duties and responsibilities have been given to federal scientists, and the welfare of the nation, if not the welfare of the whole world, calls for the discharge of these duties by persons of the highest competence. The federal government is aware of this fact, and, as a result, it is engaged in the development of a program designed to attract to, and retain in, the federal service our very best scientists.

As the central personnel agency of the federal government, the Civil Service Commission must, of course, assume a position of leadership in the development of such a program. This we are doing. We are doing it, however, with the active day-by-day assistance of an Advisory Committee on Scientific Personnel. In view of the important role which this committee has played in the development of our program to date, and in view of the important role it is sure to play in its further development we desire at the outset to identify the committee and to pay tribute to the job which it has done and is doing.

From the beginning, the Chairman of this committee has been Dr. M. H. Trytten, who is the Director of the Office of Scientific Personnel of the National Research Council. Dr. Trytten's leadership had done more than anything else to insure the effective functioning of this Advisory Committee. Associated with Dr. Trytten as members of the committee are Dr. Thomas B. Nolan, Assistant Director of the Geological Survey, Department of the Interior; Dr. Edward U. Condon, Director of the National Bureau of Standards, Department of Commerce; Dr. S. B. Fracker, Assistant Director, Agricultural Research Administration, Department of Agriculture; Captain Ralph D. Bennett, Technical Director, Naval Ordnance Laboratory, Navy Department; Dr. Lucius F. Badger, Assistant Director, National Institute of Health, Federal Security Agency; and Dr. Kenneth L. Heaton, Office of Director of Civilian Personnel, War Department.

Now let us take a look at the program itself. Prior to World War II, recruiting for the federal government was a highly centralized operation. This was particularly true of recruiting for professional and scientific positions. A considerable portion of the work involved was centralized in the Civil Service Commission. As far as the Commission was concerned, a large part of the operation was centralized in Washington. Generally speaking, there was a conviction shared by people both in and out of government that this was the only way in which a system of open competition could be effectively administered.

The reasons for such a belief were clear to all concerned. The civil service system was brought into being in order to fight a .spoils system. A reluctant Congress brought it into existence because of an insistent demand upon the part of the public. A hostile group of operating officials was determined to prove that a merit system just could not work. Consequently, surrounded as they were on all sides by enemies of the merit system, the first

* From a speech before the Annual Meeting, Middle States Science Teachers Association, New York, November 30, 1946.

civil service commissioners decided that it was imperative to keep a close check on the handling of each individual case. Centralized controls, in their judgment, were absolutely essential to the survival of anything resembling a merit system. Unquestionably, this nation is indebted to those who held in a determined manner to such a concept. They are the persons who are responsible for the deep roots which the merit system now has in the federal government. They are the persons who are responsible for the fact that some of the nation's most outstanding scientists have been able to work, year in and year out, in the federal government's laboratories, irrespective of their political affiliations.

With the advent of World War II, it became clear that centralization just would not work. The reasons for such a conviction were obvious. Centralization resulted in delays. Delays of months and even years in establishing lists of eligibles were cited in support of this point of view. And many times, even after long delays, it was felt that the best-qualified available personnel had not been secured. Also, centralization of recruiting kept those, for example, who were responsible for supervising the government's scientific projects from playing an active role in the selection of their personnel. In other words, centralization violated a basic principle of sound management.

Throughout the war period, recruitment for the federal service, particularly in the professional and scientific fields, was characterized by a high degree of decentralization. The Civil Service Commission continued to assume responsibility for the establishment of qualification standards. It also assumed responsibility for seeing to it that there was adherence to these standards. But the departments and agencies, in many instances, assumed direct responsibility, with the cooperation of the Civil Service Commission, for the recruitment of their own personnel. As a result, there was a genuine pooling of the resources of the federal government.

With the war over, the Commission was faced with the necessity of determining to what extent it was going to continue to operate under a policy of decentralization, and to what extent it was going to return to a policy of centralized controls. The decision has been made. And the effect of that decision is to carry decentralization even further than it was carried during the war.

Here is how this decision is being carried out in the recruitment field, with particular reference to recruitment for professional and scientific positions:

1. The departments and agencies of the federal government have been invited to establish United States Civil Service Boards of Examiners within their departments.
2. These boards are to be made up primarily of top operating officials and outstanding specialists in subject-matter fields. Nominations for membership on the boards must be approved by the Civil Service Commission.
3. The boards are to be established both in Washington and in the field service.
4. These boards are to develop, subject to the approval of the Commission, minimum qualification requirements for positions for which they hold examinations.
5. Also, these boards, in cooperation with the Commission, are to conduct active recruiting programs designed to attract persons of outstanding qualifications.
6. The members of the boards are to be responsible for rating the applications received for examinations, and are likewise to be responsible for establishing eligible lists and certifying names from these lists for the filling of vacancies. These actions will all be subject to review by the Commission.
7. Boards of Examiners will hold examinations for positions which are peculiar to a department or agency. Where a position is peculiar to a comparatively few agencies, a joint Board of Examiners will be established. Where a position is common to all departments and agencies, the examination will ordinarily be conducted through the Civil Service Commission's own offices. Even under such circumstances, however, if the department or agency is prepared to carry forward a highly specialized recruiting

program of its own, the Commission will usually, in the professional and scientific categories, be willing to have such a program handled by a Board of Examiners.

This is not a paper program. These boards are already in the process of being established. By January 1, 1947, over 750 of the boards were in actual operation.

During the fiscal year ending June 30, 1947, we expect to make approximately 600,000 regular civil service appointments in all occupational categories. Over 300,000 of these appointments will be made from lists of eligibles established by these Boards of Examiners.

The figures in the professional and scientific fields are even more significant. In strictly professional and scientific jobs the federal government will be called upon to make over 33,000 regular civil service appointments during the same fiscal year. If we can find these 33,000, over 22,000 will be made from lists of eligibles established by Boards of Examiners made up of outstanding professional and scientific men in the various departments and agencies.

In operating under this policy of decentralization, the Civil Service Commission is not divesting itself of responsibility for what happens in the field of recruitment. This we could not do under the law. This we could not do and preserve a genuine civil service system. We are saying, however, that we shall discharge our responsibility by (*a*) setting standards, (*b*) delegating authority to others to act within those standards, and (*c*) checking up from time to time to make sure that there is adherence to standards.

On the whole, departments and agencies are following a similar policy. During the war, they delegated authority to act on personnel matters to their field establishments. They found that it worked. Consequently, in most instances, they are continuing this policy of decentralization. In this manner, both the Civil Service Commission and the departments and agencies are making it possible for professional and scientific personnel to participate directly in recruiting activities. In this way, also, the government's recruiting machinery is being greatly speeded up.

All this has definite implications from the standpoint of recruiting for the public service some of the best scientific students in our institutions of learning. It means, for example, that the responsibility for keeping in touch with the colleges and universities as the source of supply for professional and scientific positions in the federal service will be a joint responsibility resting upon the Commission and the departments and agencies. It means that, in addition to personnel officials, those in charge of our scientific laboratories will also take a definite interest in making appropriate contacts with colleges and universities. They will manifest this interest because they themselves will be playing a definite part in recruitment activities. It also means that there will be increasing emphasis on contacts between the field offices of the Commission and operating agencies and the colleges and universities.

That all this is not idle speculation is shown by a very recent development at the grass-roots. The Civil Service Commission has a regional office at Denver, Colo., which serves federal establishments in the states of Colorado, Utah, Wyoming, and New Mexico. In that region there has been organized a College-Federal Service Council. Operating heads of the principal federal agencies in the Denver region and administrative officers of colleges and universities located in that region are members of the council. The council has now provided for the establishment of committees on the social sciences, physical sciences, and biological sciences. These committees are made up of representatives both of federal agencies and of colleges and universities.

This College-Federal Service Council will work on the problem of providing a better flow of qualified persons from the colleges and universities into the federal service and will likewise work on the problem of making the facilities of the colleges and universities available for the in-service training of persons already employed by the federal government.

The committee on the physical sciences is now obtaining factual information from college professors, agency heads, personnel officers, and federal employees which it intends to use as a basis for findings on the requirements and policies of the federal service with respect to collegiate training, recruitment, in-service training, and advancement of personnel.

The Commission has followed this project with great interest. It is convinced of its worth-whileness, and as a result, similar developments will take place in all our regional divisions. In addition, steps will undoubtedly be taken in the direction of setting up a similar group at the national level.

In the development of this program for the utilization of Boards of Examiners, particularly in the professional and scientific fields, we have had the benefit of the continuing assistance of our Advisory Committee on Scientific Personnel.

The Civil Service Commission realizes, of course, the importance of qualification standards in the development of an effective recruitment program. If our standards are high, it helps to attract outstanding persons. If they are low, such persons show very little interest in the possibilities of federal employment. Our Advisory Committee is likewise very much interested in this phase of the problem.

The Commission is at present engaged in a project which is sure to have far-reaching implications as far as this question of standards is concerned. We are in the process of developing what we refer to as classification standards. These standards are written statements outlining the duties and responsibilities of various classes of jobs and indicating the series and grades to which these various classes should be assigned.

These statements are very important from a salary point of view. This is true because the grade in which a particular position is placed determines the salary the incumbent of the position is to receive. In addition, these standards contain a statement of the knowledge and abilities required to perform the duties of particular classes of positions. They are not statements of the number or types of courses which must have been completed in order to qualify for such positions. They are statements of the knowledge and abilities which a person must have to perform the duties of the position, irrespective of how he may have acquired such knowledge and developed such abilities.

The relationship between such fundamental statements and the development of courses of study in the scientific field is, of course, obvious. With such statements in front of them, the members of the physical and biological committees of the College-Federal Service Council in Denver, for example, will be able to develop a much more realistic program than they could without such statements.

When the Advisory Committee on Scientific Personnel was acquainted with this program, it recognized at once its importance from a long-term point of view. Consequently, members of the committee are participating actively in the development of the standards for scientific positions. We feel confident that with their help we shall be able to develop standards that will attract our best minds to the federal service.

And so, in a variety of ways, the federal government is putting itself in a position

where it can carry forward a more effective recruitment program in the scientific field. This alone, however, will not solve this problem of attracting to, and holding in, the federal service the best-qualified scientists the nation can produce. Conditions surrounding employment within the federal career service must be of such a nature as to appeal to men and women who have decided to devote their lives to scientific pursuits.

Considerable progress has been made in this direction. Our salary schedules in the middle and upper brackets compare very favorably with those in colleges and universities, as well as with those in industry. The working conditions surrounding government employment more and more approximate those of the most progressive employers in the nation. Government today is on a five-day week. Federal employees are entitled to more than five calendar weeks of annual leave each year. Sick-leave provisions are liberal. In addition, the federal government has developed a retirement system which, from the standpoint of professional and scientific workers, is an attractive one and again compares very favorably with retirement systems outside the government.

Recognizing that progress has been made, we of course realize that much more remains to be done. Here, for example, are a few things that we feel must be done in the months which lie immediately ahead if we are to attract to, and hold in, the federal service first-rate men and women.

1. We must pay higher salaries to our top professional, scientific, and administrative personnel. Our present ceiling of $10,000 on salaries for the career service should be lifted to at least $15,000.
2. We must provide better opportunities for continued growth to those who are engaged in professional, scientific, and technical work. In-service training and leaves of absence for advanced study, as well as opportunities to observe how problems are handled outside of government, should all be included in such a program.
3. We must develop programs for promoting career servants which will minimize the possibility of such persons finding themselves in dead-end jobs.
4. We must do much more than we have done up to the present time in the development of administrators who are skilled in the handling of professional and scientific projects and who know how to supervise those who have dedicated their lives to the pursuit of truth in the scientific field.

Up to now, we have been talking about things that the federal government is doing, and the things which the federal government recognizes it must do, if it is to attract and hold in the federal service our best-qualified scientists. But if this goal is going to be reached, we must receive a great deal more help and assistance from the faculties of our institutions of learning than we have received up to the present time. Generally speaking, students of science have a woeful lack of understanding as to the manner in which the federal government, as an institution, actually functions. They do not know what the opportunities are for the carrying forward of scientific work in the federal government. They do not know what policies the federal government, as an employer, actually pursues.

There is no excuse for this kind of situation. Whether these students of the sciences are to become government employees, or whether they are to go back to their communities and assume their responsibilities as citizens, it is incumbent upon us to give them, while they are students in our institutions of learning, a much better understanding of our federal government. If we do, some of the best will decide to become career public servants.

Those who do not will go back to their communities and provide information as to the progress that has been made in the development of a career public service

in the federal government. They will also know what needs to be done in order to strengthen it. In other words, they will help to place back of the career public service an informed public opinion without which it is impossible to attract to, and hold in, the public service our best-qualified citizens.

We are going to hear more and more about the federal government's needs in the scientific field. We are going to have more and more contact with the top scientists in our federal scientific laboratories. Let us approach our problems with an open mind and become intelligent citizens as far as our government is concerned. Insist that the students in our schools likewise become intelligent citizens as far as the government is concerned. Then, if we who are a part of the public service will keep working at the job of making government an increasingly attractive employer for scientists, together we can make sure that our nation's laboratories are supplied with scientists who are second to none.

BUTTERFLIES

Gauze-winged, they flutter, one by one,
These bright ambassadors of sun,
Brushing past each leafy bower,
Appraising sweetness of the flower.

What radar guides their aimless flight
Through devious avenues of light
Until unerringly they rest
Upon the clover's nectared breast?

MAE WINKLER GOODMAN

PERCEPTUAL RESEARCH AND METHODS OF LEARNING*

By G. T. BUSWELL

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RESEARCH in visual perception has depended largely on two sources for data. One of these sources has been the studies which have used various types of tachistoscopic apparatus; the other has been studies of eye movements using photographic or other kinds of recording techniques. My purpose is to discuss the applicability of this type of research to methods of learning, with particular emphasis on learning to read.

The literature of psychology includes a long series of tachistoscopic studies, ranging from those using such simple devices as flash cards to those employing precision tachistoscopes by which duration of exposure can be controlled to a thousandth of a second, and dealing with various kinds of subject matter such as numbers, geometric forms, and verbal materials. The purpose of these experiments has been to improve accuracy and scope of perception and to reduce perception time. Recent application to military problems has focused increased attention on this kind of research. Renshaw's work, reported in the June 1945 issue of the *Journal of Psychology* and in other places, has become widely known. Less spectacular, but ranging over a longer period of time, have been the experiments aiming at an improvement of the reading process by use of this method. The flash card, which is in essence a poorly controlled tachistoscope, has long been used as an aid in learning to read; variants of the tachistoscope, such as the American Optical Company's Metronoscope, the kinetoscopic apparatus used with the Harvard reading films, and the film-projection apparatus used in the University of Chicago reading laboratories, have been employed in clinical work in reading at all levels of maturity.

Paralleling this use of tachistoscopic methods has been the development of techniques for studying perception by the use of various devices for recording eye movements. These range from the first crude attempts of Javal to secure records on a smoked drum by means of a mechanical lever attached to the cornea of the eye to more elaborate types of apparatus for photographing eye movements developed particularly by Dodge, Judd, and Dearborn. More recently eye-movement data have been secured by electrical recording of muscle currents. These eye-movement studies have dealt with a wide range of problems involving the perception of numbers, verbal materials, and pictures. They have provided data on continuous perceptual behavior as contrasted with the discrete, separate responses recorded by the tachistoscope.

The implications of tachistoscopic research and eye-movement research are understood better when considered together. When considered alone extravagant hypotheses can be drawn. For example, it is easily possible to learn to recognize words in a tachistoscopic exposure of one-hundredth of a second. However, data from eye-movement studies show that the average duration of fixations of the eyes in reading, even for adults, is approximately one-fourth of a second. If the easily attainable tachistoscopic rate of a one-hundredth-second exposure could be maintained in reading, simple multiplication would give a speed of 6,000 words per minute. No one has ever

* From an address before Sections I and Q, A.A.A.S., Boston, December 28, 1946.

proposed that this is attainable, nor am I implying that psychologists employing tachistoscopic methods hold any such beliefs. However, in view of the statement, supported by evidence, that tachistoscopic training results in improvement in reading, there is some necessity for understanding the nature and the range of such improvement.

Obviously, perceptual data from the tachistoscope and the eye-movement camera are of two kinds. The tachistoscope measures the time required for a single recognition. It does not follow that another word could be exposed and recognized in the next one-hundredth second, and still another in the following one. Recognizing *a* word in one-hundredth of a second does not mean that *words* can be recognized at a rate of 100 words *per* second. An experimental attempt to do this with a kinetoscope projector would at once illustrate the difficulty. Reading involves the continuous recognition of consecutive verbal symbols with the subsequent fusion into units of meaning. It must be, therefore, that improvement resulting from tachistoscopic training is to be explained in the changed character of the perceptual experience, that is, centrally rather than peripherally. Renshaw recognizes this point of view in his emphasis on "field structuring." He summarizes his position as follows:

> In tachistoscopic work the exposure time/length of material relation is a function of grouping and field-organization in perception. Concepts such as the "span of attention" or "span of visual apprehension" must be specified in terms of the developmental level of skill of the perceiver. The limits of improvement from training by tachistoscopic methods are undetermined. The control of motivation and the active restructuring of the visual field by the perceiver seem to set the limits of improvement through training (*J. Psych.*, 1945, **20**, 230).

The essential characteristic of the tachistoscopic method is that it provides a means of controlling a single perceptual experience; that is, it so limits the time of exposure that not more than one fixation of the eye is possible. Precision tachistoscopes are superior to flash cards for this reason. As flash cards are frequently used, the duration of exposure is long enough for two or more eye fixations. When movements of the eye are thus possible, the total perceptual experience may be a series of impressions, in which case what is seen on the second and later fixations may be added to what is seen on the first. This is a crucial distinction. It is a summation of stimuli or elements as contrasted with a single "structured" or integrated pattern that is observed. The distinction is well illustrated by the methods of aircraft identification used during the war. The original WEFT system (wings, engine, fusilage, tail) represented a series of rapid impressions of each part. The later practice in identification was based on the assumption that a single impression would give the plane "as a whole." The tachistoscopic practice of flashing the image of an airplane on a screen was carried out at a rate of exposure which guaranteed that perception actually was limited to a single fixation of the eyes.

This article is not concerned with military applications of perceptual research but rather with its use in learning in school and particularly in the case of learning to read or to improve one's ability to read. Here the situation, operationally, is quite different from aircraft identification. The process of reading involves a continuous series of perceptual experiences, and the scope of each perceptual unit (the words or letters perceived in a single eye fixation) varies according to the limitations of the central process of recognition and interpretation. The chief values to be gained from tachistoscopic exercises would be in (*a*) reducing the duration of the fixation pauses or (*b*) widening the span of recognition; that is, increasing the number of words recognized in a single eye fixation. It is unfortunate that the two principal experiments in im-

proving reading by tachistoscopic training, namely, Renshaw's experiment in Gary, Ind., and MacLatchy's experiment in Bexley, Ohio, did not include records of eye movements in their data. Such records would have furnished objective and precise measures of the nature and amount of perceptual changes that accompanied the reported improvement in reading.

Two principal factors have been reported that influence perception in reading, and these have constituted the major basis for changes in methodology. One of these has to do with the perceptual organization of the material being read, and the other with the degree of freedom from vocalization in the reading process.

The history of methods of teaching reading shows a striking parallel between prevailing psychological beliefs regarding the nature of perception and the practices used in the schools. The *New England Primer*, widely used in early American schools, was constructed in harmony with the view that perception was a summation process. This book began by teaching the letters of the alphabet, both small letters and capitals. This was followed immediately by teaching combinations of two and three letters. In some cases these letters made words, but in others only meaningless combinations. Next, words were introduced, first those of one syllable and then in succession those with two, three, four, and five syllables. Continuous reading material was introduced following the five-syllable words.

The theory supporting the *New England Primer* was that letters were the units of perception and that the normal sequence was from letters to syllables, then from small to larger words, and finally to continuous reading material. Syllables were the sum of their letters; words the sum of their syllables; sentences the sum of their words. The findings of both tachistoscopic and eye-movement research have demonstrated the falsity of this concept. It has been shown that even large words can be recognized as wholes and that with improved methods of teaching the span of recognition in silent reading can be two or more words. In carrying on the process the reader may not even be aware of words as such but only of their fusion into a continuous fabric of meaning.

An interesting example of an early awareness of a superior method of reading is afforded in Horace Mann's *Second Annual Report* as Secretary of the Board of Education of Massachusetts in 1838. Mann wrote as follows:

> When a motive to learn exists, the first practical question respects the order in which letters and words are to be taught; i.e., whether letters, taken separately, as in the alphabet, shall be taught before words, or whether monosyllabic and familiar words shall be taught before letters. In those who learnt, and have since taught, in the former mode, and have never heard of any other, this suggestion may excite surprise. The mode of teaching words first, however, is not mere theory; nor is it new. It has now been practised for some time in the primary schools in the city of Boston,—in which there are four or five thousand children,—and it is found to succeed better than the old mode.

A later and highly important example of the effects of perceptual research on methods of learning is afforded by the change from oral to silent reading practices in elementary schools. Although this silent reading reform did not reach its climax in American schools until the decade from 1915 to 1925, it was foreshadowed in the report of Horace Mann above-mentioned. On this issue Mann wrote:

> I have devoted especial pains to learn, with some degree of numerical accuracy, how far the reading, in our schools, is an exercise of the mind in thinking and feeling, and how far it is a barren action of the organs of speech upon the atmosphere. My information is derived, principally, from the written statements of the school committees of the respective towns,—gentlemen who are certainly exempt from all temptation to disparage the schools they superintend. The result is, that more than eleven-twelfths of all the children in the reading-classes, in our schools, do

not understand the meaning of the words they read; that they do not master the sense of the reading-lessons, and that the ideas and feelings intended by the author to be conveyed to, and excited in, the reader's mind, still rest in the author's intention, never having yet reached the place of their destination.

It is easy to demonstrate that the perception of words may be followed by a series of oral responses without any attention to the meaning of what is read. Even today schools may still be found that cling to a method of oral reading that is frequently nothing more than a calling of words. However, such practices are now the exception rather than the rule. The scientific basis for this change in methodology is found in the early studies of eye movements by Judd, Dearborn, and C. T. Gray. These studies showed that the prevailing pattern of perception in oral reading consists of one or more fixations for each word, while for silent reading the span of recognition was generally a group of two or more words. The superiority of the silent-reading process on the basis of perceptual factors was unassailable. The retarding effect of vocalization was clear. It is difficult to find examples of research in psychology that have had as widespread effects on methods of learning as have the studies of visual perception in oral and silent reading. The evidence was so impressive that, at the present time, the major teaching of reading in the elementary school employs far more silent reading than was the case during the earlier period, and in most elementary schools the proportion of reading experience now is much greater for silent reading than for oral reading. While this silent-reading reform appears on the surface to be far-reaching in effect, a closer analysis of the outcomes of the movement indicates that the change was more superficial than many of its advocates presume. It was superficial in the sense that silent reading may be carried on, and in many cases is carried on, by exactly the same psychological process as oral reading, except for the fact that there is no oral vocalization of the words. There is still for many readers subvocalization, or, as it is commonly called, "inner speech," accompanying reading.

Although, in principle, the schools have quite generally accepted the idea that the basic process in reading is a direct association between the words as perceived and their fused meaning, there are still many schools that have not sensed the full import of this relationship. There is still a considerable degree of intermixture of oral and silent reading in the primary grades. As a result, what often passes as silent reading is only noiseless reading. Psychologically, the oral process still persists, only instead of pronouncing the words aloud the child whispers them to himself, as evidenced by lip movements. The process of subvocalization may be so far suppressed that no lip movements or throat movements are observable, but the reader still is conscious of reading word by word instead of being conscious of the meaning only. Completely silent reading, or "nonoral" reading, as it is sometimes called, is carried on without subvocalization or consciousness of words as words. It is similar to the auditory perception of speech. Few persons listening to a lecture follow the speaker by subvocalizing after him the words he speaks. If anyone doubts this, let him try to subvocalize after a speaker for a moment or so; he will soon be aware that auditory perception generally carries on by direct association between stimuli received by the ear and recognition of meaning without the accompaniment of subvocalization. Reading, at the most effective level, is a similar process.

In 1935 the public schools of Chicago developed a method of teaching such nonoral reading for use in Grades I and II. In the ensuing ten years more than 70,000 pupils in 137 different elementary schools in that city learned to read by this method. In 1945 I made a survey of the results of this

method. Four hundred sixty-five pairs of pupils at the sixth-grade level were studied, one member of each pair having been taught in Grades I and II by the nonoral method, and the other by the conventional mixture of oral and silent reading. The pairs were closely matched by age, school grade, and I.Q. Pupils were compared on the basis of scores on standardized reading tests, absence of lip movement, and acceleration or retardation. Comparisons were also made for bright and dull children separately. The published results of this investigation indicated a superiority for the nonoral method in every one of the eight criteria used in the study. The nonoral method did not eliminate lip movement in all cases, as by theory it should have done, but its superiority over the customary method was unquestionable.

These changes in methodology should now be related to the studies of perception with which this discussion began. The situation may be summed up as follows: The many published studies of oral and silent reading agree that in the process of silent reading the span of recognition, as indicated by the average number of words per fixation, is markedly wider than in the case of oral reading. Likewise, the duration of fixations is notably shorter. The rate of oral reading averages approximately 175 words per minute at the adult level, whereas the rate of silent reading for similarly selected persons and materials will average approximately 300 words per minute. The difference in rate is due primarily to the slowing effect of the articulation process. Persons who subvocalize in silent reading have a much slower rate than those who suppress all tendencies to deal with words separately. Where subvocalization is suppressed, rate of reading is a function of rate of comprehension and rate of perception. Since the only purpose of reading is to comprehend, rate should be determined primarily by ability to comprehend. Rate of comprehension should not be restricted by inadequate perceptual processes. Improvement in the perceptual aspects of reading is still the best chance to contribute to efficiency in reading. This cannot be realized without freedom from the restrictions of subvocalization.

The position stated in the preceding paragraph is not the result of so-called armchair reflection. As director of the adult reading clinic of The University of Chicago, for the past five years I have listened week after week to otherwise competent adults complain that their rate of reading was so slow that they were unable to keep pace with the amount of reading they should do. Half of these adults were graduate and professional students. In a striking number of cases tests showed that their rate of silent reading was very close to their rate of oral reading. Eye-movement records for these persons showed for their silent reading all the characteristics of oral reading—fixations on practically every word and longer than average duration of fixations. The fact was that they were subvocalizers—the victims of a method of teaching reading that fixed oral-reading habits first and so strongly that the later silent reading was only noiseless reading, showing none of the characteristics common to effective silent reading. In the reading clinic, using techniques that gradually forced a rate of silent reading beyond that at which articulating is possible, most, but not all, of them succeeded in setting up and fixing a perceptual pattern commensurate with a silent reading process. In the course of twenty one-hour corrective exercises, 234 subjects made an average gain of 54 percent in rate without loss of comprehension. This gain in rate was accompanied by an increase of 29 percent in the number of words per fixation and an increase of 13 percent in speed of fixation time. These changes were primarily perceptual. For most of the cases comprehension was satisfactory at the start. Their problem was rate of reading, and the difficulties were in perception rather than in comprehending what was read.

The customary average rate of reading nonfiction at the end of the program of instruction in the schools is 300 words per minute. Rates running down as low as 150 words per minute are not uncommon both in high school and in college, and the variation is roughly in proportion to the amount of subvocalization present. Above the average of 300 words per minute, cases will be found which, in reading nonfiction with comprehension (not scanning), reach rates of 600 to 800 words per minute. As has been stated, comprehension should be the determiner of rate. In fields where one is familiar, and with due consideration for flexibility in terms of purpose and content, there is little reason for continuing to accept as satisfactory a rate of reading that is a product of a considerable degree of subvocalization and of perceptual habits that are much inferior to those known to be attainable. If schools can be persuaded to accept the fact that, in essence, reading is a process of direct association between perceptual stimulus and meaning without any intervening subvocalization, and, if they can get the full import of the statement that reading is a process of thinking the meaning rather than saying the words, students in perceptual research can explore the limits of efficiency in perception and their relation to the controlling factor of comprehension.

AT THE AIRPORT

Innumerable swirling saws, cutting at the air,
The great level cement walks, rimmed by lights—
I walk under the dream of all these planned spaces,
And remember Leonardo da Vinci, living in the streaming wind.
I see the brooding eyes of the old man, the orbs peering at centuries;
And these wings that a man dreamed are monuments to him.
At the airport, the motors take over—and the winds of the future.

DANIEL SMYTHE

Book Reviews

PREHISTORY

Dating the Past. An Introduction to Geochronology. Frederick E. Zeuner. xviii +444 pp. Illus. 30*s*. Methuen. London. 1946.

THIS book by the distinguished Professor of Geochronology and Environmental Archeology in the University of London is a pioneer in a relatively young field. It covers the whole field of geochronology but emphasizes especially the chronology and climates of the Pleistocene (Ice Age), on account of their practical importance in prehistoric archeology and human paleontology.

Part I deals with dendrochronology, or tree-ring analysis, developed by Douglass, Huntington, Antevs, and others, and its use in dating early history and late prehistory of man.

Part II describes varve analysis, the varve chronology, pollen analysis, and the correlations of the time scales and climatic records shown by varves and pollen profiles with the record of raised beaches. The late glacial and postglacial chronology so obtained is then applied to the important prehistoric sites of the late Old Stone Age, Middle and New Stone Ages, and the Metal Ages. It carries the dating back to about 15,000 years ago.

Part III is devoted to the chronology of the Ice Age and Paleolithic man. Using radiation curves calculated by Milankovitch, the author develops an absolute chronology as follows: Duration of the entire Pleistocene, 600,000 years; double climax of early glaciation, 590,000 and 550,000 years ago; of the second ("Antepenultimate") glaciation, also double, 476,000 and 435,000 years ago; of the third ("Penultimate") glaciation, likewise double, 230,000 and 187,000 years ago; and the triple climax of the last glaciation, 115,000, 72,000, and 25,000 years ago. The interglacial intervals are found to be 60,000, 190,000, and 60,000 years long, respectively.

This chronology is then applied to the various Paleolithic sites of central and eastern Europe, Siberia, northern France, Portugal, the Channel Islands, and the British Isles.

The Pleistocene pluvial climates of the Mediterranean region are correlated with the glacial stages elsewhere, and tentative dates are assigned to Paleolithic sites in that area also. The author then reviews the dating of past climates, early man, and industries in Africa, Asia, Australia, and America.

Finally, a table of approximate dates is given for each of the several culture or industry stages of prehistoric man and for the fossils of man himself. This chronology of early man and his cultures forms the core of the book. As it falls within the main interest and competence of the author, it represents his main contribution.

Part IV deals with a variety of topics—the measurement of geological time, the age of the earth and the universe, the time-rate of geological processes, the time factor in biological evolution, the time-rate of development of species, and related chronological subjects. These extend the geochronology to about 2,000 million years.

The work is concluded with a 33-page bibliography (arranged by chapters), a 22-page index, and a section of plates.

The book, replete with chronological tables, both relative and absolute, will appeal mainly to workers in archeology, anthropology, and Pleistocene stratigraphy, and less directly to those in climatology, geography, geology, paleontology, and biology.

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DIFFICULT PROBLEM

Drought, Its Causes and Effects. Ivan Ray Tannehill. xii + 264 pp. $3.00. Princeton Univ. Press. Princeton. 1947.

IN *Drought, Its Causes and Effects* the author, who is Chief of the U. S. Weather Bureau's Division of Synoptic Reports and Forecasts, has skillfully handled an exceedingly complex subject in a manner that will appeal to both laymen and scientists. The reader is made to feel that he is standing beside the author, so to speak, when weather records, sunspots, tree rings, maps, and charts are being examined for clues to the drought problem.

In the closing paragraph of his introductory note the author says:

> In this book we shall proceed with an open mind. First we shall look at the nature of the problem and see what drought, with its associated crop failures, famines and dust storms, means to the people of the world; and we shall review briefly the history of droughts in the United States. Next we shall look at the facts in the case from the standpoint of weather. We shall review the rainfall records and related weather data and look for a clue. Third, we shall try to put together the pieces of this great puzzle and see if we have a satisfactory explanation and any hope of predicting drought in the future.

Between this introductory note and the conclusion we find several chapters of delightful and informative reading. The chapter on Famine, showing how several nations on earth are frequently visited by starvation because of insufficient rainfall, emphasizes the need for continuous study about drought and its cause. Readers will probably vary in their choice of the intervening chapters. Some will be impressed with the discussion of sunspots and their relation to weather, and others will be more interested in the chapter dealing with the Pacific as a weather barometer.

The twentieth and last chapter is a summary, and the author says in part:

> In conclusion, the writer regrets that it is necessary to say that the problem of drought is not completely solved. A great deal of work remains to be done. The changes of the future may bring new phases of the problem which will have to be studied.
>
> The pattern of world rainfall is vital to huma n civilization. Time has scrawled a bold but cryptic message, alternately dry and moist, on the rocks, on the fossils of flora and fauna of the ages, and finally on the gravestones of civilizations. The scrawl continues, and at last our thermometers, rain gages, barometers, rawinsondes, and other instruments are beginning to decipher the story. The record shows unmistakable evidence of the interactions of atmosphere, oceans, and continents, under the basic control of the sun.
>
> In the future, farmers will not have to gaze despairingly into a clear sky, wondering if a few clear days will continue into a disastrous drought. Even if we are never able to control the climate, much will be gained by knowing what to expect. Droughts are not mere chance occurrences; they are part of a physical process which can be measured and studied and predicted with increasing precision as our observations of the sun and the upper air and the oceans continue to accumulate.

From a critical point of view it seems to me that hopes are aroused in the earlier chapters that are not fulfilled in the book. Then, too, I sensed the implication that farmers cannot do much about droughts, that they must suffer when it comes or call for governmental relief until the rains return. We know that during the drought period of the thirties many farmers in the worst of the old dust bowl area were able to keep cover on their land. They didn't

make much money, to be sure, but they kept their land intact because their cropping system was adapted to the hazards of the climate. We learned also that some farmers, through skillful management of the water supply, grew crops when their less provident neighbors failed on adjoining lands.

What the author missed is the fact that under effective conservation measures, particularly contouring, much or all of the rainfall reaching the land is stored in the reservoir of the soil. This not only aids with conservation of the soil by increasing the growth of soil-holding vegetation, but it is the equivalent of changing the climate insofar as the life of plants is concerned, especially in the Great Plains.

To date conservation measures have been applied to something over ten million acres of land in the former dust bowl. If a series of dry years returns, the Great Plains are in far better condition to withstand the damaging effects of drought than they were in the thirties.

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MONSTROUS DRAMA

The Last Trek of the Indians. Grant Foreman. 382 pp. Maps. $4.00. University of Chicago Press. Chicago. 1946.

FOR the scientist layman living in the East, Middle West, or Oklahoma who wants to learn what happened to the Indians of his state and how Oklahoma came to harbor remnants of some fifty-five tribes, of whom but six were indigenous, this books holds the answer. It completes a study of Indian removal, which Grant Foreman began by describing how the southeastern Indians were displaced, and accounts for what happened to the broken fragments of tribes who were living north of the Ohio River when James Monroe became President in 1817.

Except for the New York Iroquois, who wisely resisted resettlement, the typical northern tribes were the Delaware and Shawnee, who were so used to being pushed around that they retained only a semblance of old tribal organization and a tradition of an eastern homeland. They were the victims of intertribal and colonial wars and the Revolution; they had abandoned gardens and village sites, often twice in a generation, and removed, generally westward, before the advancing white man "... to find better hunting or maple sugar groves or huckleberry country; to flee from epidemics; or to follow the fortunes of a rival chief or medicine man...." They broke into factions and scattered in many bands to be thrown with strange bedfellows like the Wyandots and Senecas of Sandusky, both of Iroquoian stock.

At the turn of the eighteenth century the young government undertook to remove the Indian residents of Ohio, Indiana, Illinois, and New York—Delaware, Shawnee, Ottawa, Wyandot, Kickapoo, Potawatomi, Peoria, Kaskaskia, Piankashaw, Sauk and Fox, Cayuga, and Seneca—without plan or consistent policy, predetermined method, or personnel familiar with the history and ethnology of the peoples involved. Ethnology was not yet even in its infancy, and a century and a half were to pass before its findings were applied. Instead, the pressure of political expediency ruled the day, the corruptibility and cupidity of Indian chiefs were played upon through bribes and the persuasive properties of whisky, and with flagrant disregard of human rights the agents of land companies stooped to murder and arson to secure land cessions for a hungry frontier. The documents that Foreman has assembled to support his brief provide a sad commentary on the beginnings of "free enterprise" in the Middle West.

Yet the picture is not clear-cut since history has hopelessly complicated the situation for the scholar; Foreman has been prevented by the sheer mass of evidence and its diversity from clarifying the material for the reader.

Reports of His Majesty's Indian superintendents in America, the similar manuscript reports of the Department of Indian Affairs (at first a branch of the War Department, afterward of Interior), and the correspondence of Army officers, Indian agents, missionaries, and schoolmen constitute the literature on Indian removal and now repose in the National Archives and in the collections of various libraries and historical societies. Besides these sources, Foreman has used the printed reports of the Commissioner of Indian Affairs (1824–1907), Congressional documents, bills, and hearings, the U.S. Statutes at Large, and the American State Papers. But, aside from the *Handbook of American Indians*, ethnological reports on the tribes removed were overlooked.

The fore part of the book is concerned with removal; the latter and major part with what happened when the Indian tribes reached Kansas, only to be uprooted and resettled in Indian Territory, now Oklahoma. The reviewer has followed each group on its long trek, and he has no reason to question essential facts or treatment of sources. It is a good job of history, but there are so many treaties, negotiations, and removals that each new group treated involves repetition that grows wearisome despite the intrinsic interest of the material and, like the atrocity tales, dulls the sensitivities. No clear design emerges in the treatment, and a more general summary would have helped the average reader. As it stands it is a scholar's book and a fine contribution.

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HISTORY OF ANESTHESIA

Victory over Pain. Victor Robinson. xiv + 338 pp. $3.50. Henry Schuman. New York. 1946.

THIS is a scholarly account of man's search for suitable anesthetic agents. Its publication coincides with the one hundredth anniversary of the first successful public demonstration of ether anesthesia. We tend to forget the magnitude of the miracle of modern anesthesia and the momentous changes in surgery and obstetrics that it has permitted. Even the anesthetic properties of so simple a substance as ice were not recognized until the past decade. How many agonized screams have echoed through the years because surgeons did not know of this wonderful power of frozen water! It is interesting to pause occasionally to take stock of what we have done and perhaps to wonder why scientific progress has been so painfully slow in many great periods of the past.

Each of the many types of anesthetics available today has a turbulent history associated with its development. The inevitable clash of personalities seeking personal glory and the social and religious taboos which defy change are colorfully portrayed by Professor Robinson in this absorbing volume.

Before the advent of modern anesthesia, surgical procedures were rarely employed. They were as much dreaded by the surgeon as by the patient. Surgery was a desperate measure employed only when the alternative appeared to be the death of the patient. The skill of a surgeon was measured in minutes rather than by his ability to perform the operation properly, and the patient always arranged his affairs beforehand as if his last hour were approaching.

The science of anesthesia as we know it today is the product of work done principally in the last century. More progress

has been made during the past one hundred years than in all prior history. Each period discussed in the book is made vivid with anecdotes and extracts from pertinent letters of the time which are of interest to the layman as well as the physician. The story starts with the early Greeks, who were never successful in doing more than dulling the sensibilities minimally with plant products, and the Chinese, who undoubtedly made contributions which have become lost. The arrangement is chronological, and each chapter deals with a new period.

In times such as these it is refreshing to contemplate a science which was crystalized from a mixture of investigative genius, personal feuds, jealousy, ignorance, and prejudice to become one of the major contributions that man has made to his own welfare.

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NO MORE FRONTIERS

Radio's Conquest of Space. Donald McNicol. x+374 pp. Illus. $4.00. Murray Hill Books. New York. 1946.

DRAWING from a lifetime of intimate association with the pioneers of the wireless industry, Donald McNicol has in this book presented the history of radio achievement in a manner most readers will enjoy. The book is nonmathematical, and it will be found easily readable.

Mr. McNicol explains clearly and concisely for beginners the operation of each device. This detailed explanation regarding the functions and particular advantages of new apparatus over its predecessors enables the reader to appreciate better the contributions of each inventor. The author goes to unusual lengths to maintain historical accuracy, even at the expense of repetition. Of particular interest to the younger generation of radio men are the youthful endeavors of men who later became heads of large manufacturing firms bearing their names.

To those who have spent many years in the radio profession this book will have a strong appeal because of the down-to-earth presentation of the struggles of men of genius, without any attempt to dramatize their achievements. The veteran engineer will be delighted as McNicol recalls the operational procedures and the equipment used in the horse-and-buggy days of wireless. Newspaper notices, personal correspondence, and extracts from professional journals are of special interest to men who will remember having read these articles when first published.

In arranging topics for discussion, the author has eliminated confusion by devoting each chapter to the chronological development of a single device. Other qualities which are unusual and commendable are the little asides which the author makes regarding politics, commercial programs, and mythology. Philosophical observations, the inalienable right of all mature authors, add spice to this interesting narrative. The competence of the author to handle his subject is eloquently revealed by the intimate personal anecdotes which he relates about his friends who were the makers of radio history. At times, however, McNicol assumes that his readers will all have the same familiarity with the lives of the radio great that he enjoys; so that in some cases the curiosity of the reader is only whetted and not satisfied. Possibly this is a virtue and not a fault.

The physical make-up of the book is average. The latter part of the book is well illustrated, but there are not enough pictorial or schematic diagrams to illustrate unfamiliar obsolete equipment.

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ROGUE RIVER

The Colorado. Frank Waters. xvi + 400 pp. Illus. $3.00. Rinehart. New York and Toronto. 1946.

Listen, Bright Angel. Edwin Corle. viii + 312 pp. $3.75. Duell, Sloan and Pearce. New York. 1946.

ABOUT any river there is an inexhaustibility that defies all learning. But what are we to do with such a river as the Colorado and its Grand Canyon, so mighty in power and splendor as to shrivel the imagination? A thousand books could be written about this river, and every one might be a good book, yet every one would fail to do full justice to it. For the Colorado is an eternal paradox. It is a place where angels and devils meet. It is the most revealing natural phenomenon in America, yet the most incomprehensible.

Frank Waters and Edwin Corle are two Southwesterners who independently and almost simultaneously decided they knew the Colorado River well enough to write about it. Although they produced somewhat similar books, which occasionally overlap, they are different enough to make it profitable to read both. They are not, of course, strictly "scientific" books, for the authors are not scientists or technologists—they are writers*—but if the science in them were taken out, not much would be left. Some of the sciences that the reader will have the pleasure of meeting, though superficially, are:

Geology. The whole Colorado River country is a geologist's paradise. All thinking about the Grand Canyon must begin with the question: How long has this been going on? Geologists are still studying the age of the river, with perennial answers to the question. (See Professor Chester R. Longwell's article, "How Old Is the Colorado River?" in the December 1946 issue, *American Journal of Science*, for the latest summary of the problem.) Like the Colorado itself, geology runs deep through both of these books, clarifying our understanding and enhancing our appreciation.

Anthropology. The people of the Colorado country are inseparable parts of both books, but see especially Corle's Section V, titled Land of the Sky Blue Water, which includes a fascinating, humorous, and firsthand account of the Havasupai Indians, who have lived since God knows when in their labyrinthian canyon home along Havasu Creek, now within Grand Canyon National Park, Ariz. In this country, too, flourished the ancient cliff dwellers and the great pueblo peoples, and the story of the Colorado is their story also.

Engineering. Man naively thinks that he has conquered the Colorado with Hoover Dam, in the colossal building of which 110 men lost their lives but which reclaims 2,000,000 acres of desert and illumines and empowers the city of Los Angeles and her numerous satellites. But wait a few years and see, say Messrs. Corle and Waters. It may be that the river will have the last say, thinks Waters. Says he:

> Capable of impounding the river for two years, Boulder Dam also retards its movement of silt. The water, settled and clear, is released; the silt remains. Within 50 years Lake Mead will be filled with water; in 300 years the whole vast reservoir will be filled in solid with sand and silt.

* In biography Waters has written *Midas of the Rockies*, the story of Winfield Scott Stratton and the history of the Colorado gold discoveries; he is also the author of, among other books, two historical-ethnological novels, *The People of the Valley* and *The Man Who Killed the Deer*, the latter descriptive of Pueblo life and ceremonialism. Corle also is a novelist and has written two of the best American Indian stories I have ever read: *Fig Tree John* (Apachè) and *People on the Earth* (Navaho). Among his other works is a regional book on the Southwest, *Desert Country*. All of them deserve a wide reading.

Another dam, probably in Bridge Canyon, must be built farther upriver. Then still another until all possible 11 sites will have been utilized and exhausted. Then again the Colorado will resume its way. Which one of us dares assume that one transient race of men ... can do more than retard for a geologic moment the river's immemorial and immeasurable task of transporting bodily the whole vast Colorado Pyramid into the sea?

A nice question, and Waters takes the long view and hopes he won't be anywhere around when the dam goes out.

Navigation. The attempts to navigate the Colorado, from either the north or the south, have been shining examples of man's egotism. There have been a few successes and many failures. These books cover this subject to some extent, especially Lieutenant Joseph C. Ives's famous *Explorer* expedition of 1858, but one may go to another recent book, Jerry MacMullen's *Paddle-wheel Days in California* (1944), for a fuller account of Colorado River navigation.

Agriculture and irrigation. Waters' story of the opening up of the Imperial Valley to agriculture, involving some pretty tricky and costly plumbing paid for by private individuals, corporations, and government, forms an exciting chapter.

Geography and exploration. The explorations of Major John Wesley Powell, naturalist, geologist, ethnologist, and the first man to travel through the Grand Canyon of the Colorado and live to tell the tale, are features of both books, especially Corle's. Powell was an important as well as a colorful figure in American science and deserves a full-length biography by a competent historian. Later explorers of the river —scientists and adventurers alike— all figure in these chronicles.

Biology. This subject is scantily treated, but Corle summarizes the wildlife and forest resources, especially of the Kaibab country on the north rim of the Canyon.

One could go on. There are portions of both books, too, that are purely historical in nature, retelling the story of the early Spanish travelers—conquerors and padres— who first found the Colorado country and massacred or missionized the Indians, leaving their ineradicable marks. One such mark is the name of the river itself—Río Colorado. In both volumes also there is a personal quality that makes them pleasant to read; for both authors have so thoroughly identified themselves with the Southwest that their own experiences become a part of the chronicle and give a lively, authentic, and individual ring to what might otherwise be a dull documentary discourse. *The Colorado* is the twenty-ninth in the now well-known Rivers of America series. The book is illustrated with drawings by the Russian-born artist Nicolai Fechin, and Waters sets great store by these pictures; unfortunately, however, they are not reproduced well enough to render their full values, being printed from coarse-screen half tones on regular text paper. Included also is one rather muddy oil sketch of an Indian ceremonial reproduced in color. *Listen, Bright Angel* is unillustrated except for a map or two.

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BIRD NOTE

Audubon Bird Guide: Eastern Land Birds. Richard H. Pough. xxxvi+312 pp. Illus. $3.00. Doubleday. Garden City, N. Y. 1946.

IN THIS guide the 275 species of land birds that occur in the area of eastern North America are each described—males, females, and sometimes juveniles. Concise paragraphs treat the identification, habits, nests, voices, and distribution of each bird.

The author discusses briefly and generally psychology and behavior, ecology, migration, and economic relations. There are observations on conservation, the effects of

insecticides, and even information on the selection and use of binoculars for bird studies.

The detailed figure of the topography of a bird will be helpful to amateurs. The 48 excellent color plates have over 400 illustrations that show not only the male and female plumage and sometimes the juvenile, but also seasonal variations, and are of great value in identifying the species.

There is a bibliography and a most excellent index, and the book is bound in a flexible cover. Out of five of the reviewer's friends who glanced at the book as it lay on his desk, four used the word "concise," and two went out to buy copies.

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HANDBOOK

Household Physics. Madalyn Avery. Rev. ed. xi+470 pp. Illus. $4.50. Macmillan. New York. 1946.

THE primary function of any elementary textbook is to set forth basic principles. When the text is an "applied physics" written for use in a college and for a particular group of students, the examples and applications should be chosen from the field of the students' experiences or so as to give information of use when the student at some later time encounters any of the phenomena involved, either in a course or in some life experience. This principle, in general, is well adhered to throughout this text.

In addition to the presentation of material from which household applications can be drawn, most of the rest of the entire subject matter of an elementary college physics text has been included, the revision rearranging the subject matter into the divisions and order most often found in a college text. Apparently because power electrification is reaching rapidly and extensively into rural areas, the labored discussion of individual farm electric plants has been eliminated. The extensive discussion of the sewing machine, a favorite piece of equipment upon which to base a household physics text, happily has been deleted.

Mention is made of an apparatus for determining metabolic rate and bodily energy expenditure. An airtight mask fitting over mouth and nose is a constituent part of the apparatus, and yet it is indicated as useful in determining the energy required in eating a meal—an obvious impossibility.

It seems evident the text is written to serve two purposes: to serve as a basis for a physics course, and to give instruction in household equipment. Appearing in various places in the book are directions as to what to look for in selecting equipment, how it should be constructed, and instructions as to how to take care of it. Many of these items are outside the pale of a physics course.

Although the revision is recent, the material on household equipment is not up to date. The better ice refrigerators are not constructed as shown in the illustrations. Electric range units, about which much discussion is centered, do not include the more recent ones with "infinitely-variable" heat. Only a brief discussion of the home freezer is given when the present apparent interest in the subject seems to merit a more extensive treatment.

It is in the fulfillment of the second purpose that the text is incomplete. It might be argued that the instructor should be able to elaborate upon the principles and applications involved, thereby filling in the gaps caused by omission or contraction; but there are not enough physics instructors who keep abreast of developments in the field of household equipment to make such a makeshift course substitution a success.

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Comments and Criticisms

POLLEN COUNTS

Would it be out of order to call attention to a technical error in one of the honorable mention articles under "The AAAS-George Westinghouse Science Writing Awards for 1946" (SM, January 1947)? I quote from the third paragraph on page 81: "After exposure the slide is treated with a red dye which is absorbed only by the ragweed pollen." The author evidently refers to Calberla's solution with fuchsin which is often used for staining atmospheric pollen samples. While this stain penetrates the exine of pollen grains more readily than most other plant tissues, it is *not selective for ragweed pollen.* The author probably intended to bring out the point that stained pollen grains are readily differentiated from other small particles on the slide such as sand and soot.

One could also find considerable fault with the second and third sentences of this same paragraph where the author slips so lightly over the matter of counting the pollen on a gravity slide and interpolating the results in volumetric terms.

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ESKIMO INFANTICIDE AND POLYANDRY

There is a story of the man who declared that all Indians walk single file. When asked what evidence he had for this, he replied, "The one I saw did."

Professor Garber in his interesting article on "Eskimo Infanticide" (February 1947 SM) derives Eskimo polyandry from a shortage of women caused by female infanticide. He cites an instance in which the St. Lawrence Island natives sent him word that they needed a large number of marriageable girls. This is not evidence of an actual demographic unbalance of the sex ratio. Polygyny is so common among Eskimos that some men are commonly left without wives. Violent sexual competition among the males is so strong that it is quite possible that an Eskimo group would be only too happy to have the government supply them with more women if that could be done.

Population statistics indicate that Professor Garber is quite wrong in his assertion that female infanticide results in a shortage of women. On the contrary, occupational hazards for males are so great that in most Eskimo communities there is a surplus of women over men in spite of the fact that a large percentage of the girl infants are killed.

In 1924 Edward Weyer, in *The Eskimos*, (Yale Univ. Press, 134–135), brought together data from a number of Eskimo groups outside East and South Greenland (where infanticide is rare). The ratio of girls to boys under ten years of age ranged from forty-two to ninety-two girls to each one hundred boys. Among adults over fifteen years of age the proportions were reversed. Out of twenty groups on which data were available only three had fewer women than men. In more than half of these groups there were more than one hundred and ten women to one hundred men.

These wider facts indicate that Eskimo polyandry cannot be due to a shortage of women as caused by infanticide. The shortage is caused by the sociological fact of widespread polygyny. Strong men monopolize the available women and weak men are left out. This, combined with a strong principle of the sexual equivalence of brothers, results in fraternal polyandry.

Anthropologists have learned to be extremely wary of simple biological explanations of cultural phenomena. While not rejecting the possibilities of biological influences on human behavior, we find that it is not safe to overlook cultural factors when explaining cultural phenomena.

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APPRECIATION

I'd like to comment on what I think has been a definite improvement in the character of the articles included in the last dozen or so issues of the MONTHLY. In the current number "On Life as a Separate Entity," by Thomson King, is especially fine, and I have taken the liberty of writing directly to Mr. King complimenting him on it. I think the MONTHLY is serving a very definite purpose for those of us who are engaged in working with the industrial applications of science. To open and read it each time is like opening and passing through a broad, high gate yielding upon ever-widening vistas of knowledge. To me it is an indispensable experience.

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The Brownstone Tower

As A layman in the social sciences, I should like to discuss the current situation in that field as it looks to me from my bay-window substitute for the Brownstone Tower. Social scientists are in an uncomfortable position. In the present precarious state of the world they know that their subjects should be more important for the future of the human race than any of the natural sciences. But they cannot yet convince natural scientists, or the public, or even themselves that they *are* scientists and can use the scientific method on human problems in an unbiased and potentially fruitful manner. They seem to be frustrated in their public relations, and their offerings to the SM are characteristically argumentative, insisting, with theoretical reasonableness, that since man is a part of nature, students of human society should be regarded as natural scientists. There is no question that they have performed in the manner of natural scientists in their studies of cultures of the past and those of contemporary backward peoples, but it remains to be proved, at least in the SM, that they have demonstrated their capacity to act as natural scientists in studies of current problems of so-called civilized societies.

While social scientists are trying desperately to secure public recognition as natural scientists and to gain public confidence, they are continually being embarrassed by encroachments into their own fields by natural scientists, clergymen, philosophers, politicians, and, in fact, any literate persons who think that the world is out of joint and offer remedies that some editor will accept. For example, in the May issue of the SM appeared a review of a book by Roger J. Williams, a well-known chemist, on "humanics;" and in the same issue was reviewed a book by Lecomte du Noüy, an equally well-known biophysicist, on human destiny. No social scientist would write a book on a new system of organic chemistry; why, he must wonder fretfully, would a chemist write a book on social engineering? The answer, I think, is that everyone in a democracy is expected to have opinions on social questions. Lay opinions of prominent, intelligent citizens will continue to be published and read until social scientists prove by their works and the results of their works that they are as uniquely competent in their fields as organic chemists are in theirs.

I do not intend, as yet, to limit articles on social-scientific questions in the SM to those written by social scientists, but I do want to encourage them to write for the SM. I want to publish articles by them that will bear evidence of true research and that can thereby be distinguished from inspirational and philosophical writing backed by no conscious research. I want to help them both by getting better articles on social science and by excluding unworthy articles. To that end I have welcomed and accepted an offer of cooperation from the American Sociological Society. Dr. Louis Wirth, President of that society, has appointed the following special committee to advise me: Alfred McClung Lee, Wayne University, Chairman; Calvert L. Dedrick and Abraham Jaffe, Washington, D. C.

Finally, I am in favor of including a Division of Social Science in the proposed National Science Foundation. If social scientists can demonstrate that they are natural scientists, they can do so more effectively working with physical and biological scientists than apart from them.

F. L. CAMPBELL

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